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REPORT TO THE PRESIDENT BY THE PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT-VOLUME IV

Washington, D.C.

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Report to the President

Volume IV

By The

PRESIDENTIAL COMMISSION
on the Space Shuttle Challenger Accident

Washington, D.C

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PRESIDENTIAL COMMISSION ON SPACE SHUTTLE CHALLENGER ACCIDENT—THURSDAY, FEBRUARY 6, 1986

National Academy of Sciences
Auditorium
2100 Constitution Avenue, N.W.
Washington, D.C.

The Presidential Commission met, pursuant to Presidential Executive Order, at 9:50 o'clock a.m.

PRESENT:
WILLIAM P. ROGERS, Chairman
NEIL A. ARMSTRONG
DR. SALLY RIDE
DR. ALBERT WHEELON
ROBERT RUMMEL
DR. ARTHUR WALKER
RICHARD FEYNMAN
ROBERT HOTZ
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA

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PROCEEDINGS

CHAIRMAN ROGERS: Ladies and gentlemen, I now would like to call this first meeting of the Presidential Commission on the Space Shuttle Challenger Accident to order.

I want to make just a couple of preliminary remarks. As you know, this Commission was appointed by the President on Monday, and because of the time frame within which we are working, we wanted to start as expeditiously as possible, and the members of the Commission have been very accommodating and agreed to come to Washington yesterday.

We had a preliminary get-together to discuss our plans and where we were to go based upon the Executive Order, and we have, with the cooperation of NASA and the White House and other officials, been able to set up this meeting for this morning. The purpose of the meeting this morning is to be brought up to date on the events that have occurred since the accident, principally by officials from NASA. They have been very cooperative and have been working closely with us, and we are obviously going to rely in large part on the investigations that they have conducted and will conduct in the future.

On the other hand, as we said when the

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President announced the appointment of the Commission, we have our own responsibilities. We can seek other evidence, get any other information we may desire, and the NASA officials have been, as I say, very cooperative in that respect.

I would like to, by way of a beginning, refer to the Executive Order that created the Commission because we want to stick very closely to the instructions that we received from the President, and I will just read briefly the important part of that Executive Order.

It says "The Commission shall investigate the accident of the Space Shuttle Challenger which occurred on January 28, 1986, and the Commission shall:

"(1) Review the circumstances surrounding the accident to establish the probable cause or causes of the accident; and

"(2) Develop recommendations for corrective or other action based upon the Commission's findings and determinations.

"The Commission shall submit its final report to the President and to the Administrator of the National Aeronautics and Space Administration within 120 days of the date of this Order."

So our first task, it seems to me, and I think

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other members of the Commission, is to deal with, one, review the circumstances surrounding the accident to establish the probable cause or causes of the accident.

Now, with that opening statement, keeping in mind that is our purpose this morning, to be brought up to date on the events that have occurred since the accident, we will call on NASA officials, and I guess the first witness is Dr. Graham, if the doctor will proceed to the podium.

Doctor, I will ask the Clerk to swear you in.

THE CLERK: Do you swear the testimony you are about to give before this Commission will be the truth, the whole truth, and nothing but the truth, so help you God?

DR. GRAHAM: I do.

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TESTIMONY OF DR. WILLIAM R. GRAHAM, ACTING ADMINISTRATOR, NATIONAL AERONAUTICAL AND SPACE ADMINISTRATION

DR. GRAHAM: Mr. Chairman, members of the President's Commission on the Space Shuttle Challenger Accident, NASA welcomes your role in considering and reviewing the facts and circumstances surrounding the accident of the Space Shuttle Challenger.

NASA continues to analyze the system design and data and, as we do, you can be certain that NASA will provide you with its complete and total cooperation. Along with the President, I look forward to receiving your report and to the resumption of space flight with our national Space Shuttle System.

I would like to introduce now Mr. Jesse Moore, who is NASA's Associate Administrator for Space Flight and also the Chairman of NASA's 51-L Data Design and Analysis Test Task Force. He will conduct the briefing.

Thank you.

THE CLERK: Do you swear the testimony you will give before this Commission will be the truth, the whole truth, and nothing but the truth, so help you God?

MR. MOORE: I do.

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TESTIMONY OF JESSE W. MOORE, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT, NATIONAL AERONAUTICAL AND SPACE ADMINISTRATION, AND CHAIRMAN, 51-L DATA DESIGN ANALYSIS TEST TASK FORCE

MR. MOORE: Mr. Chairman, members of the Commission, we are here today before you to discuss the Space Shuttle Challenger accident and to talk to you about where we stand today in terms of our analysis that we have done so far as a result of that accident, and supporting me here today are various members of the NASA centers involved, as well as members of the Astronaut Office down at the Johnson Space Center.

I would like to say that we tried, in preparing this document for you, to put it together to give you a sequence of how NASA goes about getting ready for a flight, what some of the background associated with the Space Shuttle System is, and then, finally, tell you where we are with respect to the overall investigation that we are currently working on right now.

We will have to apologize because we probably have some acronyms in our document here that may be kind of difficult. Some of the charts that may come on the television screens may be difficult to read, but we have

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tried to put together the best set of information we could in the time available to do it.

I would like to now proceed with the agenda, please. [Ref. 2/6-1]

I plan to cover the overview, and then I would ask various members involved in the Space Shuttle System to cover respective parts of the Shuttle, and I will start out by asking Arnold Aldrich, who is the Manager of the National Space Transportation Program Office at the Johnson Space Center to talk about the orbiter system as well as to give you some background on the Shuttle and overall performance, and then I will call upon Dr. Judson A. Lovingood of the Marshall Space Flight Center to talk to you about the responsibilities of the systems that the Marshall Shuttle Projects Office have, and then I will ask Robert Sieck of the Kennedy Space Center to talk to you about the launch and landing operations at Kennedy.

I think what is also very important to this group is the design and development process that NASA follows in acquiring hardware and software before we fly it, and we will tell you about how we do that and the overall process, preparations with respect to that aspect.

Finally, we will close with our actual flight

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preparation process: How do we get ready for a flight; who is involved in getting ready for a flight, and to try to give you some background information about the overall flight process involved in the Space Shuttle Program.

The next chart shows an organization chart showing how NASA is organized from the Administrator level down to what we call the field center level, and I won't spend a lot of time

going into great detail on this, but I will tell you that Dr. Graham is the Acting Administrator of NASA. I report directly to Dr. Graham. I am the Associate Administrator for Space Flight. And then reporting to me institutionally are four NASA centers involved in not only the Space Shuttle program but a number of other programs in NASA. The centers are the Lyndon B. Johnson Center in Houston, Texas. They are also the John F. Kennedy Space Center in Florida, the George C. Marshall Space Flight Center in Huntsville, Alabama, and the National Space Technology Labs in Mississippi. [Ref. 2/6-2]

(Viewgraph.) [Ref. 2/6-3]

MR. MOORE: The next chart, please, will show a little bit more detail in terms of how I operate the Office of Space Flight. And in this chart I have four principal positions in my front office: a Deputy

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position; a Deputy Associate Administrator for Technical Matters; and a Deputy Associate Administrator for Management. I have two staff functions, principal staff functions. One is looking at STS program integration, looking and making sure all elements of the program are integrated from a standpoint of program, policy and budget. Then I have a number of what I call line divisions that report to me that have various responsibilities which are listed on the chart, and I will just quickly try to let you have a feeling for what those are.

The box on the far left shows my Customer Services and Business Planning Division. That division principally interacts with the Shuttle customers to give them schedule information and planning information prior to our launches. Then I have a division called the STS, and here STS—you will see that quite a bit—stands for the Space Transportation System, Orbiter Division and Logistics Division. This division is responsible for the overall program aspects and policy aspects of the Shuttle Orbiter System, and the logistics to support the Shuttle Orbiter System, meaning all the hardware and the spares that we need to make sure the Shuttle flies.

CHAIRMAN ROGERS: What does STS stand for

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again?

MR. MOORE: I'm sorry, STS, you will hear that term quite a bit, stands for the Space Transportation System, and that is another way we use of talking about Space Shuttle. It is the Space Transportation System. If you look at the Space Shuttle, you can see the Space Shuttle here, and different people look at it in different ways. And some say the Space Shuttle is the orbiter only, but the Space Transportation System involves more than just the orbiter. It involves the external tank, it involves the solid rocket boosters, and all the people, facilities that we have to support it. And that is kind of what we call in broad terms the Space Transportation System.

CHAIRMAN ROGERS: Thank you.

MR. MOORE: In addition to our Orbiter Division we have a Propulsion Division, and this principally is, from a program standpoint, a budget and policy standpoint, responsible for the propulsive elements on the Shuttle, and those elements include the Shuttle main engines, of which there are three, the external tank which provides the fuel for the main engines on the Shuttle, and then the solid rocket boosters which provides the—a major part of the thrust during the initial ascent phase of the launch.

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And then I have an STS Operations Division. This is responsible for, again, program and policy and budget related to how we operate the Shuttle in our launch operations down at the

Kennedy Space Center as well as in our flight operations activity that is involved and being performed at the Johnson Space Center.

There are other supporting divisions on the right—Resources, Advanced Programs, and Space Flight Development Systems. These are kind of supportive to the overall Space Transportation System, and then each of the centers listed below have various responsibilities.

And I think the next chart will kind of give you a feel for the overall management responsibilities.

(Viewgraph.) [Ref. 2/6-4]

MR. MOORE: You can see the Office of Space Flight kind of looked at from an overall management point of view and not so much from an institutional point of view. My office has responsibility for policy, advocacy of the program, budget and resources, marketing, and kind of ensuring that the overall corporate structure is maintained, and then external relations interfacing with the outside world as far as the overall Shuttle is concerned.

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There is a Program Office at the Johnson Space Center called Level 2, and Arnold Aldrich, whom you will be hearing from in just a minute, is the manager of this overall office. His job is overall program management integration, which means making sure that the system all plays together, that everything is ready from a systems standpoint from an overall performance, that the hardware all matches and so forth. And then there is a customer service function down at the Level 2 office as well to make sure the cargo integration and work in that area is also done appropriately.

Then, reporting to the Level 2 program offices are various project elements at the four NASA centers that I talked to you about, and I will just quickly go through from left to right the various projects and the responsibilities for these projects are the responsibilities of, on the left, the Johnson Space Center has the responsibility for the Shuttle orbiter, for the orbiter crew equipment, meaning all the components and so forth necessary for the flight crew, and also the Astronaut Offices at the Johnson Space Center, for Flight Operations, meaning at liftoff, the flight of the Shuttle, and its orbital operations and its landing operations are basically the responsibility of the Johnson Space Center, and to

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actually do the payload integration, making sure that the hardware we fly in the Shuttle is properly integrated into the cargo bay prior to our launch.

The Kennedy Space Center on the next box has the responsibility for ground support equipment such as all the launch pads and all the launch facilities that are required to support the launch of a Shuttle. They have responsibility for actually launching the Shuttle, the launch operations complex at the Kennedy Center does the actual countdown and so forth prior to a launch. And then they also do the hardware payload processing prior to installing, and they actually install the payload elements into the bay of a Shuttle.

At Marshall Space Flight Center they have the responsibility for the Shuttle main engines, for the external tank, for the solid rocket booster, and for Spacelab, which is a cargo element that flies inside of the Shuttle.

As far as the NSTL—again, NSTL is National Space Technology Laboratories—they basically provide us test facilities for testing the Shuttle main engines.

Next chart.

(Viewgraph.) [Ref. 2/6-5]

MR. MOORE: The next chart I'm just going to quickly let you look at. I don't intend to brief this

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in detail. What I have tried to do in this chart, you will see it discussed later by Mr. Aldrich. What I have tried to do in this chart is to give you a more detailed vertical cut from the previous chart, and on the right of the chart some of the specific functions that are done by this particular structure.

(Viewgraph.) [Ref. 2/6-6]

MR. MOORE: Now, the next several charts will talk about the planned evolution of the Shuttle program, and this is a plan which encompasses the 1981 timeframe through the 1986 timeframe, and I will try to show to you and to your Commission what flights have been done and the kinds of things that have been done during that period of time on the various missions.

There was a phase in the program that initiated in the April 1981 timeframe and ended in late 1982 called the Orbital Flight Test Phase.

(Viewgraph.) [Ref. 2/6-7]

MR. MOORE: During this phase we flew four Shuttle missions, STS missions, and as a part of those flights, we flew instrumented pallets—a pallet is a cargo element that sits inside of the cargo bay—to try to get some feel for how we could accommodate payloads in the Shuttle. We flew the RMS, another acronym—and that stands for the Remote Manipulator System,

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—and that is the Shuttle's arm which we now fly routinely on most flights. We did fly our DOD, or Department of Defense, payload on one of the early flights, and we began doing some experimental flying on pharmaceuticals, doing some early experiments to see how those experiments would react to zero gravity.

Beginning in STS 5, which occurred in late 1982——

(Viewgraph.) [Ref. 2/6-8]

MR. MOORE: —we began what we called the early payload capability demonstration phase, and we looked at and we did fly a large number of different kinds of payloads to give us a feel for the capabilities of the Shuttle with respect to accommodating a number of different kinds of payloads. COMSAT is short for communications satellites, and in addition to the communications satellites, we flew several upper stages during that period of time. One is the PAM, or Payload Assist Module.

Let me pause. I think we put an acronym listing in the back of your book here, and we are going to try to make that as complete as we can because we in this business do an awful lot of talking in acronyms, and I apologize for that, but there are a couple of sheets in the back of the book with acronyms. We will

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try to make that more complete as time goes on.

We also flew the IUS, the Inertial Upper Stage, and you should note that we had an Inertial Upper Stage on this particular mission, 51-L, and I will come back to that point later. We also flew Spacelabs, I talked about. We did an EVA, which is an extravehicular activity where a crewperson would go outside of the Shuttle, and we also did an MMU flight, or a Manned Maneuvering Unit flight, where we actually flew a powered system away from the Shuttle and returned back to the Shuttle.

We did rendezvous on orbit, we did satellite repair, we did—on the Solar Array, and we also did a refuelling demonstration on the program. Beyond that period of time we have entered into what we call the Payload Operational Phase where we have done satellite retrievals, where we

have flown some DOD, additional DOD, Department of Defense payloads, and we have also done some salvage rescue operations in space with the rescue of the SYNCOM satellite last year.

(Viewgraph.) [Ref. 2/6-9 & 10]

CHAIRMAN ROGERS: Up to that point, had the military, DOD, been involved in these programs?

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MR. MOORE: The DOD has been involved in the Space Transportation System from the outset. In fact, they are working the launch pad facilities and have the responsibility now for the launch pad system development and facility development out at the Vandenberg Air Force Base. And the DOD plays a very strong role in the Shuttle program as far as working with NASA. There is a lot of interaction back and forth between the Department of Defense and NASA. A large contingent of the Department of Defense people are at the Johnson Space Center working hand in hand with our people, and we have also flown several dedicated missions on the Shuttle with the Department of Defense payloads on it.

So yes, the answer to your question is they are involved.

CHAIRMAN ROGERS: Has the role of the DOD changed at any point during this program?

MR. MOORE: Not in the recent past, sir. The role, in fact, it has gotten stronger. As time has gone on, I would say the role of the DOD is getting stronger in terms of their planned utilization of the Shuttle. We have plans in the latter part of this decade, the early part of the 1990s, where the DOD would plan to use a full one-third of the Shuttle capabilities.

So I would say the role is getting stronger,

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and their commitment to the Vandenberg Air Force Base launch system out there which will give us polar orbit launch capability—we now can only launch from the Kennedy Space Center, and basically achieve inclinations around 28-1/2 degrees to about 57 degrees latitude. The launch facilities out on the west coast will now give us polar orbiting capability which the DOD is working on that facility development.

Now, in the system deployment phase, we are in the process of implementing our major elements of the system, and at the Kennedy Space Center we have been building Pad B, the Launch Pad B. Up until this last launch we had been launching off of Pad A, and this 51-L mission was our first launch off of Pad B. We had also been putting in place our second TDRS, which is our Tracking and Data Relay Satellite System. That was a major cargo element on this flight, and the Tracking and Data Relay Satellite System is intended to allow us to communicate almost continuously with satellites from the Shuttle to the ground as opposed to using a lot of ground stations and so forth that we have been using up until this time.

We have also been planning to fly, and we have not flown it yet, a filament-wound case, which is a graphite/epoxy case to replace the steel cases on the

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solid rocket boosters. And if I could take a second, I will show you what these are.

These are the solid rocket boosters. These are steel cases here, and we have had a program underway in development to replace the steel cases with a graphite/epoxy case called filament wound case. The objective of doing that is to achieve more payload performance. We can get about 5,000 pounds more payload into orbit by going to a composite structure versus steel, and you will hear more about that later on.

The Vandenberg launch site I mentioned to you earlier, the improved engine life, or CENTAUR, the improved engine life is on the Shuttle main engine. We have a concern in the pro-

gram about lifetime associated with the Shuttle main engines, and we have been putting a lot of effort into trying to get ourselves into a position for improved lifetime. We are developing CENTAUR G prime which is an upper stage that fits into the Shuttle bay, and it was planned or is planned to be launched in—the first launch attempt was planned in the May timeframe of this year, to launch two planetary missions.

(Viewgraph.) [Ref. 2/6-9 & 10]

MR. MOORE: We are also planning this year to launch the third Tracking and Data Relay Satellite,

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again to give us the global coverage I talked about. Space Telescope is planned to be launched this year, a scientific payload. We are building the mobile launch platform, MLP-3, and the mobile platform is basically what our Shuttle System here rolls out to the launch pad on. You have seen the large crawler with the big system that the Shuttle is anchored on at the launch pad. That is called a mobile launch platform. We now have two of those in operation at the Kennedy Space Center, and we have been in the process of developing a third one at the Kennedy Space Center for operation sometime later this year.

CENTAUR G prime is another upper stage which is a derivative of the G prime system, and it has a little lower performance capability, and it is being principally developed not only for NASA missions but also for the Department of Defense missions. I should point out that CENTAUR development program is a joint responsibility of NASA and the Department of Defense, the Air Force in particular.

CSOC, the last one, is a Consolidated Space Operations Center which we are in the process of planning with the Department of Defense. It is the responsibility of the Department of Defense to develop this capability, and it would take over and develop and

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do some of the operations of the Shuttle from this particular capability in CSOC, and it is in the Colorado area, and it is planned to be operational in the early 1990s. So DOD would help us in the operations.

CHAIRMAN ROGERS: Would you mind giving us a little more information about Pad B and Pad A? You said Pad B was the first time you had used that?

MR. MOORE: Yes, sir.

CHAIRMAN ROGERS: And were the differences between—I assume there are differences between Pad A and Pad B?

Can the Commission—will the Commission be given some information about the differences?

MR. MOORE: Yes, sir. Pad A has been our primary launch platform in the Shuttle program up until this flight, this flight being the 25th flight of the Space Shuttle. Pad B is adjacent to Pad A by some few miles, and it is in design approximately identical to Pad A, and this launch, as I said, was the first launch attempt from Pad B.

CHAIRMAN ROGERS: All before were from Pad A?

MR. MOORE: Yes, sir.

Mr. Sieck, who will speak on the launch and landing operations at the Kennedy Space Center, can give you some additional information about Pad B this

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afternoon when he talks, and we will be happy to provide the Commission any additional data that you so desire regarding the similarities and differences between Pad A and Pad B.

CHAIRMAN ROGERS: Thank you very much.

MR. MOORE: The next several charts I won't spend a lot of time. I think they are mostly for your background, Mr. Chairman and Commission members.

(Viewgraph.) [Ref. 2/6-11]

MR. MOORE: These kind of plot as a function of time—and I apologize again for the line at the top. The chart did not come out very well, so you will have a hard time looking at the dates on this, but this chart basically was from the first launch of the Space Shuttle in April 1981 through the 1982 timeframe where we flew the STS-4.

The next chart—

(Viewgraph.) [Ref. 2/6-12]

MR. MOORE:—carries us into the latter part of 1983, and it shows the launches of STS-5 through STS-9, which is Spacelab. And there are a number of different kinds of payloads on here. Most of these payload names are satellites, communications satellites or other attached experiments like, for example, on STS-7, Palapa B-1 is an Indonesian satellite; SPAS-01 is

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a German payload structure and so forth, so to give you a little feel for those particular cargo elements.

DR. FEYNMAN: On the chart it says first flight of OV-99. Is that the Challenger?

MR. MOORE: Yes, OV-99 is Challenger. Let me just give you the numbers. OV-102 is Orbiter Columbia. That was the first orbiter built and flown. OV-99 is the Shuttle Orbiter Challenger. It is the second one delivered. OV-103 is Discovery, and it was the third one built and delivered. And OV-104 is Atlantis, and we just recently received that last year, as a matter of fact, and it has had its inaugural flight last year.

There is an orbiter called Enterprise which was a structural test orbiter, and it has now been turned over to the Air and Space Museum, and so we now have four flight-configured—had four flight-configured orbiters until the tragic mishap with Challenger.

Continuing on with the payload capabilities demonstration phase.

(Viewgraph.) [Ref. 2/6-13]

MR. MOORE: Through 1984 and early '85 we flew STS-41-B, 41-C, 41-D, 41-G and 51-A, and maybe I can spend a few seconds trying to give you a little bit of the sense of the nomenclature of the 41's: A's, B's and

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C's. And it is 41, the number four stands for the fiscal year of the flight. From October to September is the fiscal year, so it is scheduled in that period of time. One stands for the launch area we are using. One is the Kennedy Center Launch Area, and if we were launching out of Vandenberg that would be a two, and the As, Bs, Cs and Ds are kind of the sequences that we have planned the missions, although as things have occurred we have had to move a mission over another mission, and so you don't get exactly an alphabetized listing of the flights.

(Viewgraph.) [Ref. 2/6-14]

MR. MOORE: Our next chart here through the 1985 timeframe, and the early part of—well, I guess the next chart we will show you through the 1985, we flew STS-51-C, which was a dedicated Department of Defense mission, and we flew 51-D, 51-B, 51-G, F, and 51-I through the latter part of the 1985 timeframe. And as a matter of fact, 51-I, for a point of reference, I believe, was launched on November 27, in that timeframe, of 1985.

(Viewgraph.) [Ref. 2/6-15]

MR. MOORE: In the next chart, the 61-A, 51-J was another DOD dedicated flight. 61-A was a Spacelab flight. 61-B was, the payloads were the communications

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satellites, and then the last flight before 51-L that we flew was STS-61-C, and we flew that in early January, and it also had communications satellites on it, among other cargo elements.

And then the flight that we are here to discuss, the 51-L mission, the Challenger incident, was planned, was launched on the 28th of January. That kind of gives you, Mr. Chairman, an early overview of some of the flight history and some of the very top-level structure of how NASA is organized, and what we have done in the Shuttle program to now.

If it pleases you, I would like to proceed with the 51-L mission summary and talk to you a little bit about the events of the day during the launch, where we are in the investigation work that we have done to date, what teams we have formed, and where we plan to go from here.

CHAIRMAN ROGERS: Mr. Moore, let's see if any Commission members have any questions.

DR. WALKER: I had one question. Why is 51-L after some of the sixties?

MR. MOORE: It was originally scheduled to be in an alphabetized sequence, but because of some of the cargo changes and so forth, we moved that nomenclature into the next fiscal year, and we just held the

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nomenclature. Once you develop your documentation for a flight, it is awfully difficult several months before that time to go back and change all of your nomenclature. And so our principle is to hold the nomenclature, even though it may appear out of sequence in terms of the chronology of numbers and the alphabet.

CHAIRMAN ROGERS: All 24 of these flights were without accident, or were there minor accidents, and if so, how many?

MR. MOORE: The 24 flights to date have been without any major accident at all. We have a category called anomalies during a flight, like we may lose a power element or we may have something look anomalous on a flight, but no major accident. We have had a launch that has shut down on the launch pad, which is called a launch abort. The system is designed so that if things are not right before the solid rocket boosters light off, it will automatically go into a shutdown sequence. We had an occurrence of that. We also had an occurrence of a main engine which was shut down during ascent prior to reaching orbit, but we did reach orbit successfully, and the system operated as it was supposed to operate.

There have been a number of electronic problems, like we have had some problems with computers on board not functioning properly, and we have had some

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problems with fuel cells, but there have been no major accidents in the Space Shuttle program to date up until this last flight.

CHAIRMAN ROGERS: Did you find that the performance improved with each launch or remained about the same?

MR. MOORE: I think our performance in terms of the liftoff performance and in terms of the orbital performance, we knew more about the envelope we were operating under, and we have been pretty accurately staying in that. And so I would say the performance has not by design drastically improved. I think we have been able to characterize the performance more as a function of our launch experience as opposed to it improving as a function of time.

CHAIRMAN ROGERS: I assume that you have rather complete records of each one of these flights.

MR. MOORE: Yes, sir, we have. As you will hear during the day, Mr. Chairman, we do a complete, thorough documentation of each flight, getting ready to each flight, and as the Com-

mission so desires, we will be more than happy to provide you with all of the information you need in those areas.

CHAIRMAN ROGERS: And do those reports show whether one flight seemed to be more successful than

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another?

And I am directing my comment—did you find that the performance was improved with each flight or not? Were you more worried in later flights or about the same, based on experience?

MR. MOORE: I don't think that we have relaxed at all in the program, and I don't think we have been more worried about the performance. I think we have gotten probably more confidence as a function of our overall performance on these things, but some of the events that we talked about, like the engine shutdown on the launch pad, that certainly worried us about the main engines because you need them to get to orbit, and we put together extensive review teams to find out what we could do about the engines program, and we have done a lot of work on that, and you will hear some more about the engine activities.

But as a function of time, I think our performance has been better characterized in terms of understanding the Shuttle system from a total system point of view is the way I would describe it.

DR. WALKER: I have one other question.

When were the graphite/epoxy casings to be phased into the program?

MR. MOORE: They are scheduled to be flown on

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the initial Vandenberg launch site flight, which is now targeted for the middle of the summer. It is mid July at this point in time is the current plan. So we have not flown any elements of the filament wound case, the graphite/epoxy cases up until this point in time.

DR. WALKER: Once you use them, was the plan to abandon the steel casings?

MR. MOORE: No, it is not. We have a major question that the program is looking at right now, and we probably won't get any good data on that until later downstream, and our question, among others that is on the table about the graphite/epoxy cases today, is can we reuse them?

You know, we currently reuse the steel cases. The Shuttle returns, it has its engines on the back, the SRBs are returned. They have parachutes on them. We go back and retrieve the SRBs and go through a refurbishment cycle on them to reuse them. For the graphite/epoxy cases, we are doing some of our final testing at this point in time, and we are not sure whether or not we can reuse those filament-wound cases after we fly them and they come back and impact the ocean. We have not made a determination like that, so we are not planning to get out of the steel case SRB business at this point in time. We have a lot of

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additional work to go on the filament-wound cases.

MR. HOTZ: Mr. Moore, have you made any design changes in the steel casings of the SRBs since the beginning of the program?

MR. MOORE: I think there have been some very minor design changes in the SRB, and I think Mr. Judson Lovingood from the Marshall Space Flight Center will talk about that as he comes up here this afternoon or later on this morning. He will give you a detailed rundown of the chronology of the SRBs, the external tank and the main engines.

CHAIRMAN ROGERS: How many times can you reuse the booster?

MR. MOORE: We have not set a real high use limit. We probably, I think—and Bill Lucas, maybe you can help me on this—20 times, Mr. Commissioner, is the current plan for the reuse of the steel cases on the SRBs.

CHAIRMAN ROGERS: What is the largest number of uses?

MR. MOORE: I think the largest—and again, I am recalling from memory—is about three to four times. This particular flight, 51-L, as I recall, had maximum of two uses of any of the components, possibly three, if my memory serves me correctly.

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MR. SUTTER: I have one short question. The flights are characterized, the first flights were test flights to check the Shuttle system, and then the second phase was capabilities demo phase.

In the first flights which were labeled flight tests, was there a documentation of what was trying to be accomplished, what instrumentation was required, and then after those flights, was there a documentation of what the flights proved?

MR. MOORE: Yes, sir. We have very, very extensive documentation on all those flights, what we learned from those flights and what were changed as we left from the orbital flight test phase into the other phases of the program. We maintain very, very extensive records of all the flights.

MR. SUTTER: And at the conclusion of those flights were the objectives pretty well achieved?

MR. MOORE: In general, I would say the objectives of those flights were met. Each flight data was analyzed in great detail and fed back in to the program designers to look at what they actually achieved versus what they expected. And again, we will be able—we will be happy to make available to the Commission any data that the Commission so desires relative to any of the flights up until now.

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Now, if I might, Mr. Chairman, I would like to move into the 51-L mission which is the mission we are talking about, Challenger's tragic mission, and I would like to start out by giving you a very brief look at what the cargo elements were on board.

(Viewgraph.) [Ref. 2/6-16]

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MR. MOORE: I have talked about these, but let me talk to you again quickly. The largest payload component on board, and I should point out that the shuttle cargo bay, you are going to hear more about the dimensional characteristics and performance characteristics of the shuttle, but I should point out that the shuttle cargo bay is 15 feet in diameter and 60 feet long, to give you some feel of the dimensionality of the cargo bay, and we have flown a maximum of eight people in the shuttle up until this point in time.

On this flight, we had the Tracking and Data Relay Satellite. This was to be the second Tracking and Data Relay Satellite deployed. There is one on orbit now, and it was supported by an Inertial Upper Stage developed by the Air Force and used by NASA for the deployment of the satellite from low earth orbit where the shuttle takes you, up to the geosynchronous orbit where the Tracking and Data Relay Satellite has a requirement.

We also had on board a payload called Spartan-Halley. This was a structural element that actually sat across the shuttle bay attached to the cargo bay and supported several science instruments to do some observations of Comet Halley. And then we had in the crew compartment or the middeck area, we

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had the experiments associated with the Teacher-in-Space Program.

We had an experiment called CHAMP, Comet Halley Active Monitoring Program, a fluid dynamics experiment, some student experiments looking at different kinds of things from high school students, The Radiation Monitoring Experiment, and a Phase Partitioning Experiment.

Most of those sat in the middeck area of the orbiter, and you will hear some more about that particular area, and where the lockers are and so forth for putting those kinds of experiments. They are fairly small experiments.

(Viewgraph.) [Ref. 2/6-17]

MR. MOORE: The next chart shows the layout of the major elements of the cargo, and it showed the TDRS-B/IUS sitting in the cargo bay, the Spartan-Halley on the impasse, the support structure. It also shows on there an acronym which I talked about before called the RMS, which is the Remote Manipulator System. That is the arm on board.

The arm was planned to be used on this flight to pick the Spartan system up, deploy it overboard, leave it in orbit for a couple of days, rendezvous back with it, pick it up, and store it back into the cargo

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bay and return back to the earth.

GENERAL KUTYNA: Jess, may I ask, how many remote manipulator arms do you have? Is that the only one?

MR. MOORE: No, we have another arm, and also we have a program with the Canadians for possibly refurbishing another one.

DR. WALKER: Could you say a word about the IUS?

MR. MOORE: Yes. The IUS is a two-stage solid inertial upper stage. It is solid rockets, and the TDRS in this case, I believe, is 5,000 or 6,000 pounds, and its purpose was basically to boost it from low earth orbit, which was about 140 or 50 nautical miles up to its position in geostationary orbit, which is about 22,000 miles. So it provides the propulsion to basically boost the Tracking and Data Relay Satellite up to its final orbital destination in geosynchronous orbit.

It is a two-stage rocket system. The first stage burns, and then after it burns it separates, and then it burns a second stage, and at the end of the second stage burn the IUS second stage separates from the TDRS and then the Tracking and Data Relay Satellite provides its own navigation and its own orbital adjustments with its own propulsion system on board.

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(Viewgraph.) [Ref. 2/6-18]

MR. MOORE: The next chart gives you a quick summary of the STS 51-L mission profile. This shows the liftoff. In the case of 51-L the liftoff occurred at 11:38 a.m. on the 28th. We go through what we call a High Q phase or a high dynamic pressure phase for the flight, and then we go through planned SRB staging, and that SRB staging is about two minutes, and this 51-L mission was planned for 128 seconds, and at that point in time we had planned to stage off the SRBs, continue with the tank on the orbiter.

Remember, the tank provides the fuel to the shuttle main engines until we achieve our orbital destination some 150 or so miles into space. The tank stays with the orbiter or is planned to stay with the orbiter on this flight for about 523 seconds, after which time it has essentially depleted itself of its fuel. We shut the engines down, and some ten to eighteen seconds later we then separate the external tank from the orbiter, and then we plan to go about our orbital profile.

That plans to give you some kind of feel for the profile. We had a six-plus day mission plan, and we had planned to land at the Kennedy Space Center on six plus a few hours, six days plus a few hours, so the

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day-by-day mission profile is given to you in your upper righthand portion of this vu-graph.

DR. RIDE: You might say something about the Max Q phase of the flight.

MR. MOORE: The Max Q is the maximum dynamic phase. We see that we planned in the launch profile. We go through a throttling down of the main engines during that period of time, and we are concerned about loads on the orbiter, and so we throttle our main engines down, and this particular flight had a nominal engine profile of flying at like 104 percent of rated power, where we have flown a large, large part of our flights to this date.

We throttle down during that period of time to some lower percentage, and then after we have gone through that phase of the flight, we will begin to throttle back up again and hold that throttle setting until we get to geosynchronous orbit.

We are trying to minimize the loads on the total shuttle system during the time it is seeing its maximum dynamic pressure.

DR. FEYNMAN: Was there any special extra heavy load on this particular flight higher than other flights?

MR. MOORE: We do not think so, sir. In terms

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of the prelaunch calculations, we get wind data prior to launch. We look at day of launch winds even an hour or so right before launch and try to get wind profiles and any kind of loads like that, and we have load indicators on the orbiter that are sensitive to different kinds of winds, whether you are getting a tailwind or a sidewind, and all of our calculations during that day had indicated that our loads condition was okay.

MR. HOTZ: Is there any change in the thrust of the solid rocket boosters when you are throttling back the main engines?

MR. MOORE: No, sir. The way the liftoff works is the shuttle main engines come on at approximately six seconds prior to what we call liftoff. We bring those engines up to their near nominal thrust level. We check those engines to make sure we have full redundancy on all the engines.

We have redundant systems on the engines, and once that check is made, a signal is sent to the solids to ignite the solids, and that happens about, as I said, about six to seven seconds after you have ignited the main engines.

Once the solids are ignited, then it lifts off the launchpad, and the solids are designed to provide stable thrusting during that period of time until they

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are separated, in this case 128 seconds after liftoff.

MR. HOTZ: They don't change during the entire burn?

MR. MOORE: They are not planned to be changed during the entire burn. Now, we do have a thrust cone on the back of each of the solids, and there is a little gimbaling motion in case we do get a little bit of loading effect.

We can change the gimbal on there to change the orientation of the thrust, but the planned thrust of the solids is to have a matched pair of solids, a balanced thrust during the entire flight.

MR. HOTZ: Thank you.

MR. ACHESON: Mr. Moore, at some point in the presentation today will we be briefed on the test procedures, the preflight test procedures of all of the elements?

MR. MOORE: Yes, sir.

MR. ACHESON: And the contractor test procedures?

MR. MOORE: Sir, our briefing under the shuttle systems, when we begin to talk about the orbiter, we begin to talk about all of the propulsive elements of the shuttle system.

We will talk about the test procedures, the

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NASA people involved, the NASA structure involved, the contractors involved, and then we will talk about our design approach, our certification approach, our testing approach.

We will also talk about the entire process that we use to get ready for a shuttle launch, and how that is tiered up from flight hardware and flight software point of view until it comes up to my level at NASA Headquarters. We will give you very, very much detail on that during the course of the day.

(Viewgraph.) [Ref. 2/6-19]

MR. MOORE: The next chart shows some specific mission data on STS 51-L, launch data on 51-L, January 28th, 1986. The orbiter is OV-99 Challenger. And we had a planned liftoff time of 9:38. Now, we had a three-hour launch window, and for a lot of our flights we don't have the luxury of a very long time to launch in terms of meeting payload requirements.

Some launch windows are like 50 minutes, and others are like an hour and a half or two hours. This launch we had three hours to launch. The throttle setting on the main engines were 104 percent of rated power level, and we have flown many times at 104 percent, and the abort thrust setting in case we had a problem going uphill was 104 percent as well. We keep

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the same engine thrust. The inclination of the orbit we had planned was 28.45 degrees, and we had planned to achieve an orbital altitude of 153 and a half nautical miles circular.

DR. FEYNMAN: What is the inclination? What angle is that?

MR. MOORE: It is basically the inclination of the orbit relative to the latitude of where we are launching out of Kennedy, and it is the inclination relative to the—say, polar inclination. You are at 90 degrees. You are basically going around the earth, over the poles of the earth, and you can allow the earth to spin.

You have got an inclined orbit here like the 28 and a half degrees, and so you are not getting full coverage of the earth, so if you are plus or minus 28 and a half degrees latitude coverage in effect and your orbit is like a sine wave which walks across a still map if you were to plot continuous maps of the orbit.

One of the considerations among others that we have to do in this program is to look at our landing sites, not only for end-of-mission landing sites, which is a concern, but also abort once around, which is a condition where something could happen during the powered flight phase of the profile and not allow us to achieve

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a full stable orbit.

In that case, we could go once around the earth and come back. Edwards was a planned landing if we had an abort of that nature. We look at weather alternates as well.

The Kennedy Space Center has inclement weather on a fairly high frequency—witness the last launch prior to 51-L—in terms of clouds or in terms of rain, and we have very stringent rules about what landing requirements are on the system, and so we have a weather alternate.

We also have a trans-Atlantic abort capability in the event we lose an engine during a certain phase of the flight.

We have runways and people and systems on standby in places in Africa and also places in Spain where the shuttle could land if such a problem like that occurred, and in this case for Mission 51-L we had runway availability in Dakar, Senegal, and also in Casablanca, Morocco.

Both of those runways were considered viable trans-Atlantic landing sites in the event we had a problem, and we look at that on a real time basis during the preparations for launch and during the actual launch count.

We also have what we call an RTLS. Let me say

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before I mention this there is a whole number of abort kinds of capabilities in the system. We are not planning to go into great detail today on that, but we will be happy to provide you with additional data on kind of the abort modes in the shuttle program.

We also have one other capability called RTLS. That stands for Return to Launch Site, and that is in the event again during a certain phase of the projectory if we have a problem, we can return back to the Kennedy Space Center. After that particular problem has been noticed, and after we have separated the solids, you can come back to the Kennedy Space Center and land there.

So, a constraint for launch is that we have good weather at the shuttle landing strip at the Kennedy Space Center for some 30 to 40 minutes after a launch to make sure that we have a capability if that event occurred to land at the Kennedy Space Center.

DR. RIDE: It might be helpful to go into a little bit more of the things that you might do an RTLS for or the constraints on an RTLS.

MR. MOORE: Arnie is planning to cover that, Sally, during his discussions today about what an RTLS and what other abort modes might be, but that is a good point. We will do that.

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Flight duration, as I mentioned, was six days.

(Viewgraph.) [Ref. 2/6-20]

MR. MOORE: Now, I would like to tell you a little bit about launch date chronology leading up to our launch on the 28th, and this will give you a feel, a very preliminary feel, about the meetings that we have in terms of getting ready for a launch and who participates in that, and I am sure we will want to spend some more time on that as time goes on.

The first day we met at the Kennedy Space Center was on January 25th. Prior to that time there had been a number of meetings that a lot of the project people and even myself had participated in, talking about are we ready to launch Challenger on the 25th, at that point in time, or the 26th, I guess, was when that was scheduled, and we all agreed, so we all met at the Kennedy Space Center on the 25th of January, anticipating a launch on Sunday, and that was the 26th.

We have what we call an L-1 Day Review. Participants include myself, my senior managers, and my NASA Center people, directors, the contractor senior people, where we sit around the table and review the status of the system prior to launching. That meeting occurred at 11:00 a.m., and the major outcome of that

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meeting was that we had a weather problem, potential weather problem, on Sunday.

We decided at that point in time to hold a meeting Saturday afternoon or late Saturday evening, I should say, 9:30.

We met again with essentially the same type of people there, although not as large, and at that time we got our weather reports, and we decided the weather for the next day was no go. We had no optimism for the weatherman that said the rain was going to stop, or we would have an attempt to get off, and it takes an effort to get the team up, and so we decided to bet on the weatherman's forecast, and decided not to launch that day.

Well, it turns out the early part of Sunday morning for about an hour was a reasonable time. The frontal system had not reached Florida yet, and so we didn't win that call in terms of the weather, but it was a no go on Saturday night.

DR. FEYNMAN: Would you explain why we are so sensitive to the weather?

MR. MOORE: Yes, there are several reasons. I mentioned the return to the landing site. We need to have visibility if we get into a situation where we need to return to the landing site after launch, and the

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pilots and the commanders need to be able to see the runway and so forth. So you need a ceiling limitation on it.

We also need to maintain specifications on wind velocity so we don't exceed crosswinds. Landing on a runway and getting too high of a crosswind may cause us to deviate off of the runway and so forth, so we have a crosswind limit. During assent, assuming a nominal flight, a chief concern is damage to tiles due to rain. We have had experiences in seeing what the effects of a brief shower can do in terms of the tiles. The tiles are thermal insulation blocks, very thick. A lot of them are very thick on the bottom of the orbiter. But if you have a raindrop and you are going at a very high velocity, it tends to erode the tiles, pock the tiles, and that causes us a grave concern regarding the thermal protection.

In addition to that, you are worried about the turnaround time of the orbiters as well, because with the kind of tile damage that one could get in rain, you have an awful lot of work to do to go back and replace tiles back on the system. So there are a number of concerns that weather enters into, and it is a major factor in our assessment of whether or not we are ready

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to launch.

CHAIRMAN ROGERS: Mr. Moore, in that connection, I notice a press report that one of the contractors said that they gave a warning of some sort about the cold weather. Could you deal with that, please?

MR. MOORE: Yes. I am going to continue on with this chart, which will deal with that cold weather question in a fair amount of detail, Mr. Chairman.

CHAIRMAN ROGERS: Fine.

MR. MOORE: Since we decided not to attempt the launch on the 26th, we called a meeting on the 26th itself at 2:00 o'clock in the afternoon, again an MMT meeting or Mission Management Team meeting, to sit down and see what the weather situation was projected to be, plus the status of our launch systems, including the launch pad and the shuttle system.

We decided after reviewing everything that launch was a confirmed go for Monday, the 27th, and that we confirmed that we were ready to attempt to launch on 9:37 a.m. on Monday, January 27th. Well, on Monday, January 27th, we did in fact get ready for the launch, and that involves making sure all of the systems have been checked out, the launch system is up, and making sure you have fueled up the external tank, which we do

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about seven or eight hours before the launch, and making sure you then bring the crew on board and make sure all the systems are ready for launch.

And so we started that late in the evening, started the final countdown and began a launch attempt for Monday morning at 9:30. We had a couple of initial delays during that attempt. There are a couple of microswitches on the orbiter that we need to receive closed indications of before we close out our requirement, and we were only getting one indication of a microswitch on there was closed, and so we went back and did a pressure check in the cabin to make sure that the seal was proper on the cabin door and you didn't have any leaks, and we convinced ourselves that that was okay.

Then we have a piece of GSE or Ground Support Equipment which attaches to the orbiter door to allow the technicians to close the door, and it is fastened on by some bolts and a nut plate that is attached to the orbiter Challenger's door, and it is fastened on in three places. One of the nut plates that is fastened onto the orbiter came loose, and we could not get the bolt off in a very timely manner, and so we sent some technicians out to actually take a hacksaw and pull this piece of Ground Support

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Equipment off.

That was successfully done to our satisfaction, but by the time we finished that, we had high crosswinds, and I mentioned crosswinds earlier on January 28th or 27th, and the high crosswinds, we had wind gusts up to 30 knots, and our limit is like 15 knots.

We have a limitation, a flight rule in the program that we did not launch because of the Return to Landing Site condition if crosswinds are too high. So the winds kept getting stronger that day, and after watching the wind patterns for some hour to an hour and a half, we decided to scrub that particular launch attempt for that day.

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Then we called a Mission Management Team Meeting again, which is made up of the senior NASA managers, shuttle managers, Center directors in some cases, and contractor support people in other cases, at 2:00 in the afternoon on the 27th, and discussed should we attempt to launch on the 28th.

We had a fairly lengthy meeting, with the only concern being expressed that the weatherman had predicted the temperatures were going to be fairly cold that evening, down into the mid-20s. It was kind of the prediction.

And we talked about temperature concerns, and the main concern that came out of our meeting as far as temperature was, are the water systems or the support systems on the launch pad, the water pipes, eyewashes where technicians have water running to wash their eyes in the event they get contaminated on the launchpad and so forth, were these pipes going to freeze, and that was the major concern that the system had at that point in time.

Now the launch team guys were given the instructions to proceed with the launch for 9:38 in the morning, assuming there were no problems with tanking and getting the system working and ready, and because we had one waterpipe that broke on the launchpad, and I

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think Bob Sieck can talk a little bit more about this than I can from Kennedy, we were an hour down in our launch attempt. So instead of 9:38 the earliest we could have launched was like 10:38, because our count was delayed about an hour. We had one pipe that burst.

The other problem that we had and were concerned about all during this discussion was ice. We were concerned about ice buildup, and I think this is where you read the article, Mr. Chairman, about the ice concern. We were concerned about ice on the launch tower and that particular ice doing some damage to the orbiter surfaces and the orbiter tiles because of how fragile those tiles are from impacts and so forth.

There were technical meetings held to assess the ice situation. A major technical meeting was held involving a number of people that was chaired by Arnie Aldrich here of JSC. Their assessment came back that the system is okay, we should hold the launch for probably one more hour to allow a last-minute ice team to go out at about 20 minutes before launch and to validate the ice concerns, to go back and do another doublecheck of the ice, and that was done.

They came back and reported that everything was okay, and that we ought to go for launch, and a launch window then opened up at 11:38, I believe was the

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time, on Tuesday morning.

DR. FEYNMAN: On the 27th you made a launch attempt?

MR. MOORE: Yes, sir.

DR. FEYNMAN: That means you put fuel into the tanks?

MR. MOORE: Yes, sir.

DR. FEYNMAN: Does it stay in the tanks all this time, or do you take it out?

MR. MOORE: No, sir. Immediately after we, so-called in our jargon, scrub, we start safing the vehicle. The crew stays on board and does a number of functions to get the vehicle in a safe condition to make sure all propellants and all electrical systems are properly safed. Other people go to the launchpad to start safing the launchpad, and then we allow the hazard and safety teams to go out and make sure it is all acceptable for people to come out and do the deservicing on the tank, and the fuel is drained out of the tank, and it will not be replenished again until we get ready to launch again, which is again some seven or so hours before the actual liftoff time.

DR. FEYNMAN: And all this time even when the tank is empty, the tank is standing there, and the rest of the equipment, in the weather?

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MR. MOORE: Yes, sir.

DR. FEYNMAN: How early was it put out in the weather?

MR. MOORE: Sir, I am going to have to ask Bob Sieck. This tank and the entire stack on Challenger was moved out, I believe——

MR. SIECK: December 21st.

MR. MOORE: It was moved out to the launch pad on December 21st, sir.

DR. WHEELON: At your meeting where you were concerned about the weather and the temperature, did you discuss and consider the impact that that weather might have, and the temperatures in particular, on the vehicle?

MR. MOORE: Yes, sir, that was discussed at the meeting, and I think the technical team meeting that Arnie chaired—Arnie can probably comment on the specifics of that, because he came back to me after the meeting was held on the temperature discussion and reported that everybody was okay from a temperature standpoint.

And I will ask Arnold Aldrich when he comes up to talk about that in a fair amount of detail, since Arnie chaired the meeting from all the parties involved in that particular session.

CHAIRMAN ROGERS: I thought that the report I

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read about temperature referred not to the outside of the space ship but to the booster rocket. The claim was, according to the newspaper, that there was concern that the cold temperature might have affected the booster rocket inside not outside.

MR. MOORE: That may be. The one paper or article I remember seeing, Mr. Chairman, was the article on the effects on the orbiter and so forth, and I will ask the people here who are in charge of the solid rocket booster to talk about any discussions that went on relative to that, and feel free to ask those questions to the members who have the responsibility for the various program elements as they discuss their systems.

CHAIRMAN ROGERS: What I refer to is not a rumor or just gossip. It was a statement by one of the contractors that was a quote that was issued.

MR. MOORE: Yes, sir.

DR. WHEELON: But, Jess, just to come back and be clear, at your meeting was the potential impact of low temperatures on the SRBs discussed with you? Was it dismissed or not discussed?

MR. MOORE: At the meeting that I had with Mr. Aldrich, who had come back after the review from his technical team meeting, it was not discussed at my

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meeting. It was discussed at the meeting we had, the Mission Management Team meeting at 2:00 on the 27th of January. Is that right, Arnie? It was discussed by all of the people representing all of the systems. It was discussed on the 27th of January. Again, I will ask Arnie to give a little bit more details of that, since he was involved in that particular meeting on the orbiter.

DR. WHEELON: But it was not presented to you as a matter of potential concern?

MR. MOORE: It was not presented to me as a matter of concern. That is correct.

DR. WALKER: Mr. Moore, will you at some point tell us how many temperature sensors you had in the vehicle and where they were?

MR. MOORE: I will have to have the system design people do it, and let me ask the project element management down here if they would please talk about that to the extent they can.

Jud, can you do that from the Marshall side, and Arnie from the orbiter side?

(Viewgraph.) [Ref. 2/6-21]

MR. MOORE: Okay, the next chart shows the initial assessment after the launch, after the Challenger lifted off at 11:38 on January 28th. I

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mentioned the fact that the launch had been delayed for a couple of hours. The launch processing equipment problems I talked about, the ice inspection of launch complex and ice removal. I did not have any concerns about the temperature expressed other than the concern on the complex, launch complex.

The actual flight, the ascent appeared normal based upon our initial quick looks for the first 73 seconds, and it went through its main program roll maneuver where the shuttle rolls from its initial launch configuration through its maximum dynamic pressure I talked about, and then the throttle down and throttle back up of the shuttle main engines.

The vehicle again appeared to be performing nominally at our 104 percent thrust at approximately 1,200 miles an hour at approximately 47,600 feet, when all our telemetry stopped, and at that point in time we observed the breakup from the ground. All of our controllers that we heard over the loop and all of the net had indicated that the flight was nominal were all of the calls that I had heard during the morning of the launch.

(Viewgraph.) [Ref. 2/6-22]

MR. MOORE: What we initially put in place, the immediate actions that we took, we impounded

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immediately all data and information from this flight at all sites. We have a contingency plan in NASA, and each center has a contingency plan on STS contingency events of this nature, and immediately I then was requested by Dr. Graham to form a Mishap Investigation Board.

I immediately put that into effect, and I had members, the director of the Kennedy Space Center, the director of the Marshall Space Flight Center, Arnold Aldrich, the National STS Program manager at Johnson, and Walt Williams, a former NASA employee, a special assistant to the NASA administrator, as immediate members, and additionally I added in the next couple of days Bob Crippen of the Astronaut Office at Johnson Space Center, and I also added Joe Curran, who is director of the Space and Light Sciences from the Johnson Space Center. As ex officio members on my group,

I added John O'Brien, chief counsel at NASA, and Milt Silviera, who is the chief engineer at NASA. Jim Harrington was my director of STS integration. He was the executive secretary. And shortly after this formation, we immediately put into effect a number of teams to start action.

I have listed those teams on this chart, and I will quickly just run through them. I think you can read them, and they are probably self-explanatory, but

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some of them, I think, probably are worthy of some mention.

(Viewgraph.) [Ref. 2/6-23]

MR. MOORE: The Flight Data Trajectory and Com Team was immediately formed, a team to do the analysis of the launchpad facility at beach area, and I should point out, Mr. Chairman and Commission members, that we have held all the data, impounded all the data, kept the configurations the same as it was the day of the incident, and we will be working with your Commission as well as our own activities before releasing any of those types of information.

DR. WHEELON: Just before you get too far away from it, could we go back to the trajectory circumstances surrounding the accident? You have indicated in the handout that you were at 47,600 feet, going approximately 1,200 miles per hour. You have just gone through maximum dynamic pressure. To what level did you throttle down during that period?

MR. MOORE: I will have to recall my memory, but we do have a throttle profile. Let me look at my information. We throttle down initially from 104 percent, and I will be happy to give you a copy of this throttle profile. We throttled down from 104 percent at

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20 seconds to 94 percent, and that is held until 36 seconds, and then at 39 seconds, between 36 and 39 seconds we throttle down to 65 percent and hold that from 39 seconds until 52 seconds, and then from 52 seconds to 57 seconds we throttle up to 104 percent.

DR. WHEELON: Was that the usual throttle-up, throttle-down profile?

MR. MOORE: It varies depending upon the loads and so forth that we have got in the system, you know, the cargo elements and the kind of profile that we have to fly to achieve it, but we always generally go through a throttle bucket of that general type, and we will be happy to get you the specific.

DR. WHEELON: But to your knowledge there was nothing that distinguished this profile from those flown previously except for the payload compensation?

MR. MOORE: No, sir, to my knowledge there was nothing unique or that distinguished this, and let me say that we are doing—right now a lot of work is going on in looking at the detailed trajectory calculations. It takes—the Marshall Space Flight Center gets data. The Cape gets data. Johnson is the lead in this thing, and they have got all of that flight data, and we have a major team going and looking at synching up all of the trajectory data during the

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various mission phases and mission events that went on.

DR. WHEELON: What was the Mach number at this time?

MR. MOORE: About 1.8, I believe, if my memory serves me correct.

MR. SUTTER: Depending upon the load and everything, isn't there a variation in the loads as you go through this, rather dynamic?

MR. MOORE: There is a variation in the load, and we use a parameter called Q alpha, which is dynamic pressure versus an angle of attack to look at the load calculations, and also have instrumentation that we look at, and look at various load points on the wings and the various surfaces of the orbiter, and do that calculation based upon a given kind of wind profile.

We put balloons up starting at like 36 hours before launch to 24, down to about an hour or so before launch to get wind profile data. That is fed back into our computer programs to give us load indications, to see if we have got any exceedences on any parts of the orbiter.

MR. SUTTER: So during this flight then the load versus time and compared to other flights is something that will be known?

MR. MOORE: That is correct. Yes, sir. We will know that very,

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very precisely, and as to our knowledge on the day of launch, we did not have any loading sequences on any of the indications and trajectory analyses we had.

DR. WALKER: Is the throttle controlled by the crew?

MR. MOORE: The throttle is automatic.

DR. WALKER: Thank you.

DR. RIDE: The throttle can be controlled by the crew, but on a nominal ascent it is not.

MR. MOORE: Sally knows very well. Excuse me. A nominal throttle is automatic by the general purpose computer system on board which basically flies the flight profile.

There are a number of other teams we put in place, photography teams, data analysis, pedigree teams looking at the hardware, looking at the processing of this hardware. The quality records, the manufacturing records, and so forth on that are all being put in place.

Looking at security, in terms of anything that would be anomalous as far as security, range safety, public affairs. We have got a team on the flight crew with Bob Obermeyer chairing that from JSC. Marshall has a team on the main propulsion system, and the flight vehicle impoundment has also been formed.

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We formed some additional teams on salvage and recovery, and our philosophy in the recovery of all the debris or wreckage from this tragic event has been to identify as best we could the areas and to delicately move when recovering, the parts that we possibly can without doing any additional damage.

We have, as you will see on the next chart, the next slides, a lot of support from a lot of different people in this whole area.

The other thing I want to mention to you is that we are forming a devil's advocate team, and that devil's advocate team is a TBD over there, which means To Be Determined. I have not

named the members of that team, and that will be a team which will set off and support my activities and think up scenarios that may have occurred on this mission that will not be intimately involved into the detailed scenario analysis that we are doing with our own teams in place here. There will be a team set off to the side and hopefully do some independent thinking to make sure we are not letting anything fall through the cracks.

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(Viewgraph.) [Ref. 2/6-24]

MR. MOORE: The status as of today, we have reviewed some data, and our analysis does continue. As I said earlier, we are putting a very, very detailed time line of all events together. The initial time lines that we saw right after the occurrence were kind of first order time lines, and we are going back and developing and constructing the high-speed data to look at it.

We are enhancing all of our photography that we can, and we are concentrating a lot of that photography on the righthand solid rocket booster. As you probably have seen, we have released some photos which—I have three of them in here—which would indicate a plume in the righthand solid rocket booster.

The salvage and recovery operations is proceeding. I would like to just say, Mr. Chairman, that we have had extensive cooperation from all branches of the military, and we very much appreciate that, and also extensive cooperation from the National Transportation Safety Board, who have just been invaluable to us in helping us and assisting us in this grave incident that we are going through.

The wreckage analysis and reassembly is proceeding well, and we have essentially from a

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procedural standpoint turned that over to the National Transportation Safety Board. They are working with us in laying out areas where we are trying to preserve as much of a wreckage as we can, and laying it out in some manner that we hope will give us some clues in terms of what kind of anomalies we did experience on this flight.

The next three charts show the three photos that we released, and again I apologize. We are working on getting each member of the Commission quality photos to replace the photos that did not turn out. In the interest of time I was not able to put any better photos.

You can see them on the monitor here. This photo was taken at about—and you are looking at the righthand solid rocket booster here, and it may be difficult to see on your screen, but the external tank outline is here. The solid rocket booster is shown here.

These are some reflections, we believe. We also think this is a reflection, but again these are very, very preliminary, and we are not prepared to conclude exactly what all of these are. These appear to be the engine plumes, and you can see the tail of the orbiter here at this point in time, and that photograph was observed at 58.3 seconds. [Ref. 2/6-25]

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(Viewgraph.) [Ref. 2/6-26]

MR. MOORE: The next chart will show what appears to be a plume in this area, in the area of the righthand solid coming out at a time of 59.8 seconds.

(Viewgraph.) [Ref. 2/6-27]

MR. MOORE: And the final chart shows the plume has basically grown and merged into the tail from the engines and the other solid, and it basically looks like it has moved quite a bit here, and that occurred at 73 seconds, just milliseconds from the tragic event.

CHAIRMAN ROGERS: Would you mind showing us on the model where that plume is?

MR. MOORE: Let me make one comment. I can't show you exactly where it is, because we don't know exactly. I can show you the vicinity of where it is, is what I will attempt to do.

This is the righthand solid rocket booster, and it appears that the plume is in this area in here. Somewhere in this area is where it would appear, and until we complete our detailed photographic enhancements with the best laboratories that we can get to support us through the overlays and make sure the trajectory siting and the angles of the cameras are all pinned down, it is going to be very difficult to pin it down any closer than to just say that it appears in this area right in here.

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MR. HOTZ: Which segment of the solid rocket would that be?

MR. MOORE: We do not know. This is an aft segment here, and there is an aft center segment right in here that is joined together in this area. This is the structural attach point here to the external tank, and we don't know whether it is the aft center segment. We don't know whether it is the aft segment. We don't know for sure it is the SRB.

I will caution you, it appears in that area, but we are not ruling out anything at this point. I just can't say that other than there appears to be a plume in that area. That is basically all the data that we have at this point in time until we do our high-speed photography enhancement and begin to try to pin that down some more.

DR. WALKER: Could you show us where the seams are in the solid rocket booster, approximately?

MR. MOORE: I can attempt to do that. You are going to see that laid out in quite a bit of detail when the Marshall people talk about the solid rocket booster and so forth.

DR. WALKER: I can wait until then.

MR. MOORE: Would you, please? Thank you.

(Viewgraph.) [Ref. 2/6-28]

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MR. MOORE: Now, my final chart, Mr. Chairman and members of the Commission, is to tell you that the activities we initiated on that tragic Tuesday at NASA are continuing. We are doing everything we possibly can to analyze the data from this occurrence and put in place a mechanism to fully assess and evaluate and determine the problems associated with this particular mission.

Yesterday I was designated by Dr. Graham to be the chairman of the 51-L Design Data and Design Analysis Task Force. We are continuing to analyze the facts and circumstances and to identify any design issues that we can surrounding this incident, and we are authorized to use any technical and scientific resources within NASA and any available external resources that we possibly can that we feel the need to call upon to solve this problem, and we would be happy to support you and the members of this Commission in any way you deem fit, and we are planning to proceed forthright in our analysis and detailed evaluation of this tragic event. And that is all the charts that I have prepared this morning, Mr. Chairman.

CHAIRMAN ROGERS: Thank you very much, Mr. Moore, for a very good briefing. We appreciate it.

MR. MOORE: With your permission, I will

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introduce Arnold Aldrich from the Johnson Space Center, and he will go through a process of covering the other elements that I cited earlier.

CHAIRMAN ROGERS: I would like to suggest that we take a five-minute recess, if you don't mind, before we get started.

(Whereupon, a brief recess was taken.)

CHAIRMAN ROGERS: Ladies and gentlemen, we will come to order, please.

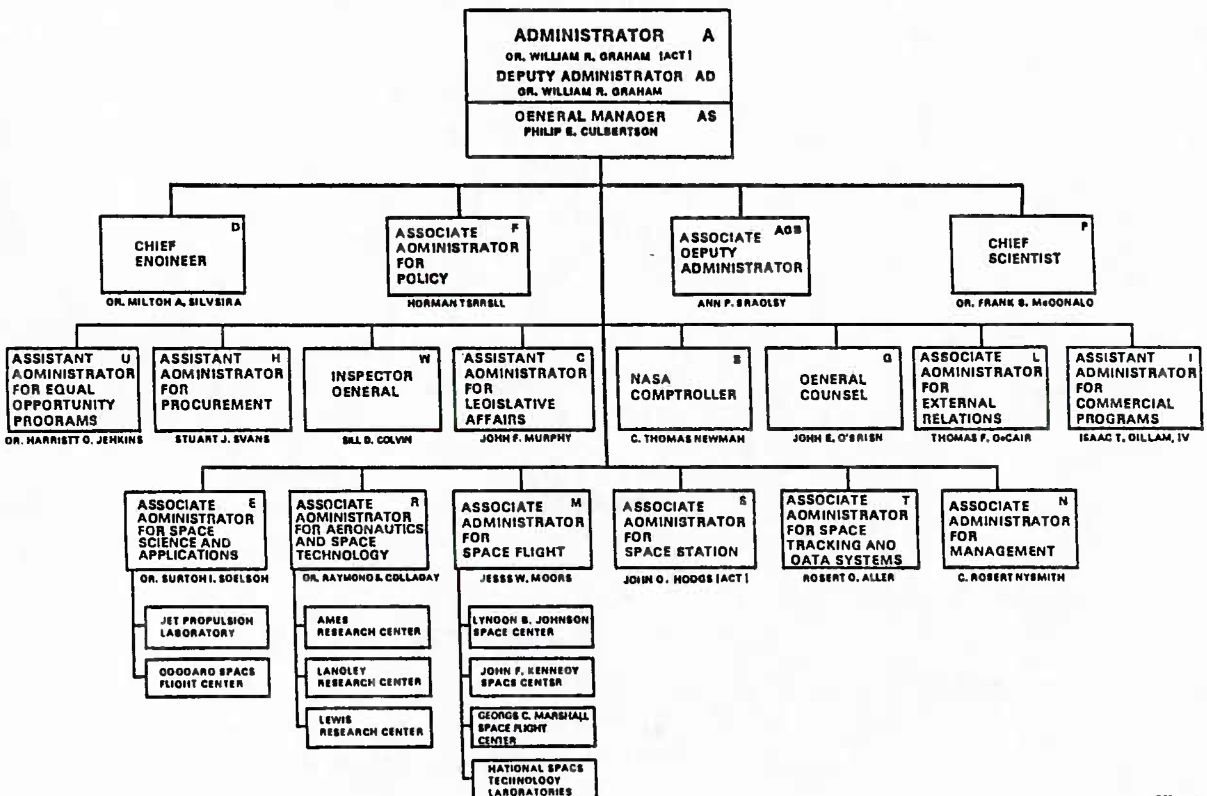
Would you please swear Mr. Aldrich in?

Mr. Aldrich, proceed.

AGENDA

I.	INTRODUCTION	DR. WILLIAM R. GRAHAM ACTING ADMINISTRATOR, NASA
II.	OVERVIEW	JESSE W. MOORE ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT, NASA
III.	SPACE SHUTTLE SYSTEMS	ARNOLD D. ALDRICH MANAGER, NATIONAL SPACE TRANSPORTATION SYSTEMS PROGRAM JOHNSON SPACE CENTER
		DR. JUDSON A. LOVINGOOD DEPUTY MANAGER, SHUTTLE PROJECTS OFFICE, MARSHALL SPACE FLIGHT CENTER
		ROBERT B. SIECK DIRECTOR, SHUTTLE MANAGEMENT & OPERATIONS, KENNEDY SPACE CENTER
IV.	DESIGN AND DEVELOPMENT PROCESS	THOMAS L. MOSER DIRECTOR, ENGINEERING, JOHNSON SPACE CENTER
V.	FLIGHT PREPARATION PROCESS	RICHARD H. KOHRS DEPUTY MANAGER, NATIONAL SPACE TRANSPORTATION SYSTEMS PROGRAM, JOHNSON SPACE CENTER

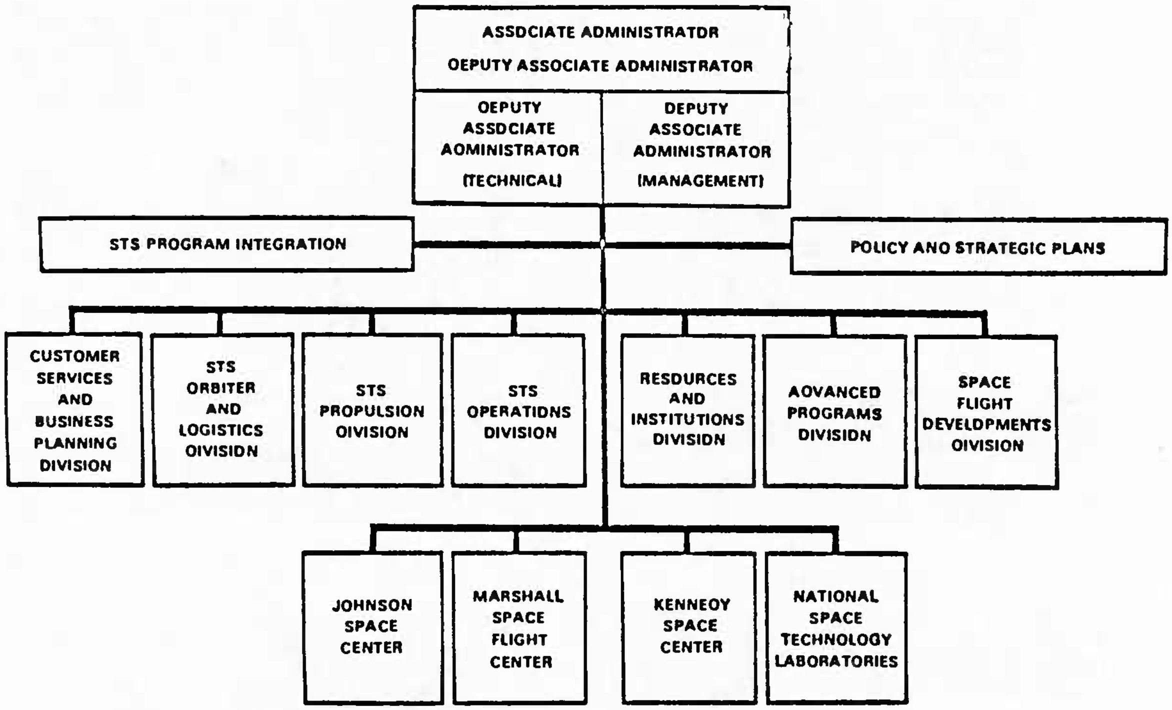
[Ref. 2/6-1]



ADMINISTRATOR A
DR. WILLIAM R. GRAHAM (ACT)
DEPUTY ADMINISTRATOR AD
DR. WILLIAM R. GRAHAM
GENERAL MANAGER AS
PHILIP E. CULBERTSON
CHIEF ENGINEER D
DR. MILTON A. SILVEIRA
ASSOCIATE ADMINISTRATOR FOR POLICY F
NORMAN TERRELL
ASSOCIATE DEPUTY ADMINISTRATOR AGB
ANN P. BRADLEY
CHIEF SCIENTIST P
DR. FRANK B. McDONALD
ASSISTANT ADMINISTRATOR FOR EQUAL OPPORTUNITY PROGRAMS U
DR. HARRIETT G. JENKINS
ASSISTANT ADMINISTRATOR FOR PROCUREMENT H
STUART J. EVANS
INSPECTOR GENERAL W
BILL D. COLVIN
ASSISTANT ADMINISTRATOR FOR LEGISLATIVE AFFAIRS C
JOHN F. MURPHY
NASA COMPTROLLER B
C. THOMAS NEWMAN
GENERAL COUNSEL G
JOHN E. O'BRIEN
ASSOCIATE ADMINISTRATOR FOR EXTERNAL RELATIONS L
THOMAS F. DeCAIR
ASSISTANT ADMINISTRATOR FOR COMMERCIAL PROGRAMS I
ISAAC T. GILLAM, IV
ASSOCIATE ADMINISTRATOR FOR SPACE SCIENCE AND APPLICATIONS E
DR. BURTON I. EDELSON
JET PROPULSION LABORATORY
GODDARD SPACE FLIGHT CENTER
ASSOCIATE ADMINISTRATOR FOR AERONAUTICS AND SPACE TECHNOLOGY R
DR. RAYMOND S. COLLADAY
AMES RESEARCH CENTER
LANGLEY RESEARCH CENTER
LEWIS RESEARCH CENTER
ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT M
JESSE W. MOORE
LYNDON B. JOHNSON SPACE CENTER
JOHN F. KENNEDY SPACE CENTER
GEORGE C. MARSHALL SPACE FLIGHT CENTER
NATIONAL SPACE TECHNOLOGY LABORATORIES
ASSOCIATE ADMINISTRATOR FOR SPACE STATION S
JOHN D. HODGE (ACT)
ASSOCIATE ADMINISTRATOR FOR SPACE TRACKING AND DATA SYSTEMS T
ROBERT O. ALLER
ASSOCIATE ADMINISTRATOR FOR MANAGEMENT N
C. ROBERT NYSMITH

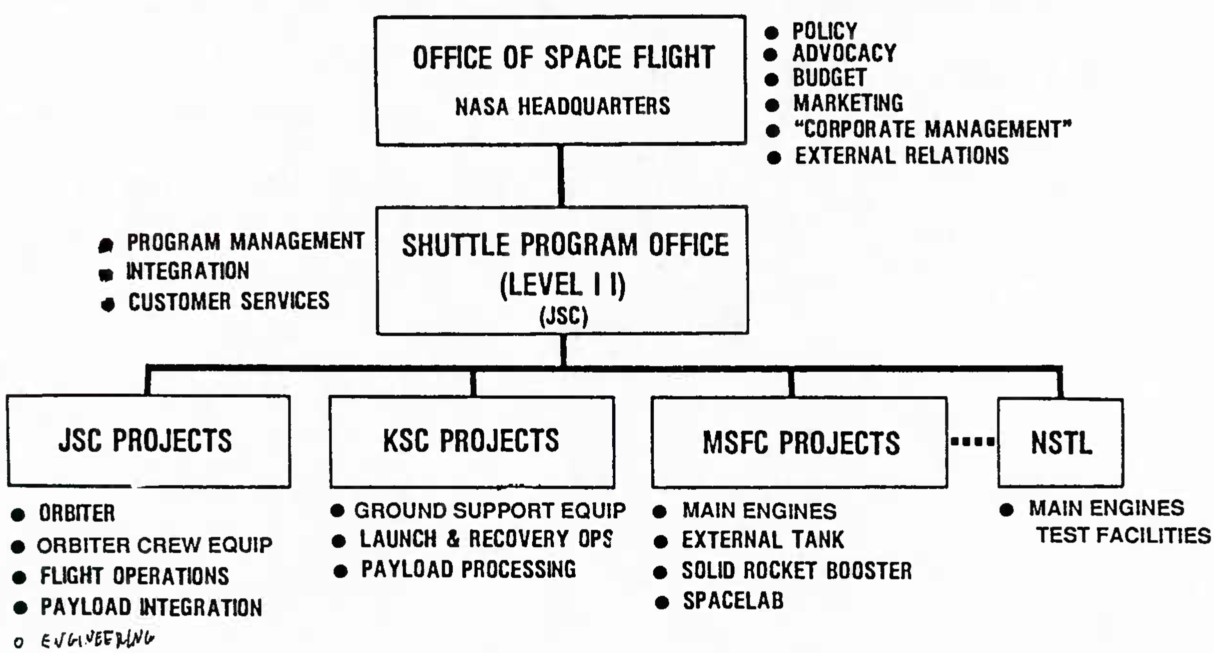
[Ref. 2/6-2]

OFFICE OF SPACE FLIGHT



[Ref. 2/6-3]

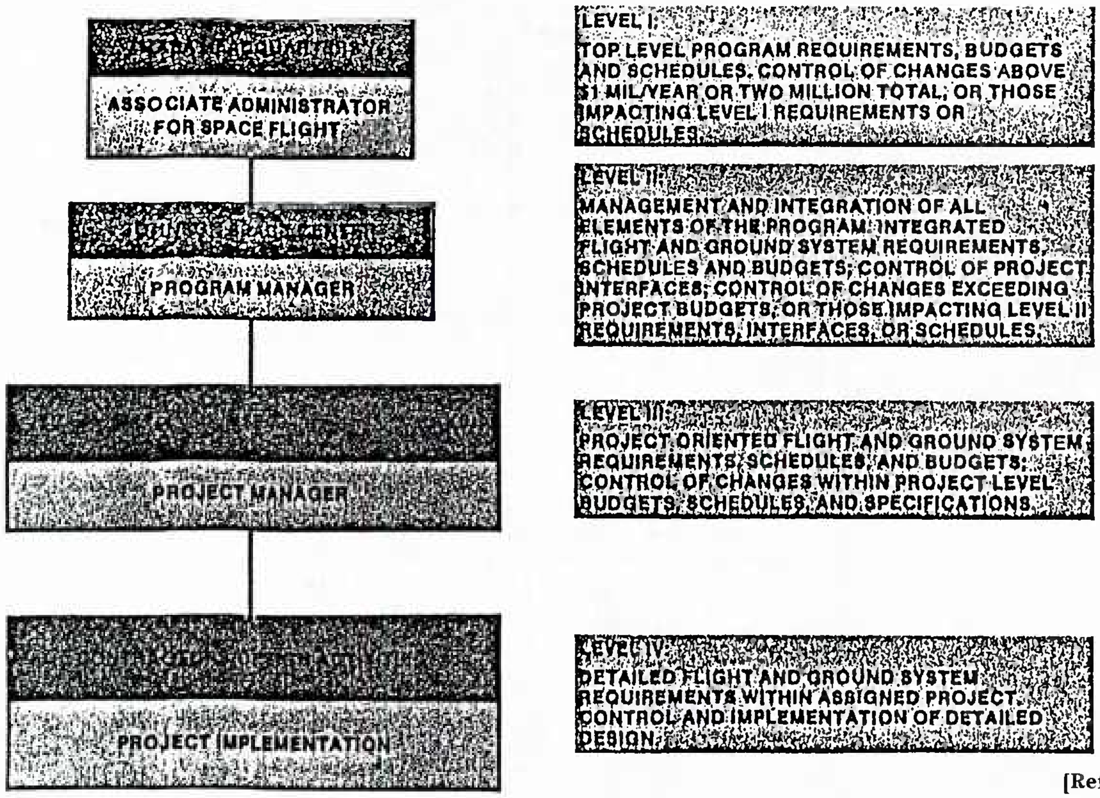
STS MANAGEMENT RESPONSIBILITIES



[Ref. 2/6-4]

NASA-S-83-00119B

NATIONAL STS PROGRAM
MANAGEMENT RELATIONSHIPS



[Ref. 2/6-5]

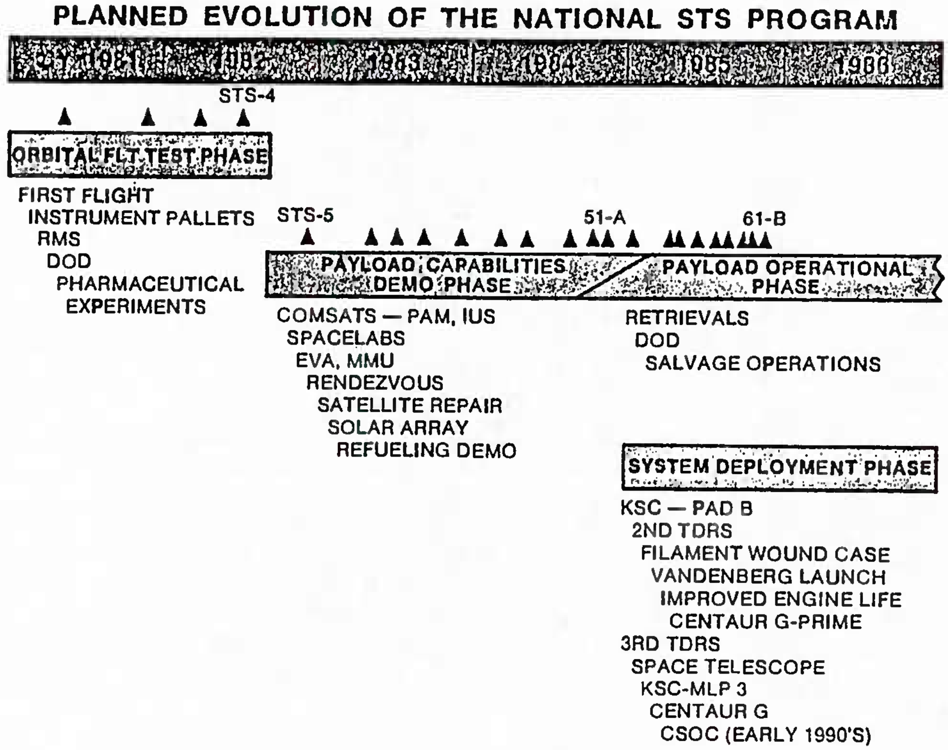
NASA National Aeronautics and Space Administration

Lyndon B. Johnson Space Center Houston, Texas 77058

NASA-S-84-05850E

DECEMBER 1985

PLANNED EVOLUTION OF THE NATIONAL STS PROGRAM



[Ref. 2/6-6]

NASA-S-84-05855

NATIONAL STS PROGRAM

ORBITAL FLIGHT TEST PHASE

STS-1
FIRST LAUNCH/LANDING •
FIRST FLIGHT OV-102

STS-2
OSTA-1 • RMS

STS-3
OSS-1 •
LANDING AT NORTHRUP STRIP

STS-4
DOD • CFES

[Ref. 2/6-7]

NASA National Aeronautics and Space Administration

Lyndon B. Johnson Space Center
Houston, Texas 77058

NASA-S-84-05854

NATIONAL STS PROGRAM

PAYLOAD CAPABILITIES DEMO PHASE

STS-5
SBS-C • TELESAT-E

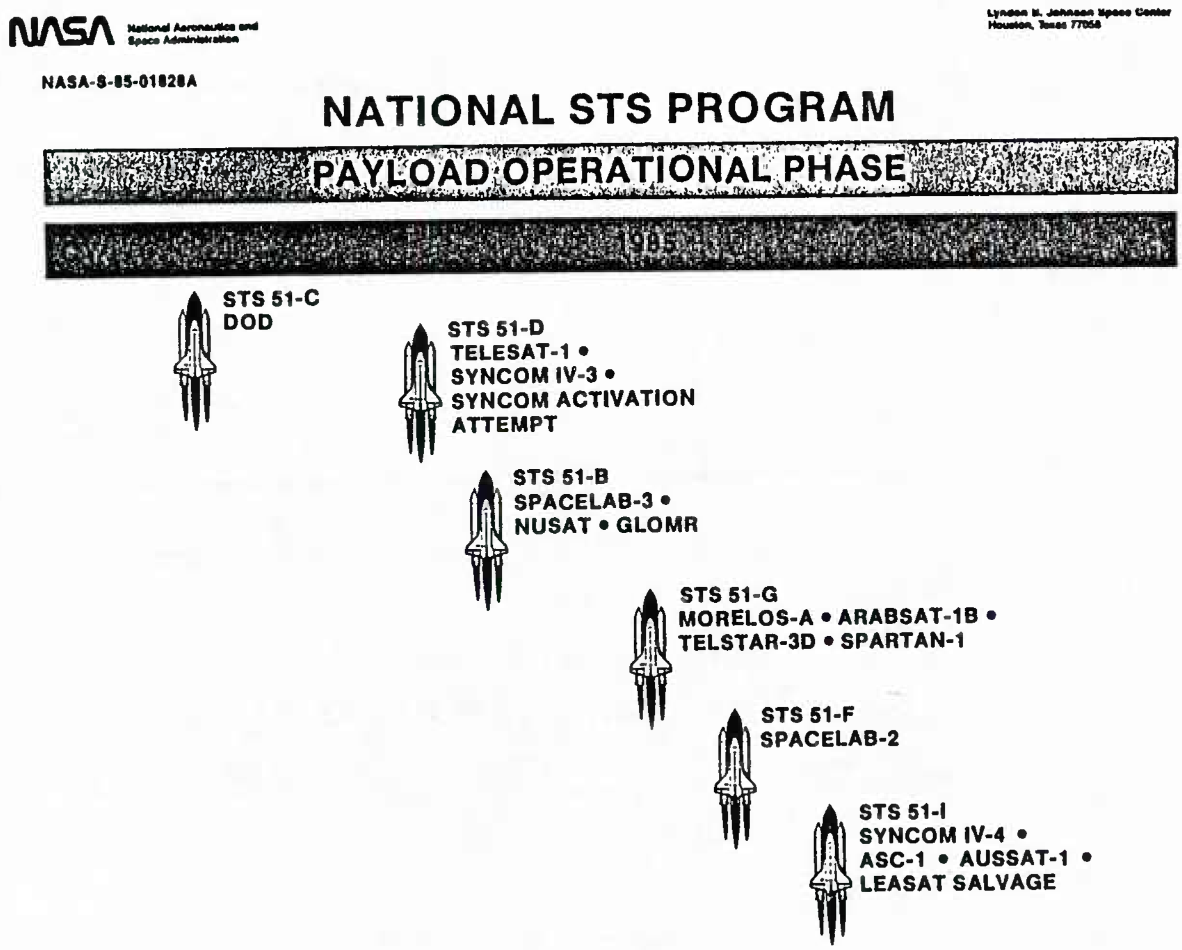
STS-6
TDRS-A • FIRST EVA • FIRST FLIGHT OV-099 •
FIRST LIGHT WEIGHT TANK

STS-7
PALAPA B-1 • SPAS-01 •
TELESAT-F • OSTA-2

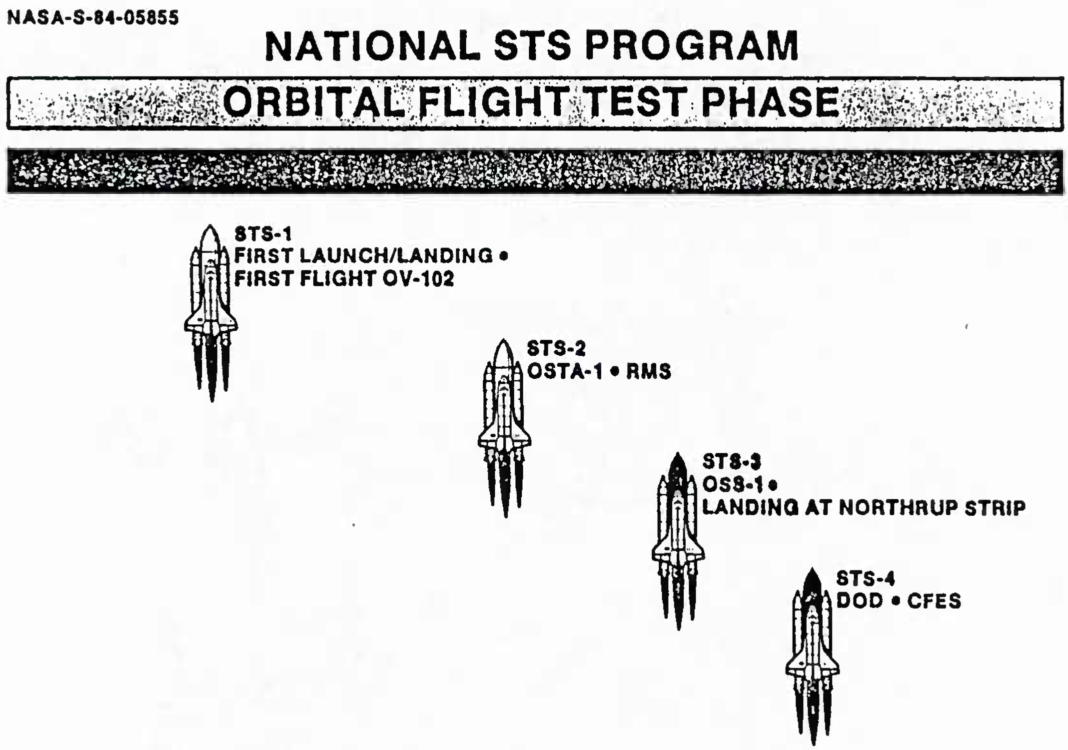
STS-8
INSAT 1-B • PFTA •
FIRST NIGHT LAUNCH/LANDING •
FIRST HIGH PERFORMANCE MOTOR

STS-9
SPACELAB 1

[Ref. 2/6-8]



[Ref. 2/6-9]
[Ref.2/6-10]



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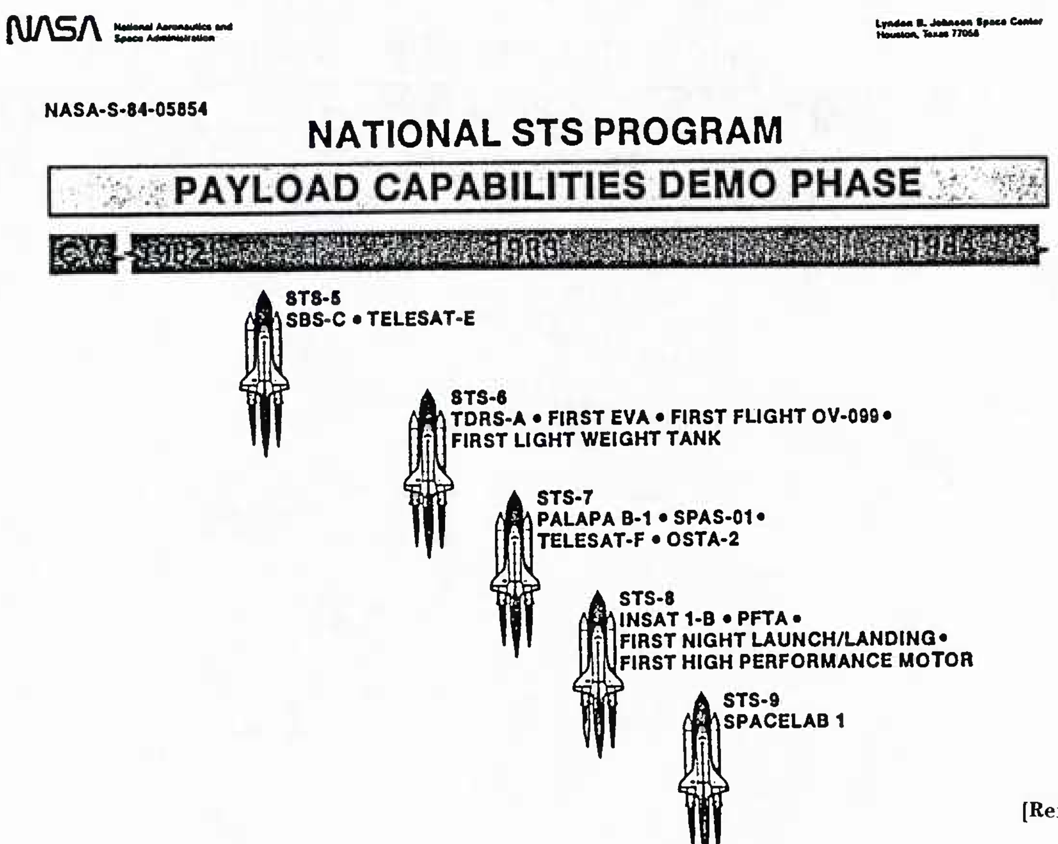

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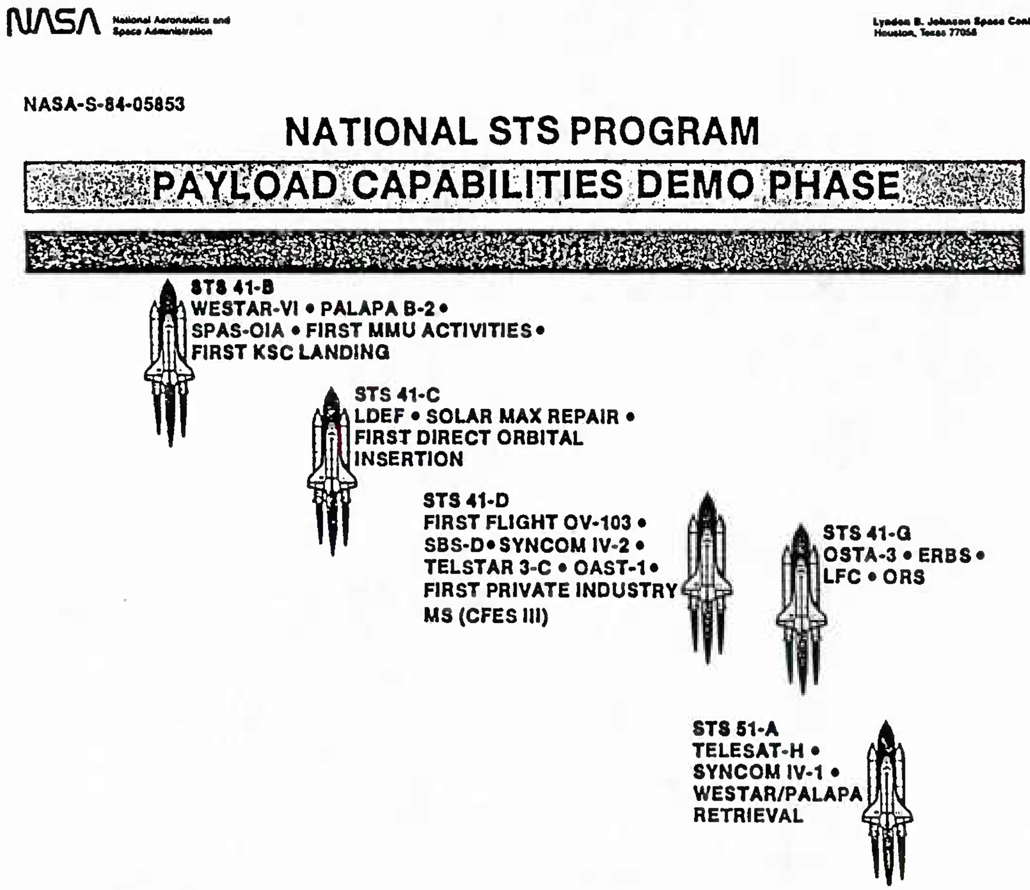

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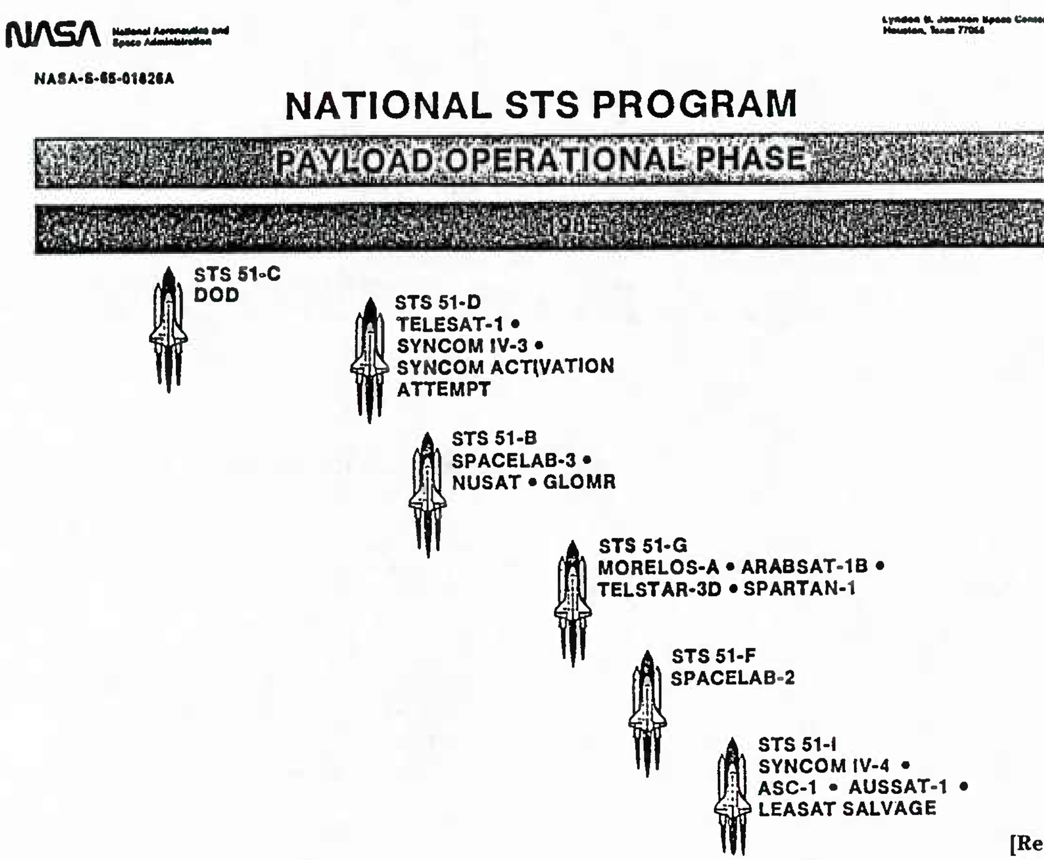

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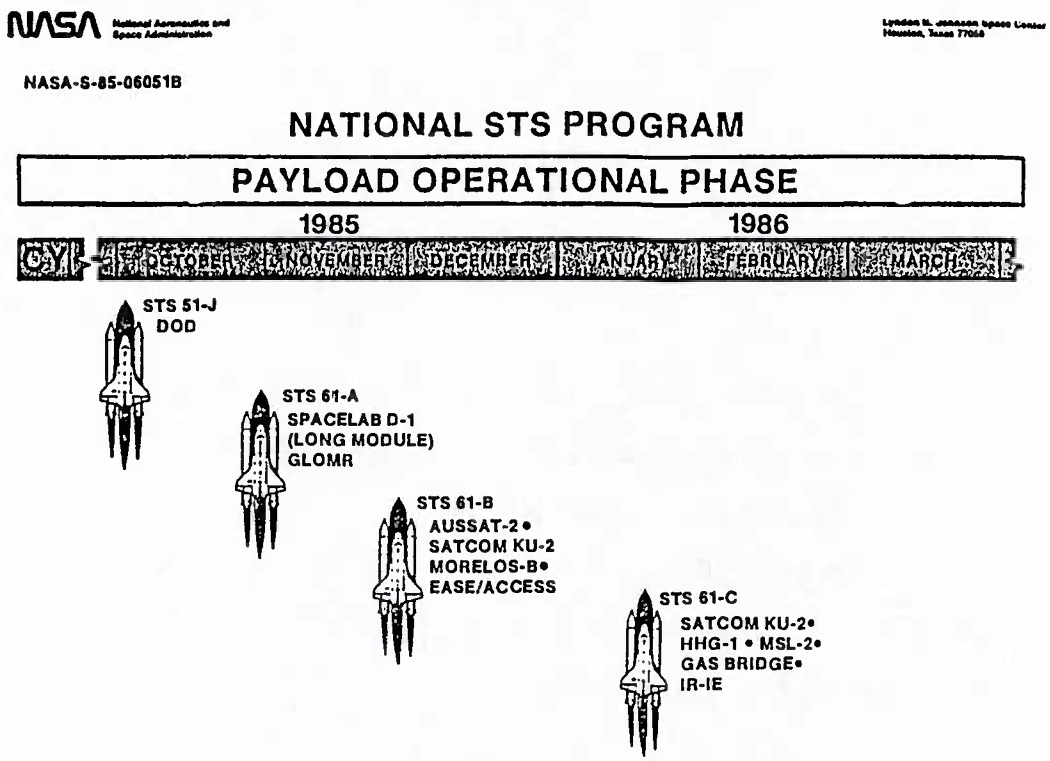
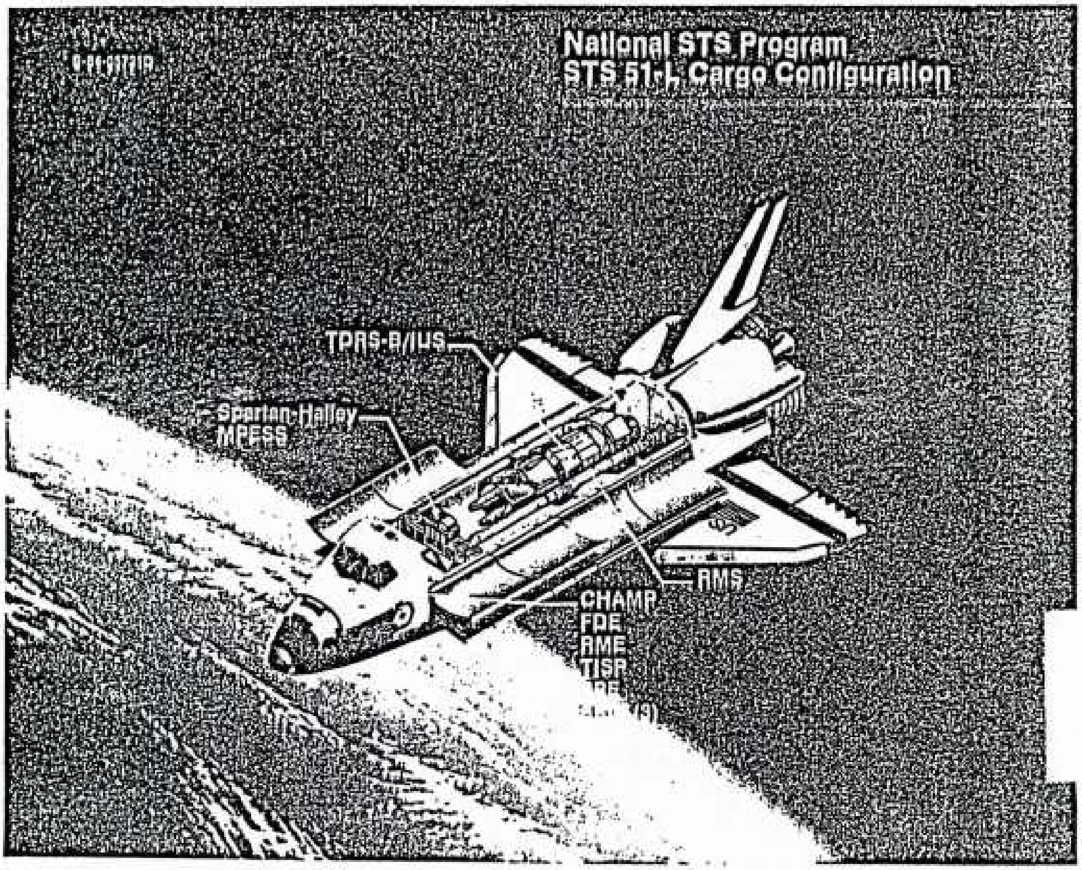

NASA
NASA-S-85-06051B
NATIONAL STS PROGRAM
PAYLOAD OPERATIONAL PHASE
1985
1986
CY
OCTOBER
NOVEMBER
DECEMBER
JANUARY
FEBRUARY
MARCH
STS 51-J
DOD
STS 61-A
SPACELAB D-1
(LONG MODULE)
GLOMR
STS 61-B
AUSSAT-2 •
SATCOM KU-2
MORELOS-B•
EASE/ACCESS
STS 61-C
SATCOM KU-2•
HHG-1 • MSL-2•
GAS BRIDGE•
IR-IE

[Ref. 2/6-15]

STS 51-L CARGO ELEMENTS

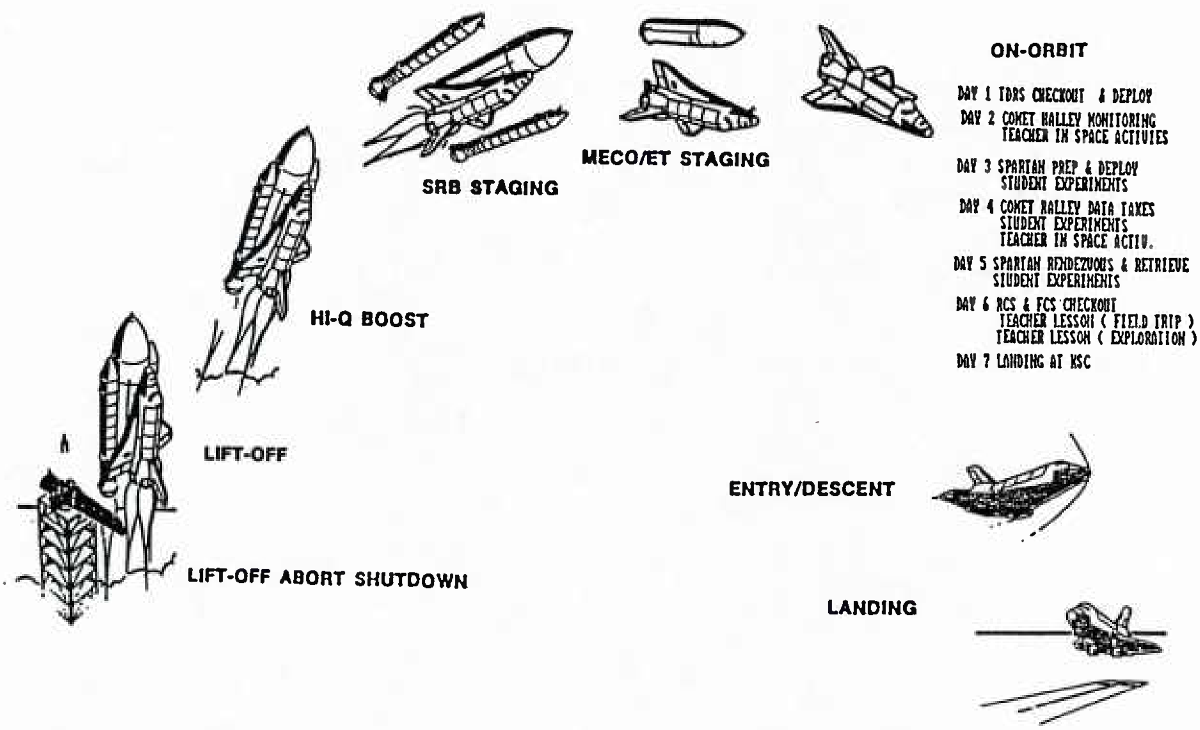
- TRACKING AND DATA RELAY SATELLITE-B/INERTIAL UPPER STAGE
- SPARTAN-HALLEY/MISSION PECULIAR SUPPORT STRUCTURE
- CREW COMPARTMENT
  - TISP - TEACHER IN SPACE PROGRAM
  - CHAMP - COMET HALLEY ACTIVE MONITORING PROGRAM
  - FDE - FLUID DYNAMICS EXPERIMENT
  - STUDENT EXPERIMENTS
  - RME - RADIATION MONITORING EXPERIMENT
  - PPE - PHASE PARTITIONING EXPERIMENT

[Ref. 2/6-16]



[Ref. 2/6-17]

STS 51L MISSION PROFILE



[Ref. 2/6-18]

STS 51-L MISSION DATA

0 LAUNCH DATE:	JANUARY 28, 1986
0 ORBITER:	OV-099 CHALLENGER
0 LIFTOFF:	9:38 A.M. EST
0 MAX LAUNCH WINDOW:	OPEN 9:38 A.M. EST CLOSE 12:38 P.M. EST
0 THROTTLE:	104%/104%
0 INCLINATION:	28:45 DEGREES
0 ORBITAL ALTITUDE:	153.5 NMI CIRCULAR

0 LANDING SITES:

o NOMINAL END-OF-MISSION:	KSC	WEATHER ALTERNATE: EDW
o ABORT-ONCE-AROUND:	EDWARDS	WEATHER ALTERNATE: KSC
o TRANSATLANTIC ABORT:	DAKAR	
o TRANSATLANTIC ABORT:	CASABLANCA	
o RTLS:	KSC	

0 FLIGHT DURATION - 6 DAYS PLUS 2 DAYS CONTINGENCY

[Ref. 2/6-19]

<u>LAUNCH DATE CHRONOLOGY</u>

0	JANUARY 25	- L-1 DAY REVIEW - 11 AM - MISSION MANAGEMENT TEAM (MMT) MTG - 9:30 PM	0 LAUNCH DAY WEATHER QUESTIONABLE 0 LAUNCH DELAYED TO JANUARY 27 DUE TO TO WEATHER NO-GO
0	JANUARY 26	- MMT MTG - 2:00 PM	0 LAUNCH CONFIRMED FOR 9:37 AM ON JANUARY 27
0	JANUARY 27	- LAUNCH ATTEMPT	0 INITIAL DELAYS: - HATCH MICROSWITCHES - HATCH GSE 0 LAUNCH DELAYED TO JANUARY 28 DUE TO HIGH CROSS WINDS (RTLS)
		- MMT MTG - 2:00 PM	0 CONCERN FOR COLD TEMPERATURES AFFECTING FACILITIES SUPPORTING ON TIME LAUNCH
0	JANUARY 28	- LAUNCH	0 DELAY IN COUNTDOWN DUE TO GSE PROBLEMS AND FROZEN PIPE 0 CONCERN FOR ICE ON SERVICE STRUCTURE & MOBILE LAUNCH PLATFORM (MLP)
		- TECHNICAL MTG - 8 AM REVIEW ICE CONCERN REGARDING ORBITER THERMAL PROTECTION SYSTEM	0 DECISION TO: - REALIGN INERTIAL MEASUREMENT UNIT - ICE TEAM INSPECTION AND CLEAN MLP DECK AT T-20 MIN

[Ref. 2/6-20]

STS-51L MISSION ACCIDENT
INITIAL ASSESSMENT

- ORBITER CHALLENGER LIFTED OFF AT 11:38 EST, JANUARY 28, 1986

CREW
FRANCIS R. SCOBEE
MICHAEL SMITH
ELLISON ONIZUKA
JUDITH RESNIK
GREGORY JARVIS
CHRISTA MCAULIFFE
RONALD MCNAIR

- LAUNCH HAD BEEN DELAYED FOR TWO HOURS DUE TO:
  - LAUNCH PROCESSING EQUIPMENT PROBLEMS DURING PROPELLANT LOADING
  - ICE INSPECTION OF LAUNCH COMPLEX AND ICE REMOVAL FROM MOBILE LAUNCH STRUCTURE RESULTING FROM BELOW FREEZING TEMPERATURES

- ASCENT APPEARED NORMAL FOR APPROXIMATELY THE FIRST 73 SECONDS THROUGH THE PROGRAMMED ROLL MANUEVER, MAIN ENGINE THROTTLE DOWN, MAXIMUM DYNAMIC PRESSURE AND MAIN ENGINE THROTTLEUP

- SHUTTLE VEHICLE APPEARED TO BE PERFORMING NORMALLY ON ALL SYSTEMS AT 104 PERCENT MAIN ENGINE THRUST AT APPROXIMATELY 1200 MPH AT 47,600 FEET ALTITUDE WHEN ALL TELEMETRY DATA INSTANTLY TERMINATED

- AT APPROXIMATELY THAT TIME SHUTTLE BREAK-UP WAS OBSERVED FROM THE GROUND

[Ref. 2/6-21]

IMMEDIATE ACTIONS TAKEN

- ALL MISSION DATA AND INFORMATION IMPOUNDED AT ALL SITES
  - AGENCY AND CENTER STS CONTINGENCY PLANS PUT INTO EFFECT

- INTERIM MISHAP INVESTIGATION BOARD FORMED
CHAIRMAN:	JESSE W. MOORE, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT
MEMBERS:	RICHARD SMITH, DIRECTOR, KENNEDY SPACE CENTER WILLIAM LUCAS, DIRECTOR, MARSHALL SPACE FLIGHT CENTER ARNOLD ALDRICH, MANAGER, NATIONAL STS PROGRAM, JOHNSON SPACE CENTER WALTER WILLIAMS, SPECIAL ASSISTANT TO NASA ADMINISTRATOR
ADDITIONAL MEMBERS:	ROBERT CRIPPEN, ASTRONAUT OFFICE, JOHNSON SPACE CENTER JOSEPH KERWIN, DIRECTOR, SPACE/LIFE SCIENCES, JOHNSON SPACE CENTER
EX-OFFICIO MEMBERS:	JOHN O'BRIEN, GENERAL COUNSEL MILTON SILVEIRA, CHIEF ENGINEER
EXECUTIVE SECRETARY:	JAMES C. HARRINGTON, DIRECTOR, STS PROGRAM INTEGRATION

- TEAMS FORMED TO TAKE IMMEDIATE ACTIONS

[Ref. 2/6-22]

TEAMS

1.	FLIGHT DATA, TRAJECTORY, AIR/END COMM.	DON PUDDY, JSC
2.	LAUNCH PAD, FACILITY, BEACH AREA	H. LAMBERTH, KSC
3.	PHOTOGRAPHY & TV	C. STEVENSON, KSC
4.	LEGAL	E. PARRY, KSC
5.	DATA ANALYSIS	R. KOHRS, JSC
6.	MFG. PEDIGREE/PROCESSING	
	MSFC PROJECTS--SRB, ET, SSME	J. LEE, MSFC
	ORBITER	R. COLONNA, JSC
	KSC PROCESSING	J. THOMAS, KSC
7.	LAUNCH OPERATIONS	R. SIECK, KSC
8.	CARGO	J. CONWAY, KSC
	TDRSS, IUS, SPARTAN	
9.	SECURITY	M. JONES, KSC
10.	RANGE SAFETY	COL. SINCLAIR, PAFB
11.	PUBLIC AFFAIRS	C. HOLLINSHEAD, KSC
12.	DOD MANAGEMENT SUPPORT	COL. SCHULTZ, USAF
13.	FLIGHT CREW	B. OVERMEYER, JSC
14.	MAIN PROPULSION SYSTEM	B. COBB, MSFC
15.	FLIGHT VEHICLE IMPOUNDMENT	E. KICKLIGHTER, KSC

ADDITIONAL TEAMS

16.	SALVAGE AND RECOVERY	E. O'CONNOR, PAFB
17.	WRECKAGE ANALYSIS	E. WEBER, KSC
18.	ANOMALY ANALYSIS INTEGRATION	R. KOHRS, JSC
19.	"DEVILS ADVOCATE" TEAM	TBD

[Ref. 2/6-23]

STATUS

- o DATA REVIEW AND ANALYSIS CONTINUES
  - DETAILED TIMELINE OF ALL EVENTS BEING DEVELOPED
- o PHOTO STUDY AND ENHANCEMENT IN PROCESS WITH CONCENTRATION ON RIGHT HAND SOLID ROCKET BOOSTER (SRB) PLUME AND RELATED AREAS
- o SALVAGE AND RECOVERY OPERATION PROCEEDING
  - EXTENSIVE U.S.COAST GUARD SEARCH
  - SURFACE RECOVERY RETURNS DIMINISHING
    - o APPROXIMATELY 5-10% OF ORBITER RECOVERED
    - o 12 TONS OF DEBRIS RECOVERED
  - UNDERWATER OPERATION FOCUSSED ON SELECTED LOCATIONS
    - o U.S. NAVY SALVAGE LEADING OPERATION
    - o PRIORITY ON RH SRB
- o WRECKAGE ANALYSIS/RE-ASSEMBLY PROCEEDING
  - NATIONAL TRANSPORTATION SAFETY BOARD LEADING OPERATION

[Ref. 2/6-24]

[NOT REPRODUCIBLE]

[Ref. 2/6-25]

[NOT REPRODUCIBLE]

[Ref. 2/6-26]

[NOT REPRODUCIBLE]

[Ref. 2/6-27]

ACTIVITIES CONTINUING

0 REALIGNMENT OF BOARD ACTIVITIES TO SUPPORT COMMISSION

0 "51L DATA AND DESIGN ANALYSIS TASK FORCE"

- ESTABLISHED BY NASA ADMINISTRATOR - FEBRUARY 5, 1986
- CHAIRMAN: JESSE MOORE, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT

0 DIRECTED TO CONTINUE

- ANALYSIS OF FACTS, CIRCUMSTANCES
- IDENTIFY DESIGN ISSUES SURROUNDING ACCIDENT

0 AUTHORIZED TO USE TECHNICAL/SCIENTIFIC RESOURCES

- WITHIN NASA
- THOSE AVAILABLE EXTERNALLY

[Ref. 2/6-28]

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SPACE SHUTTLE SYSTEMS, TESTIMONY OF ARNOLD D. ALDRICH, MANAGER, NATIONAL SPACE TRANSPORTATION SYSTEMS PROGRAM, JOHNSON SPACE CENTER

MR. ALDRICH: Chairman Rogers, members of the Commission, my name is Arnold Aldrich, and I am manager of the National Space Transportation Systems Program Office at the Johnson Space Center.

(Viewgraph). [Ref. 2/6-29]

MR. ALDRICH: I am going to describe for you a little bit about the program management again to show you where I fit in the structure that Jesse described, and then I will describe the STS system elements, some of the system element performance, and then some of the orbiter subsystems.

Following me, Dr. Lovingood will describe the propulsion elements responsible by the Marshall Space Flight Center, and Bob Sieck will describe the launch and landing facilities that make up other portions of the STS system.

(Viewgraph). [Ref. 2/6-30]

MR. ALDRICH: The next chart deals with the program management relationships. We just passed over this chart. Level 1 control of the program is done here in Washington under Jesse, Associate Administrator for

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Space Flight. They determine top level program requirements, budgets, schedules, policy for the agency on the Space Shuttle Program, and they deal with large budget items that would affect primary requirements in the overall program and the overall program schedules.

My office, as program manager at the Johnson Space Center, is management and integration of all program elements in support of the Level 1 organization. We do integrated flight system and ground system requirements, schedules and budgets, control of all project interfaces, control of changes exceeding program budgets of the different projects across the center, and those that impact overall STS program requirements, interfaces, and schedules.

Below that are the Level 3 projects at each center, and I will say more about those on the subsequent pages. Level 4 is defined on this chart by the specific contracts with industry that will be described for the fabrication, design, and provision of the flight hardware and the ground hardware that supports the STS program.

(Viewgraph). [Ref. 2/6-31]

MR. ALDRICH: The next chart says a little more about the concept of the program office at JSC for the STS program. This is what NASA calls the lead

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center concept. That is a relatively small staff at NASA Headquarters for policy, overall budget, and overall program direction. There is a large program office under myself at the Johnson

Space Center that is responsible for control and integration of all elements of the Space Shuttle System.

This work across the system is identified in the detailed work breakdown structure. It is supplied to all elements across the program, both government and contractor, for all activities and program management, the office manager's projects at the various centers, and at those centers those projects that manage the contractors that provide the actual hardware that we are talking about here today.

Integration of this total system is identified as a government role. However, we also have contractor support in those areas, and I will identify them, some of the major contractor activities on a subsequent chart.

Project managers at the centers are also in a line responsibility and report through their directors to Jesse in an institutional fashion as well as through this program chain which I am describing to you within the Level 2 program office, we have a system, a very careful and detailed

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documentation and control of all technical and management requirements for the program at all levels, and that will be discussed a little bit later in the day with some of the later briefings.

We also have very frequent communications nationwide within this program, and we use an extensive teleconferencing system, because travel is really impossible for the kind of day-to-day and continuous communications we use, and as was mentioned by Jesse, particularly in the last several years, we have very extensive involvement with the Department of Defense and the Air Force, both with their payloads and with the coming on line of the Vandenberg Launch Facility on the west coast.

(Viewgraph.) [Ref. 2/6-32]

MR. ALDRICH: The next chart describes the structure of the government-industry team. That falls under the National Space Transportation System. Again, the overall policy and direction is the government at NASA Headquarters. My role as the JSC Lead Center is for program planning and control, system and cargo integration of the total system, operations and mission integration for the preparation and the flight of the Shuttle system. In executing those responsibilities—I am sorry for these acronyms; we tried a

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chart with them spelled out, and they were very voluminous, and I have their names on a subsequent chart—Rockwell International Space Division is in charge of system and cargo integration and engineering in support of the STS program, and at the Johnson Space Center, also Rockwell International, the Rockwell support operations contract provides Shuttle engineering and operation support.

The Level 3 NASA projects of the National Space Transportation System, the orbiter, at JSC. Rockwell Space Division, Downey, California, is Prime. Space Shuttle Main Engines are the responsibility of the Marshall Space Flight Center, Rockwell International Rocketdyne Division is Prime. External tank, Marshall Space Flight Center, Martin Marietta Corporation, Michoud, Louisiana, near New Orleans, is a Prime contractor.

Solid rocket boosters, United Space Booster Production Company is prime. Solid rocket motors are fabricated and refurbished by Morton-Thiokol in Brigham City, Utah, in support of the Marshall Center.

The launch and landing facilities were developed in support of the Kennedy Space Center by a number of contractors, and in the last two years the Lockheed services operations contract is a consolidated contract that has those responsibilities in support of KSC.

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The mission support at the Johnson Space Center for mission flight support, flight preparation, and crew training, Rockwell space operations contract is also a consolidated contract and that has recently come into being at the Johnson Space Center for consolidated contractor operations there.

DR. FEYNMAN: Could you tell me the difference between the solid rocket booster and a solid rocket motor?

MR. ALDRICH: Yes, sir. The solid rocket motor is the elements—well, I probably should let Dr. Lovingood give you that in detail, but basically the solid rocket motor are the elements with the propulsive grain in them, and the rest of the systems, the recovery systems, the gimballing systems, the electronics together make up the solid rocket booster as a total system.

(Viewgraph.) [Ref. 2/6-33]

MR. ALDRICH: This chart, in fact, deals with the elements of the STS program, the orbiter which I will be discussing in a few minutes, flight software, which goes in the orbiter but which controls all of the elements of the Space Shuttle System during the various phases of flight and even during ground checkout.

Main engine external tank and solid rocket

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boosters. Flight crew equipment; have a significant activity in the program for spacesuits, for man maneuvering units which Jesse discussed, also for other crew equipments within the cockpit in support of the flight crew.

A number of cargo elements, and I will discuss some of those later, also for cargo integration of the various payloads that come from various places nationally and even internationally, integrating them into flyable cargoes to make them part of the National Transportation System. Launch and landing facilities and upper stages, and I believe Jesse described and gave you the names for each of these.

(Viewgraph.) [Ref. 2/6-34]

MR. ALDRICH: The next chart shows these pictorially. Again, it shows the solid boosters we just mentioned, the tank. The Space Shuttle main engines are shown behind the orbiter. The orbiter itself, there is cargo here. An element of the Space Transportation System is the Space Lab, which is provided by the European Space Agency and has been integrated by the Marshall Space Flight Center into the Space Shuttle.

The upper stages, the IUS, the Centaur we have talked about. The TOS is a proposed extension to that developed by private industry, and does not exist today

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in the program. The three versions of the payload assist module each with additional capability depending upon the size and performance required for the given satellite or payload which is going to use it.

In addition, the launch and landing facilities at Kennedy are part of the Transportation System. The Control Center at Houston and later the Control Center to be built by the Air Force in Colorado Springs will be part of the National Transportation System. Mission planning and training activities at the Johnson Space Flight Center and other places around the nation, and a wide range of ground support facilities which I will discuss on the next chart.

(Viewgraph.) [Ref. 2/6-35]

MR. ALDRICH: The next chart discusses ground support. I have mentioned on the launchpad the Kennedy support facilities, so the Shuttle is in direct communication with the Mission Control Center in Houston.

(Viewgraph.) [Ref. 2/6-36]

MR. ALDRICH: Also in support of the Control Center in Houston in the training and mission preparation phase are the simulation facilities and astronaut training facilities at Johnson Space Flight Center, and the payload operation control centers exist both within the

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Mission Control Center in Houston and they exist at remote locations such as the Goddard Space Flight Center, at the JPL at the Marshall Space Flight Center, and in the future likely many other places.

In our past we have used a wide range of ground support facilities around the world for communications and tracking with the orbiter. We are currently evolving a Tracking and Data Relay System that Jesse talked about which is used to relay large amounts of voice telemetry and television information from the orbiter to the ground and communications and ground control to the orbiter.

Today there is one TDRS satellite in orbit that covers about half of each orbit of the earth, and we were in the process of deploying the second and third satellites in that system. The large antenna is the ground TDRS station which is located in New Mexico, which is the focal point for all of the TRDS satellites and relays the data to the various stations around the National Complex.

(Viewgraph.) [Ref. 2/6-37]

MR. ALDRICH: The next chart shows the physical national location of these different facilities. We are here at NASA Headquarters, at the Marshall Center. We have already discussed the ET, SRB,

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and the SSME. They also provide the interface with the Department of Defense for providing the IUS upper stage and with the European Space Agency for the Space Lab.

Kennedy Space Center provides launch and landing operations and facilities. National Space Transportation Laboratory at Mississippi provides the main engine test firing facility. Dr. Lovingood will mention that in some detail. The Michoud assembly plant for the external tank is just east of New Orleans.

At the Johnson Space Center we have my program office that I am speaking to you today in support of. We also have the orbiter project focused at the Johnson Space Center and the mission control, mission design development and crew training activities are at the Johnson Space Center.

At White Sands we have an alternate landing site. We also have the TDRS ground station which works with the TDRS satellites. We also have an extension of the Johnson Space Center for hazardous orbital engine testing in California. The Downey Industrial Plant is responsible for orbiter design, development, manufacturing. In Canoga Park, the Rocketdyne Division of Rockwell International is responsible for developing the Space Shuttle main engines, design, develop, and manufacturing

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and tests. At Vandenberg Air Force Base we have launch and landing facilities on the verge of being operational. They have been developed and put in place and they are undergoing final testing at this time.

In the desert in California at Palmdale we do the detailed final assembly of the orbiter vehicles, and we also have the west coast primary landing site at Edwards Air Force Base. Brigham City, Utah, solid rocket motor, Morton-Thiokol does the construction, the pouring of the solid rocket motors, and the refurbishment of the used equipment, and reservicing for downstream flights.

In the future we will have at Colorado Springs the North American Aerospace Defense Command Control Facility. They are in support of the Air Force control of Shuttle missions.

I tried to go back and provide for you a basis of how the Shuttle program came to be. The Apollo program was flown in the 1960s, late 1960s and 1970s, and in the period 1967 to 1972 there was significant discussion nationally within and without NASA regarding the follow-on to the Apollo hardware.

The Apollo was scheduled to fly the sequence of missions that we have come to know the Apollo program did, also the Skylab missions and the Apollo-Soyuz test

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project, and during this period between 1967 and 1972 there were a series of national and NASA task forces to look at the future generation vehicles that would be flown.

At that time there were extensive discussions and phase-in A and B studies for both the space station and for a reusable service vehicle to go back and forth from a space station, and at the end of that period in the 1972 time frame the basic characteristics of the Shuttle had been determined and defined coming out of those deliberations.

This chart then picks up with the National STS program development starting with the characteristics that were defined coming out of those series of activities which I am sure you will want to know more about, and which are very involved, and will require some research to put in the perspective you might like to see it from.

Anyway, in the 1972 time frame and slightly before that the first project to be begun was the Space Shuttle main engine project. It was started with a set of design characteristics, and not long after that the orbiter project, external tank project, and solid rocket booster projects were started in support of the total design concept for the National Space Transportation

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System.

The engineering, design, development testing, mockup, and all activities of hardware development proceeded through the early seventies, and the first orbiter was rolled out in 1976. That was Orbiter 101, the Enterprise. It was a flightworthy orbiter for aero flight, but it was not built for orbital flight, and in fact it was used at Edwards Air Force Base for a series of flight tests on the back of the carrier aircraft initially and then for a series of free flights in mid-to-late 1977 to demonstrate the approach and landing characteristics of the Shuttle and orbiter system.

(Viewgraph.) [Ref. 2/6-38]

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In 1979 Orbiter 102, Columbia, was delivered to the Kennedy Space Center. It was brought into the Kennedy facilities which had been developed and brought along during this timeframe, and processing there continued to the first manned orbital flight in 1981.

Since that time we have had a series of test flights. STS-1 through 4 were called test flights, and in fact, the Columbia vehicle was extensively instrumented, and it was configured uniquely in a way I will describe shortly as a test flight vehicle. It has since been refurbished, and since that time other orbiters have flown a series of operational flights that Jesse Moore described to you.

The next chart—

(Viewgraph.) [Ref. 2/6-39]

MR. ALDRICH: —describes the characteristics of the Space Shuttle System at a broad level. I have deleted the solid rocket booster and external tank because they are covered more precisely and in more detail on the presentations of Dr. Lovingood.

The overall length of the Space Shuttle System is 184 feet. It is 76 feet wide if you look at it as it is shown in this chart. It has a capability for payload weight of 65,000 pounds due east out of the Kennedy Launch Center, 32,000 pounds in a polar trajectory out of

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the east coast. The 104 is a southerly launch azimuth out of the west coast launch facility, and it represents the mission which would correspond to the maximum performance out of that location.

The system weights represent the weights of the total vehicle on those inclinations on those max payload flights, 4.49 million pounds at liftoff and 4.49 million pounds at liftoff from the west coast.

In a minute I will come back to this chart. I would like to talk to you for a minute, first, about the Space Shuttle stack before I talk in detail about the orbiter vehicle.

I am going to use this mike over here and talk a minute to the model. In fact, I could come and talk to your model if that would be preferable.

We have defined several times in this briefing the characteristics of the Space Shuttle flight system and used the names of each of these. I would like to point out how they join together. The solid rockets are each joined forward and aft to the external tank. They are not connected to the orbiter. When the vehicle is stacked on the launch pad, the only part of the system that is load-carrying on the launch pad is the base of the solid rocket motors.

They are first mounted, the external tank is

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put between them and connected here. Then the orbiter is mounted to the external tank, two places in the back and one place forward, and those carry all the structural loads for the entire system at liftoff and through the ascent phase of flight. Also connected to the orbiter, under the orbiter wing, are two large propellant lines, 17 inches in diameter. The one on the port side carries liquid hydrogen from the hydrogen tank in the back part of the external tank. The one on the right side carries liquid oxygen from the oxygen tank at the forward end, inside the external tank.

You asked several questions regarding abort profiles, and abort profiles for this vehicle are complex and complicated. I would like to try to outline for you a little bit about the way the vehicle can fly and the way it can separate.

At liftoff, as Jesse stated, we first light the Space Shuttle main engines, three engines in the back of the orbiter, using fuel from the tank, oxygen and hydrogen from the tank. They are allowed to run until they come up to full thrust, a little greater than five seconds, and a large amount of ground complex and the onboard orbiter computing complex checks a large number of details and parameters about the main engines to be sure that everything is proper—that the main engines

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are performing right, the tank is performing right.

If all of those checks automatically pass, the solid rocket boosters are ignited and the release mechanisms, the pyrotechnics that release the solids at the base are released, and the Shuttle System rises.

CHAIRMAN ROGERS: And that takes six seconds?

MR. ALDRICH: That takes roughly six seconds.

There is an exact time line, and we will be presenting that to you in detail.

Once the Shuttle System starts off the launch pad, there is no capability in the system to separate these rockets until they reach burnout. They will burn for two minutes and eight or nine seconds, and the system must stay together. There is not a capability built into the vehicle that would allow these to separate. There is a capability available to the flight crew to separate at this interface the orbiter from the tank, but that is thought to be unacceptable during the first stage when the booster rockets are on and thrusting. So essentially the first two minutes and a little more of flight, the stack is intended and designed to stay together, and it must stay together to fly successfully.

MR. HOTZ: Mr. Aldrich, why is it unacceptable to separate the orbiter at that stage?

MR. ALDRICH: It is unacceptable because of

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the separation dynamics and the rupture of the propellant lines. You cannot perform the kind of a clean separation required for safety in the proximity of these vehicles at the velocities and the thrust levels they are undergoing, the atmosphere they are flying through. In that regime, it is the design characteristic of the total system.

MR. HOTZ: Do you mean you would have raw fuel spilling out?

MR. ALDRICH: Yes, and you would have contact between the various elements, particularly the orbiter wings and the back part of the orbiter, and it is thought to be unsurvivable.

VICE CHAIRMAN ARMSTRONG: It is physically possible to do that, but it has been proven that it can't be safe?

MR. ALDRICH: All analysis indicates there is no likelihood of it being successful.

MR. RUMMEL: Mr. Aldrich, in what manner do the boosters separate? Are there explosive bolts or what?

MR. ALDRICH: Yes, at two minutes and eight seconds the thrust tailoff is sensed in the orbiter in its computer, and at that time a time sequence to release the booster is set up, and signals are sent from the

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orbiter to the solid rockets to fire pyrotechnics fore and aft to cause the rockets to separate. There are separation motors in the forward end of the rockets to pull them away in a correct dynamic sense.

CHAIRMAN ROGERS: During the two-minute period, is it possible to abort through the orbiter?

MR. ALDRICH: I am now going to, if you will let me, and I don't have a series of charts on this because of how complicated they are.

CHAIRMAN ROGERS: I didn't mean to interrupt. Go ahead.

MR. ALDRICH: You can abort, as Sally asked previously, for certain conditions. You can start an abort, but the vehicle won't do anything yet, and the intended aborts are built around failures in the main engine system, the liquid propellant systems and their controls. If you have

a failure of a main engine, it is well detected by the crew and by the ground support, and you can call for a return-to-launch-site abort. That would be logged in the computer, the computer would be set up to execute it, but everything waits until the solids take you to altitude. At that time the solids will separate in the sequence I described, and then the vehicle flies downrange some 400 miles, maybe 10 to 15 additional minutes, while all of the tank propellant is

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expelled through these engines.

As a precursor to setting up the conditions for this return-to-launch-site abort to be successful, towards the end of that burn downrange, using the propellants and the thrust of the main engines, the vehicle turns and actually points heads up back towards Florida. When the tank is essentially depleted, automatic signals are sent to close off the propellant lines and to separate the orbiter, and the orbiter then does a similar approach to the one we are familiar with with orbit back to the Kennedy Space Center for approach and landing.

DR. WALKER: So the propellant is expelled but not burned?

MR. ALDRICH: No, it is burned. You burn the system on two engines all the way downrange until it is gone, and then you turn around and come back because you don't have enough to burn to orbit. That is the return-to-launch-site abort, and it applies during the first 240 seconds of—no, 240 is not right. It is longer than that—the first four minutes, either before or after separation you can set that abort up, but it will occur after the solids separate, and if you have a main engine anomaly after the solids separate, at that time you can start the RTLS, and it will go through

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that same sequence and come back.

DR. RIDE: And you can also only do an RTLS if you have lost just one main engine. So if you lose all three main engines, RTLS isn't a viable abort mode.

MR. ALDRICH: Once you get through the four minutes, there's a period where you now don't have the energy conditions right to come back, and you have a forward abort, and Jesse mentioned the sites in Spain and on the coast of Africa. We have what is called a trans-Atlantic abort, and where you can use a very similar sequence to the one I just described. You still separate the solids, you still burn all the propellant out of the tanks, but you fly across and land across the ocean.

MR. HOTZ: Mr. Aldrich, could you just recapitulate just a bit here? Is what you are telling us that for the first two minutes of flight, until the solids separate, there is no practical abort mode?

MR. ALDRICH: Yes, sir.

MR. HOTZ: Thank you.

MR. ALDRICH: A trans-Atlantic abort can cover a range of just a few seconds up to about a minute in the middle where the across-the-ocean sites are effective, and then you reach this abort once-around capability where you go all the way around and land in

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California or back to Kennedy by going around the earth. And finally, you have abort-to-orbit where you have enough propulsion to make orbit but not enough to achieve the exact orbital parameters that you desire. That is the way that the abort profiles are executed.

There are many, many nuances of crew procedure and different conditions and combinations of sequences of failures that make it much more complicated than I have described it.

CHAIRMAN ROGERS: I assume that any abort procedure requires a human decision; it is not done by machines?

MR. ALDRICH: That is, in fact, exactly correct, and that was discussed at length in the design phase of the program, and no scenario for automatic abort could be found to be as reliable as having the human interact.

I would also mention Columbia and the first four flights, in fact, the first five flights because Columbia flew one flight beyond the test phase. Both Columbia and Enterprise on the drop tests in the desert in California had only two crew members, and they had ejection seats in the pilot and commander seats, and those seats would be fired again by the crew and not automatically, but they would blow hatches out of the

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top of the crew cabin and eject the crewmen out for parachute recovery.

Those were taken out of Columbia after flight 5. They have not been in any of the other orbiters except for Enterprise.

CHAIRMAN ROGERS: So I assume, then, that in the event of any accident that the safety of the crew depends upon the safety of the orbiter? I mean, there is no mechanism for using parachutes or any other escape mechanism?

MR. ALDRICH: These aborts that we are describing, RTLS, trans-Atlantic abort or trans-Pacific which are being developed, and the abort-once-around are all called intact aborts. They imply the survival of the orbiter, and an acceptable approach to one of our planned landing fields.

I believe I have described the characteristics of the Shuttle flight system in that discussion of aborts and how they go together and how they separate. Now let me describe—and if I could have the picture go one more chart, and we will look at the picture of the orbiter fleet.

(Viewgraph.) [Ref. 2/6-40]

MR. ALDRICH: This was the orbiter fleet up until last week, and there are some differences between

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these four vehicles. Again, Jesse named them for you. Columbia was the first vehicle. I have described for you that it was a test vehicle, that it had ejection seats. It also had a very extensive amount of instrumentation on board. It had over 1,000 pounds of instrumentation racks in the cargo bay to take data on the environment and the performance of the orbiter during those first four flights, and that data has been widely used in rounding out the understanding of the Space Shuttle System. Columbia is also the heaviest orbiter. Columbia and Challenger were made, were manufactured earlier in the program than Atlantis and Discovery. There has been a weight reduction program that has allowed us to take approximately 5,000 pounds out of the Atlantis and Discovery vehicles that reside in Columbia, and 3,000 to 4,000 that did reside in Challenger.

During the last two years since STS-5, Columbia has been back at the manufacturing site in Palmdale, and it has been retrofitted to be of the same configuration as the other flight orbiters functionally. However, there is one additional difference between these four orbiters that you see here. Columbia and Challenger have an external thermal protection system that allowed them only to fly

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trajectories out of the east coast. The trajectories out of the west coast have a higher heating entry profile, require additional TPS externally, and Columbia and Challenger are not designed

for that. They could be retrofitted for it, but it has not been determined to be a requirement of the program.

Atlantis and Discovery have TPS systems on board where they can be flown from either launch site, and of course, the plan has been to deliver Discovery initially for west coast launches.

Now, if I could go back a little bit to the chart that has the characteristics of the orbiter.

(Viewgraph.) [Ref. 2/6-41]

MR. ALDRICH: The orbiter was conceived, as I said, in the 1972 timeframe. It is about the size of a DC-9 aircraft. It has a fairly standard aircraft aluminum skin and stringer design, and there is not a lot of unique technology in the airframe of it. I will describe a little more about the airframe components in a minute, but the thing that makes it different for orbital space flight is primarily the Thermal Protection System, and that is, in general, added externally after the orbiter is built as a flight vehicle.

It is 122 feet long. It has a wing span of 78 feet. On its wheels, it is 57 feet high. Jesse pointed

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out the payload bay is 15 feet by 60 feet. On an entry profile, it can come down the azimuth it is flying down. It can also fly cross range 1100 nautical miles in either direction to achieve a landing at a selected landing site.

Attached to it from the main engine project are three main engines, 470,000 pounds thrust each. On board the back of the orbiter also are OMS—stands for Orbital Maneuvering System—engines, 6,000 pounds each, two of them. I will show you where they are in a minute. And then for smaller corrections on orbit, for all the attitude control, there is a reaction control system—and I will describe that for you again in a minute—38 engines mounted at various places on the orbiter, and each of those engines has 870 pounds of thrust; 6 vernier engines for precise control, 25,000 pounds of thrust each.

The weight of the orbiter inert is 162,000 pounds, and that varies from orbiter to orbiter because, as I said, they have a range of difference of about 5,000 pounds dry weight.

At landing, without a payload but loaded with all of the consumables, that residual and all of the crew equipment and all of the cargo support equipment in orbit is about 175,000 pounds. And with the cargo we bring back,

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we have generally been landing cargoes, 205,000 to 215,000 pounds at touchdown from the missions that Jesse described.

(Viewgraph.) [Ref. 2/6-42]

MR. ALDRICH: If I could go to the next chart, and it is a complicated chart regarding various orbiter subsystems, and perhaps I will come back to the model in a minute, or for a minute.

The engine systems that I described on board, of course, you know about the main engines. We talked about them all day. The OMS engines are these little black engines, and there is one on each side. They do orbital attitude changes of the orbiter. The reaction control engines, the 38 large and 6 small, are mounted on this device that sticks back from this pod which I will describe in a minute about its contents, and in a bay that is forward on the orbiter nose, and these black marks here are intended to be the ports that these engines fire out. In fact, there's 14 big engines and 2 small engines in front, 12 big engines and 2 small on each side, to compose the reaction control system.

In terms of the structure, this is what we call the forward fuselage. Inside of that is the crew cabin. This is the orbiter mid-body, the orbiter wings. Attached to the wings are the elevon

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aerosurfaces inboard on both sides and outboard on both sides for control during the approach and landing phases. They also are used for load relief during the ascent phase and they actually gimbal in conjunction with the main engines to provide balanced loading of the stack.

Down under the main engines is a device called a body flap. And it is an aerodynamic system that is trimmed in conjunction with the elevons for proper angle on approach and landing. The vertical stabilizer points upward, and it has a big aerosurface on the back. It is both a rudder and a speed brake. It swings in one direction for rudder control and opens in both directions for speed brake on landing approach and roll-out.

This is called the aft fuselage of the vehicle, and there are some major engine systems in there, and I will show you them briefly on a subsequent chart. This is the payload bay, of course. Most of the orbiter, as I said, is aluminum skin and stringer. The payload bay doors are graphite/epoxy. The pods here that contain these engine systems are graphite/epoxy, but all of the vehicle is covered with some form of TPS to protect either the aluminum or the graphite.

Payload bay doors have inside them devices

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that move with the doors that are cooling radiators when the orbiter is on orbit. It has a large thermal load to release, and it is released through these radiators which are pointed, in general, towards dark space. Although they are not specifically pointed, they see enough dark space to provide total cooling for the orbiter and for the cargoes we fly.

This is the remote arm from Canada that was discussed earlier. We only have one, on the port side. The orbiter design would pick up one on the starboard side, but we have not put that implementation in place in the program to date.

Viewing windows forward and to the side. Also we have viewing windows on the top for alignment sightings, and there are viewing windows in the back of the bulkhead, forward bulkhead, for viewing the cargo. And as you have seen from our flights, we have TV cameras mounted in the four corners of the payload bay, on the join to the RMS, and on the tip of the RMS.

I will describe the thermal protection system in a little more detail on a subsequent chart.

On the underside of the orbiter we have wheel wells. We have two main landing gear on the outboard aft side of the mid fuselage, and we have a forward nose landing gear in a wheel well under this forward RCS system, at

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the forward end of the vehicle. Other unique physical characteristics. All of the vehicle has a soft exterior insulation that we talked about with respect to the ice and the rain, except for the leading edges. This is a reinforced carbon-carbon nose cap, solid, and the leading edges of the wing are reinforced carbon-carbon and solid.

And that is the orbiter vehicle. Let me show you where some of the systems are.

The orbiter is a very complicated system, and it would take a long time to describe all of it in detail, but the way I selected some charts for this was to show you the basic structural elements and then to show you the systems particularly on board that have energy stored in them since stored energy is one of the things that could relate to what we have seen in the films and know about this incident.

(Viewgraph.) [Ref. 2/6-43]

MR. ALDRICH: The next chart shows the basic orbiter airframe as it is assembled. The crew compartment is shown on the upper left, and it is a welded structure that has full pressure

integrity. It has got the hatch and windows in it, and it fits inside the upper forward fuselage and the lower forward fuselage at the top and bottom of the page as they come

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together. Then the forward reaction control system module is inserted as an engine system forward of this line, and the nosecap is added at the forward end.

The mid-fuselage is a big structure configured as shown here, and the forward fuselage segments are added to it, payload bay doors are added to it in assembly, and the wings from the side and through feed-throughs into the main structural members of the mid-fuselage.

And then the aft fuselage is added on the back end and contains some significant systems for propulsion and control of the vehicle. Two OMS pods, Orbital Maneuvering System pods, with their RCS elements also are added on the top of the aft fuselage, the body flap added in back, and the vertical stabilizer added to the top.

(Viewgraph.) [Ref. 2/6-44]

MR. ALDRICH: The next chart describes the thermal protection system, and I will describe that, and then I will deviate here and talk to you a little bit about the ice question that we had earlier.

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I mentioned, and I am sorry this is so poorly color-coded, the reinforced carbon—carbon is the hardest parts of the TPS. It is on the nosecap and the leading edges where I showed you. The plan view of the orbiter is cut down the middle, so on the bottom part of this picture you have a representation of the top surface of the orbiter, and on the top portion you have a representation of the bottom view of the orbiter.

The bottom of the orbiter sees the highest temperatures, up to 2,800 degrees for some mission profiles during entry, and it has on it what we call black tiles. The real name for them is high temperature reusable surface insulation. They are largely silicon fiber, and they are manufactured. They are very lightweight, very easy to handle, very lightweight material.

The tiles on the total underbody of the vehicle are this high temperature reusable surface insulation. It ranges from half an inch up to two inches in thickness. It is very thick on the body flap, very thick on the forward end of the orbiter, and varies other places across the vehicle.

Coming around the sidewall and up to the top of the vehicle are the things we have called in the past white tiles. You have seen them, and they are white,

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and they are low temperature reusable surface insulation, again silicon, different physical appearance, the same kind of material, and they are generally thinner than the big tiles on the bottom. They are also lighter weight.

Further upward and rearward on the vehicle, the areas shown in white are a nomex felt material with a white surface at the top about a half an inch thick, and those are put on in blanket form. These are the coolest areas of the vehicle during the entry phase.

Now, let's see. Let me divert to the question you asked about ice on the launch pad. The question had to do with both the meeting we had on the 27th to discuss the temperatures we might expect and then the discussion on the morning of the 28th with respect to ice and icicles, and they are two separate questions.

The day before, we had the mission management team with Jesse Moore and myself, and we discussed the temperatures and their effects, what they were predicted to be on the launch system and on the facilities, and the temperature ranges which were predicted, in the mid-20s, and warming into the morning the next day were thought to be, by all in attendance from my

understanding, at least, and what I believed to be well within the specification design of all of the flight

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elements of the vehicle, and in fact, we had no concern expressed for the temperatures that the flight vehicle would see.

We were concerned about the facility because about a year ago, in January of 1985, we had a launch attempt with temperatures in this range, and we had problems with icing on the launch pad that caused some of the facility water systems to malfunction in such a way that the launch could not be continued, and we had to delay a day because of this ice.

So our discussion dealt primarily with the facility and the corrective actions from the previous event as to whether the facility would be adequate to support the countdown, the servicing and the launch, and Kennedy had changed procedures and had in place arrangements and mechanizations that we all felt would perhaps be more difficult than the normal countdown but they would support the full launch processing.

At that time we made the decision to proceed, assumed we might have some slowness in the processing activities, which we did experience. We had no concern for performance or safety of the flight articles at that time, nor do I even at this time, given what we know to be the specification, certification and design of the components that represent the flight system.

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DR. WHEELON: A question, please.

Did you or your experts specifically consider the effect of the ambient temperature on the solid rocket motors, and did you judge that that was okay?

MR. ALDRICH: I would like to have you specifically ask that to the Marshall Project, Dr. Lovingood, when he is up. My answer would be yes, I am sure they did, but they ought to say that. They reported that all of their considerations for launch were acceptable and they were go for launch, which would include those temperatures, yes, sir.

DR. WHEELON: But that wasn't explicitly discussed with you?

MR. ALDRICH: It was not explicitly discussed as a concern, partly because it is really within the design characteristic of the Shuttle System, as I believe all of us understand it.

CHAIRMAN ROGERS: Do you remember any warning from I guess it was Morton-Thiokol to the effect that there might be a problem with a temperature in the booster?

MR. ALDRICH: I do not recall such a warning at that time. The following morning we had had the situation where we had some water lines break, and other water lines, the protective reaction to the low

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temperatures was to let them flow during the night so that they wouldn't freeze and break, and we had this fairly large, extensive deliberation of what the ice meant to us. There was quite a large amount of ice, primarily on the north side of the launch complex where many of these water systems are and where the water was permitted to run as part of the procedure to avoid the freezing and rupturing of the lines. And on the south side of the launch pad where the launch system was, the flight system, there was significantly less ice, but it had been characterized by an ice team which we sent out on every launch and had been out on this launch.

In addition, the sun was rising, and on the south side of the vehicle, and we were already seeing melting in several of the areas on the vehicle. We made a detailed assessment of the reports of the ice team about where the ice was located, where it might fall, what it might impact

on the launch system, and the total context of that discussion had to do with the orbiter thermal protection system which has the soft elements that I have described to you.

There was no discussion or concern expressed about the falling of ice on any other system in either the launch complex or on the solid rocket boosters or external tank. There was a detailed assessment by the

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team and reported to us of where the ice was located, and as I say, there were large amounts on the north side, smaller amounts on the south side. We calculated what ice might fall at ignition and what its trajectory might be in conjunction with the winds, and the total recommendation from all parties concerned was that we did not see a credible threat to the orbiter except for the Rockwell International orbiter contractor who in that meeting expressed some concern that there might be a slightly higher risk for the orbiter TPS because this was a condition we had no experience with before, that is, lifting off with ice on the launch pad.

VICE CHAIRMAN ARMSTRONG: Could I ask the source of the ice, what percentage was due to the ambient conditions and what was condensation on the vehicle that froze?

MR. ALDRICH: I can't give you a totally accurate report on that, but essentially the icing on the propellant lines and on the external tank was relatively normal, quite low, and well within the bounds that we had accepted on previous launches. All of the ice I'm talking about was on the launch facility which is off to the side of the vehicle and does not protrude out over any of the flight elements, or at least does not protrude out over the orbiter. There is an arm that

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goes out over the nose of the external tank.

VICE CHAIRMAN ARMSTRONG: It was unusual because of the unique weather conditions?

MR. ALDRICH: It was unusual because of the weather conditions, and the corrective actions to drain the facility water on the launch pad causing great amounts of water to be in certain locations.

DR. WHEELON: Was there either a tape recording or a written record made of these deliberations prior to launch, and would that be available to this Commission?

MR. ALDRICH: We have asked for a detailed report of the specific configuration of the ice both during this initial inspection that led to this discussion and then I am about to tell you that we sent the team out a second time to reassess it, and there will be—the written report will include the findings that the second assessment and the clearing up of small amounts of ice on the platform. There was not a recording of the meeting in which we discussed the ice debris threat to the orbiter.

DR. WHEELON: Quite aside from the ice debris, was there a recording or minutes taken of the meeting where you made the deliberations as to whether to go ahead or not?

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MR. ALDRICH: That is the same.

DR. WHEELON: You are focusing just on ice. I am not worried about ice. I am worried about a much broader class of issues. How do you record those deliberations?

MR. ALDRICH: The meeting where we elected to proceed was held the night before, the mission management team meeting that Jesse described, and all parties felt agreeable to go. The normal process during the countdown is that the countdown proceeds, assuming we are in a go posture, and at various points during the countdown we tag up on the operational loops and face

to face in the firing room to ascertain the facts that project elements that are monitoring the data and that are understanding the situation as we proceed are still in the go condition.

And this is done prior to minus 20 minutes in the count, and is done again at minus 9 minutes in the count as a matter of procedure.

DR. WHEELON: I think you are answering a different question than the one I asked, and that is these key meetings where you and Jesse made the decisions to go forward or to delay, as you had in days prior to the launch, was there a detailed record of those deliberations made or not?

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MR. ALDRICH: To my knowledge, there is not a written record or a recording of those meetings.

DR. WHEELON: Thank you.

MR. SUTTER: In releasing one of the vehicles for flight, and especially now that some of the equipment has been gone through refurbishment, and it is done by people that do work for you, is there a formal documentation of releasing the vehicle for flight? Is there a formal method of doing it, and is it signed off like when they release an airplane for flight?

MR. ALDRICH: Yes, it is, and it is signed off in great detail. And one of the briefings later in the day will go through that flight readiness process. It is the series of meetings that led up to the ones that Jesse reported right in close at L-1 day. There is a formal flight readiness review that is extremely thorough, with formal commitments and sign-off of all NASA organizations and all contractors that support NASA for these elements.

MR. SUTTER: Going back to this temperature question, then, if one of the units was designed to a given temperature range, then whoever was responsible for that unit, if it was outside of that range, would have to make that known, and there would have to be some review board action on that?

MR. ALDRICH: Yes, sir, and that would occur

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in our system.

MR. SUTTER: Okay.

DR. WHEELON: And there were no such exceptions?

MR. ALDRICH: There were no such exceptions.

DR. WHEELON: Thank you.

DR. RIDE: How does the ice team document what it sees? Do they take cameras out with them?

MR. ALDRICH: They take cameras, I believe, Sally. I have not seen pictures from the ice team, but our discussion was that it would be photo documented. They also take IR measurement devices and actually measure the temperature around the tank and the propellant lines where we have a concern for perhaps the formation of ice on the flight system, and this occurs on all flights, the concern for ice on the external tank. It has liquid hydrogen and liquid oxygen in it. It is serviced with insulated lines, and depending on the wind and the amount of humidity and the ambient temperature, you can see extensive icing for different conditions than only the low temperature case.

In fact, one of the reasons the tank, in my understanding, had a small amount of—it did not have a major sheet of ice on it. It had perhaps a slight amount of frosting. One of the reasons is that although

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the temperature is very low, the humidity was also very low.

This weather discussion we have been having was the result of a front that came through the Kennedy area with high winds, cold temperatures, and very low humidity.

I would go now to a couple of more discussions on the orbiter. I wanted to show you what was inside several of these elements.

(Viewgraph.) [Ref. 2/6-45]

MR. ALDRICH: The next chart shows what is inside the main propulsion system, inside the aft fuselage, which is primarily the main propulsion system. You can see here where the two 17-inch lines come into the bottom of the orbiter, the hydrogen, liquid hydrogen on the port side and the liquid oxygen on the starboard side, through the 17-inch lines and into a candelabra of individual lines that go to each of the three main engines.

In the lower right hand picture is a schematic of the oxygen from the forward end of the ET, hydrogen from the aft end flowing through these lines, and the orbiter interface to the main engines.

I would also point out, not very clearly visible here, but there is an extensive heavyweight load

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structure built into the aft fuselage to carry the main thrust and loads from the main engines, and that structure is seen up in this area and around the propellant lines that are shown coming from the external tank.

(Viewgraph.) [Ref. 2/6-46]

MR. ALDRICH: The next chart shows the forward and the aft Reaction Control System. I mentioned a number of engines. I wanted to point out to you the amount of propellants and the type of propellants that are on board. In the forward Reaction Control System we have 477 pounds of nitrogen tetroxide in the starboard tank, 928 pounds of monomethyl hydrozene in the port tank. They are pressurized by a helium bottle, and they feed the thruster system I previously described to you. The two aft pods contain both the RCS system on each side and the OMS system. This chart lists the characteristics of the RCS. Again, the oxidizer tank, I believe it is the lower tank, in the forward end of the pod, contains almost 3,000 pounds of nitrogen tetroxide. The upper tank, the fuel tank, contains 1856 pounds of monomethyl hydrozene. This is pressurized by RCS helium bottles down at the upper corner of the other end of the pod, and the propellant flows to the RCS thrusters, and they can be interconnected so the

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tanks on one side can feed the engines on the side or across on the other side as well.

The next chart shows the Orbiter Maneuvering System.

(Viewgraph.) [Ref. 2/6-47]

MR. ALDRICH: The way the charts are laid out, they deal with the OMS separately, even though it is the same component of the vehicle, and as you can see, there's 14,866 pounds of nitrogen tetroxide and 9,010 pounds of hydrozene in the lower Ox tank and the upper fuel tank, again pressurized by helium bottles, and they feed the big OMS engine either on this side or can be cross connected and feed the OMS engine on the other side.

(Viewgraph.) [Ref. 2/6-48]

MR. ALDRICH: The next chart describes the electrical power system within the orbiter. This shows the cargo bay, the mid-fuselage I have shown you before. All of the cargo fits within the U-shaped rings, and there is a liner, but under the liner, in the cargo bay are a number of

orbiter systems, and I have highlighted here the electrical power systems. The electrical power in the orbiter is generated by three fuel cells which are fed by cryogenic liquid oxygen and liquid hydrogen bottles. The configuration shown here

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shows three oxygen tanks and three hydrogen tanks. We can increase that to either four or five of each for longer duration missions.

Again, there is a fair amount of fluid in these tanks. There is 781 pounds of oxygen per tank, 92 pounds of liquid hydrogen in the hydrogen tanks. They provide 2370 kilowatt hours of energy to the orbiter during the mission, and 168 pounds of oxygen for supplying the atmosphere in the orbiter cabin and keeping the cabin pressurized.

(Viewgraph.) [Ref. 2/6-49]

MR. ALDRICH: The next chart shows a little bit about the crew arrangement. This is looking down only on the crew module of the vehicle. The top two pictures show the upper level in the crew module, the flight deck. The lower two pictures show the mid-deck. For launch and entry there are four crew members seated approximately as shown here, on the flight deck, the commander on the left, pilot on the right, and mission or payload specialist seated in the back, and they can participate in some of the procedures in support of flying the vehicle from the forward consoles.

Once you go on orbit on the flight deck, more or less crew members can come up to the flight deck, and a large number of the consoles on the back end of the

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flight deck deal with the payloads and the cargo, and they are operated by various mission and payload specialists.

DR. WALKER: Is the atmosphere in the crew quarters pure oxygen?

MR. ALDRICH: No, it's oxygen-nitrogen mix, roughly basic atmosphere, basic earth atmosphere. In the mid-deck you can see the configuration that was approximately what we had on the STS 51-L, two mission specialists in front of the airlock facing the module stowage, and one back by where it says the waste compartment is located. The other three crew seats shown in dotted are contingency configuration that can be used to extend the capacity of the vehicle. However, on this flight and on most flights, that is taken up with either three or four sleep stations as is shown on the right hand side of the page.

Locker storage is in front of these crewmen, and we stow there things for crew—provisions, food, clothing, communications equipment, camera equipment. We also stow some experiments and some flight activities in those areas.

Waste management compartment is back in the lower port corner, and the side hatch for exit and entry to the vehicle is shown on the port side. Airlock is

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truly that. You enter the airlock from the cabin, don a space suit, decompress, open a hatch, and go out into the payload bay for external operations on orbit. The mid-deck picture on the right shows a little more detail of personal hygiene station, galley station, and tables that are set up once the orbital configuration is arranged on board.

(Viewgraph.) [Ref. 2/6-50]

MR. ALDRICH: The next chart discusses some major systems in the orbiter that I am not going to go into in great detail today. There is an extensive avionics system in the orbiter, and it controls not only the orbiter but the solid rocket boosters and the external tank, and it works in conjunction with the launch processing system.

There are a series of avionics bays shown here. In the mid-deck portion of the cabin, there is a Bay 1 forward and Bay 2 forward. Bay 3 is in the back end of the crew compartment, and many of the major electronics components, including the general purpose computers that fly the Space Shuttle are located in that region.

Above them on a navigation fixed mount are the inertial measuring units for flying the Space Shuttle System and the star trackers for aligning it once you are on orbit. They are mounted in proximity with these

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other electronics, and they are part of the total guidance and navigation system.

In the back end of the orbiter are three more electronic bays with, again, additional electronic devices that participate in guidance control of the vehicle and its systems, and they are shown roughly in the forward part of the aft fuselage module that I showed you previously mounted on the back side of the bulkhead. These subsystems provide for guidance and control of the total Space Shuttle for ascent, on-orbit and entry. Communications and tracking provide a series of different communications modes through S band and UHF, through an on-orbit Ku band system that has an extensively high data capacity. They provide the displays and controls to the flight crew who can interface to the vehicle systems through the avionics and through the computers. They provide for the electrical power distribution and the instrumentation throughout the vehicle, and they provide a series of data processors that both record data and instrumentation measurements on board and provide those to the ground.

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Those are the basic systems of the orbiter that I was going to describe. There is one other high energy system on board. The system operates its elevons and its landing gear release and brakes and body flap and rudder speed brake with three hydraulic systems, which run around to the appropriate places on the vehicle.

Also, the throttling and control of the space shuttle main engine use the orbiter hydraulic systems. And to provide the orbiter power, there are three auxiliary power units mounted in the aft fuselage, and they have each 325 pounds of hydrozene in a tank to provide those APUs.

Those units are powered prelaunch for ascent to control the engines and the aero surfaces, and then pre the orbit for entry to control the aero flight for the approach and landing.

There are also a series of systems in the cargo bay that support the various cargo modules. There are electronics, there are beam structures to support the payloads. There are keel fittings, a wide range of instrumentation and communications systems that are put together in standard configuration so that they can support the widest range of cargo mixes that we fly in the space shuttle.

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That is the briefing that I was going to give you on the shuttle configuration and on the orbiter, and I realize it is not nearly the depth to cover any of those things in detail. But I thought that would be a good start.

And I would be glad to answer any other question about the way the vehicle works or how it is integrated.

CHAIRMAN ROGERS: Well, thank you very much. It has been very helpful.

I wonder, was there anything about this launch, excluding the discussions about ice and weather, that caused any concern over and above the normal concern? Anything unusual about this launch that we should know about?

MR. ALDRICH: I do not recall that there were any unusual things other than we had the situation with the hatch, where we did not get it off.

CHAIRMAN ROGERS: Which Mr. Moore referred to. But excluding that and excluding the questions of the weather, anything else that was discussed about this launch that was different from the previous launches?

MR. ALDRICH: No, sir, not to my knowledge.

I would add, that reminded me of one of the questions you asked earlier about the anomaly tracking.

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As we first flew Columbia and had our initial flights with the orbiter, we did have quite a large number of anomalies in the space shuttle systems, and most of them were not of consequence to completing the mission or to doing the activities on board the way we intended to do them.

There were major engineering problems with the systems that are highly redundant, and some of the redundancy areas had anomalies, and the system accounted for it properly. We track each of those systems problems individually by item. It is researched, analyzed, and closed out formally to my level in the program to be sure we have treated it correctly and completely.

We have it signed off, and we keep a formal tracking system of all of these things that occur. And we have had anomalies on every flight. With the amount of instrumentation and redundant subsystems we have, we frequently have things that need corrective action. Every one has been tracked, every one is recorded and logged and available.

And a different response than Jess gave to your question about how our performance has been: We have seen significantly fewer anomalies on flights in the last year or two than we had early in the program.

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There has been a significant correction of things we couldn't find until we flight-tested, and corrective actions.

In fact, the Atlantis vehicle, which has flown two times and was delivered in the spring of this last year, has been extremely clean and has had no significant anomaly as yet in any of its systems. So I think there is a great learning curve, particularly with respect to the orbiter, as we have flown and become familiar with the vehicles.

CHAIRMAN ROGERS: I assume that you are comparing the flight pattern of this launch with previous ones to see if there are any deviations from previous flights?

MR. ALDRICH: Yes, sir. We do do the exact characterizations of the winds and profile that Jess described. We do build a best trajectory from all the data available, and apply those best known external conditions to it, and we analyze completely, to the best of our ability, the exact loadings and the profile that the vehicle flew through.

I might add, we talked about throttling the main engines. There is in fact on board an adaptive control within the orbiter system. The solid rockets have very minor variances, depending upon a lot of

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parameters. And the on-board system senses that and throttles the main engine to a precise level to account for that.

So what you predicted it might go to pre-flight may be slightly different. That is understood. That also will be factored into our total analysis of the ascent system performance.

CHAIRMAN ROGERS: At what point in the flight was the loss of power detected?

MR. ALDRICH: We essentially lost all data with the vehicle at very close to 73 seconds, 73 and some tenths either way seconds. And loss of power and data presumably is the same thing. All contact with the ground was lost instantly at that time, and there is a great reconstruction of all events from data and from tracking and from photo.

And I haven't seen that yet. That is a fairly laborious job, but my expectation would be that it will coincide with the physical event that we saw.

DR. RIDE: Could you say something very briefly about the data lines between the SRBs and the shuttle, the shuttle computers?

MR. ALDRICH: Let me see if I can say what you want me to say.

The central control and computing for the

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entire stack during ascent is in the computers in the orbiter. Some of the sensing equipment—for instance, there is a set of rate gyros at the top of each solid rocket motor. That is fed through the electronics system to the orbiter and it is factored into the orbiter guidance computations for the total stack.

There is also telemetry and measurements on various parts of the tank and the solid rocket booster, and those come back into the orbiter and are relayed or recorded in conjunction with the orbiter data stream. And for the separation sequences, commands go the other way, from the orbiter.

The orbiter guidance and navigation system senses when separation should occur, when engine throttling or gimballing should occur—not throttling of the solids, as was pointed out, but throttling of the main engines, but gimballing of both mains and solids—and those commands go through data lines to the solid rocket boosters and to the space shuttle main engines.

DR. WHEELON: Since I sense that our chairman may soon be calling a luncheon break and since that provides an opportunity to get some data, would it be possible to get from you or from your colleagues the nominal trajectory, powered flight trajectory parameters as anticipated for this

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flight, not as actually measured? And specifically, what was—what did you expect would be the altitude versus time and the speed, the Mach number, the dynamic pressure, and the throttle position on the main engines?

Could you provide that?

MR. ALDRICH: Expected pre-flight and compared to what we found in-flight, yes, sir.

DR. WHEELON: I suspect you don't yet have the actuals, and if you do that's fine. But, I would be grateful for just the pre-flight nominal for this flight.

MR. ALDRICH: I will do that.

CHAIRMAN ROGERS: Mr. Acheson wondered if there was any way to make an estimate of how much further information you want to give to us today. The question is not designed to hurry you at all, and we are prepared to continue over until tomorrow. We just want to get some idea of what estimates you make and the time that would be involved.

Jess, can you address this?

MR. MOORE: Mr. Chairman, what we are prepared to do from here the rest of the afternoon is to go through the Marshall shuttle projects. As I mentioned earlier, the Marshall Space Flight Center is responsible

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for the propulsive elements of the shuttle.

We have probably got about a 45-minute briefing or so on the propulsive elements, and maybe a little longer. I am kind of guessing. I didn't have time last night to go through a formal dry run.

Following that, I have a presentation by the Kennedy Space Center to tell you how the launch system is processed and all the steps to get ready for launch, and that probably can be done in about 35 to 40 minutes, depending upon questions. It is mostly photographs, to give you a feel for what we go through in getting ready for launch.

Beyond that, we have got about a 30-minute pitch that will address the design philosophy—requirements, certification, testing, analysis—that we go through in NASA for procuring hardware. And then I've got another presentation which is about another 30 minutes long that talks about our flight certification, preparations for flight, and talks about the flight certification process and the specifics associated with how we pool the resources of the NASA-industry shuttle team to get ready for a flight.

If I add those up very roughly in my mind, we are probably talking about another three hours-plus of briefings if the Commission would so desire.

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CHAIRMAN ROGERS: Well, that's fine. We can extend over until tomorrow, and we don't want to hurry you at all.

And I think we will take a recess now for about an hour. I would like to suggest, though, on the presentations, that if you could relate your presentations a little more directly to the Challenger and what happened. Otherwise it becomes rather abstract.

And so, I don't want to discourage that aspect of it, but if you could relate it a little bit more to what happened here and what you did in connection with the Challenger, anything that was unusual, I think the Commission would appreciate that.

MR. MOORE: Yes, sir. What we tried to do for the Commission, Mr. Chairman, is also to give you some indication of how the systems are manufactured and how they are put together. And that specifically is applicable to Challenger, as well as the other elements of the shuttle program. So we will try to narrow our focus a little bit more on the specifics associated with Challenger and any differences that we possibly can highlight relative to this flight versus others.

CHAIRMAN ROGERS: Thank you very much.

Okay, we will adjourn until 1:30.

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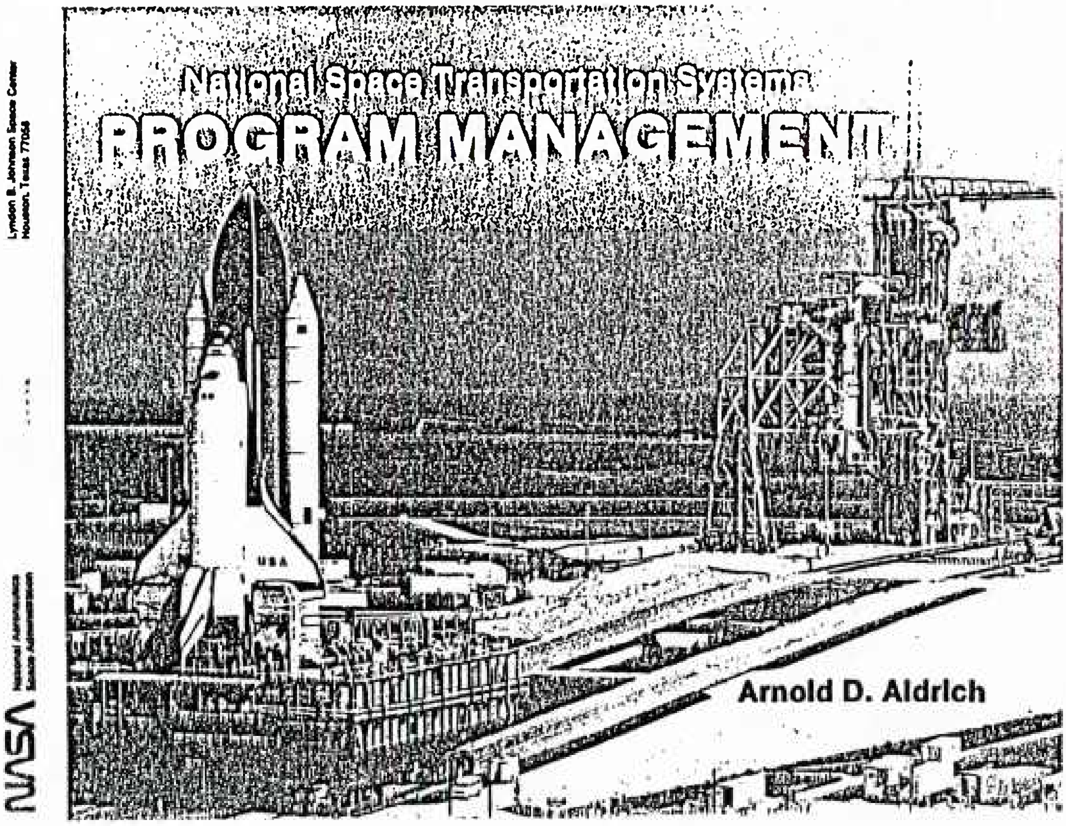
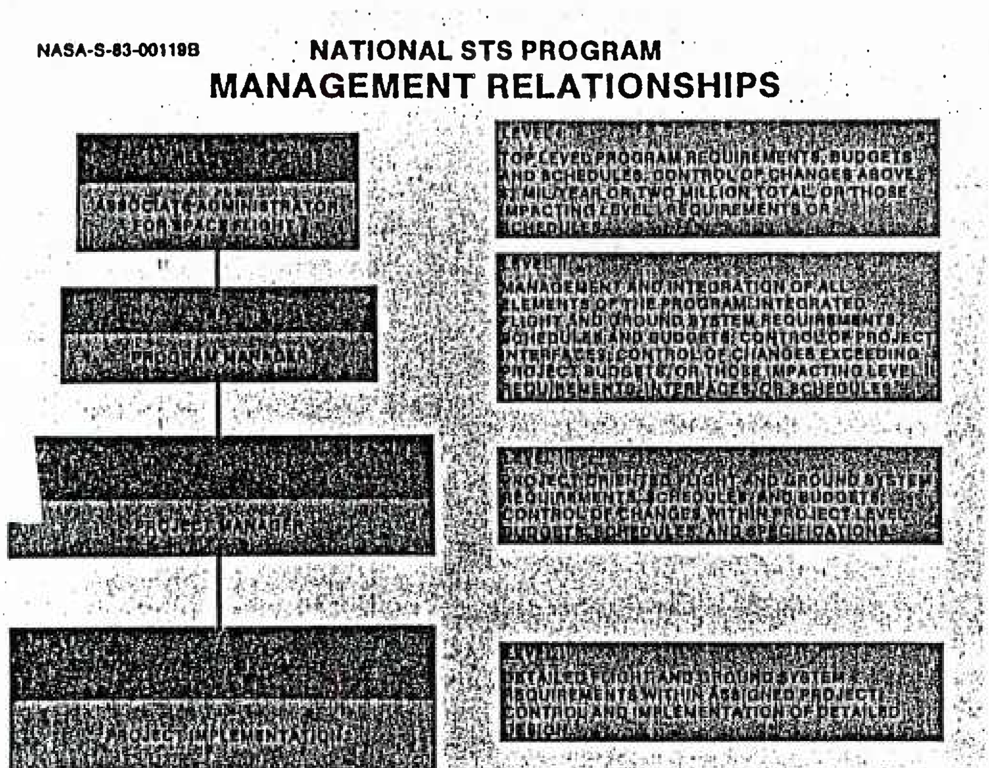
(Whereupon, at 12:40 p.m., the hearing was recessed, to reconvene at 1:30 p.m.)



[Ref. 2/6-29]



[Ref. 2/6-30]

National Aeronautics and Space Administration

Lyndon B. Johnson Space Center
Houston, Texas 77058

NASA-S-83-00110B

NATIONAL STS PROGRAM
ORGANIZATIONAL APPROACH

- LEAD CENTER CONCEPT
  - SMALL HQ STAFF
  - SPACE SHUTTLE PROGRAM OFFICE LOCATED AT JSC
- APPLICATION OF A WORK BREAKDOWN STRUCTURE TO ALL ELEMENTS INCLUDING THE GOVERNMENT ACTIVITIES AND MANAGEMENT
- PROGRAM OFFICE MANAGES PROJECTS, PROJECTS MANAGE CONTRACTS
- "INTEGRATION" IDENTIFIED AS A GOVERNMENT ROLE
- PROJECT MANAGERS PLACED IN A LINE RELATIONSHIP
- CAREFUL DOCUMENTATION AND CONTROL OF MANAGEMENT AND TECHNICAL REQUIREMENTS AT ALL ORGANIZATIONAL LEVELS
- EXTENSIVE TELECONFERENCING TO REDUCE TRAVEL
- EXTENSIVE DOD/USAF PARTICIPATION

[Ref. 2/6-31]

NATIONAL STS PROGRAM

GOVERNMENT/INDUSTRY TEAM

FUNCTION	GOVERNMENT	INDUSTRY
o OVERALL POLICY AND DIRECTION ..	NASA HEADQUARTERS	
o PROGRAM MANAGEMENT	JSC, LEAD CENTER	RI/SD - SYSTEMS ENGINEERING AND INTEGRATION SUPPORT
- PROGRAM PLANNING AND CONTROL		RSOC - SHUTTLE ENGINEERING AND OPERATIONS SUPPORT
- SYSTEM INTEGRATION		
- OPERATIONS INTEGRATION		
- MISSION INTEGRATION		
o PROJECT MANAGEMENT		
- ORBITER	JSC	RI/SD - PRIME
- SSME	MSFC................	RI/RD - PRIME
- ET	MSFC................	MMC - PRIME
- SRB	MSFC................	USBPC - PRIME
- SRM	MSFC................	MTI - PRIME
- LAUNCH/LANDING	KSC	LSOC - PRIME
- MISSION SUPPORT	JSC	RSOC - PRIME

[Ref. 2/6-32]

NASA National Aeronautics and Space Administration

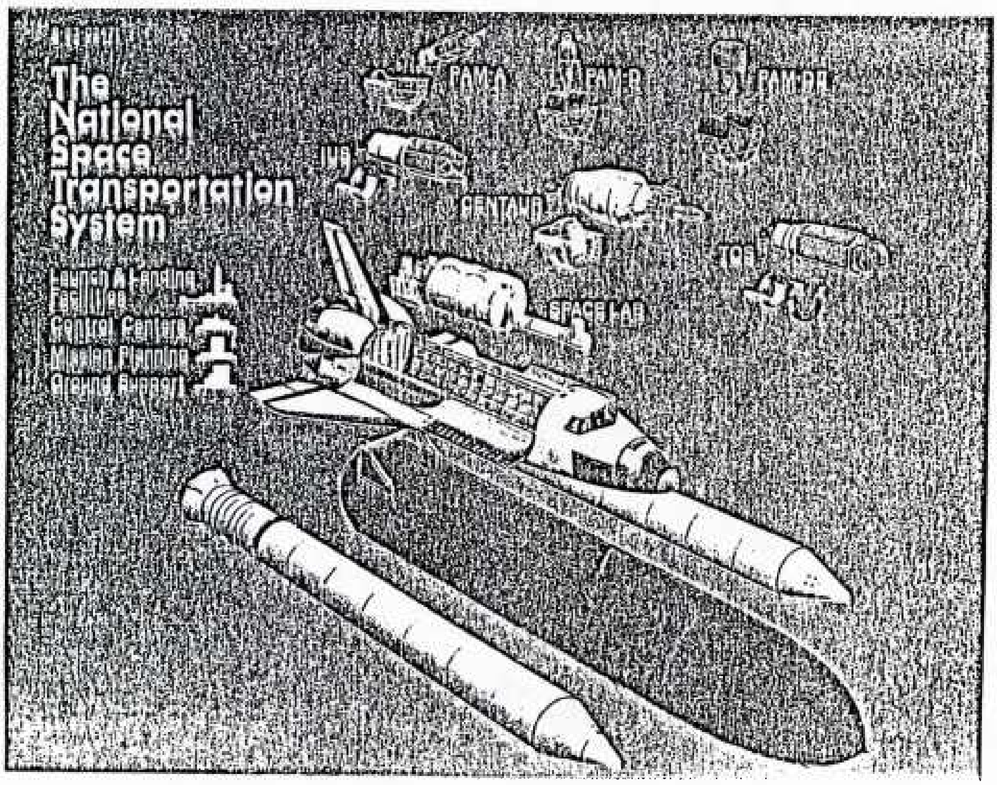
NASA-S-84-04222

NATIONAL STS PROGRAM
SHUTTLE SYSTEM ELEMENTS

- ORBITER
- FLIGHT SOFTWARE
- SSME
- ET
- SRB
- FLIGHT CREW EQUIPMENT
- CARGO
- LAUNCH AND LANDING FACILITIES/EQUIPMENT
- UPPER STAGES
  - PAM
  - IUS
  - CENTAUR

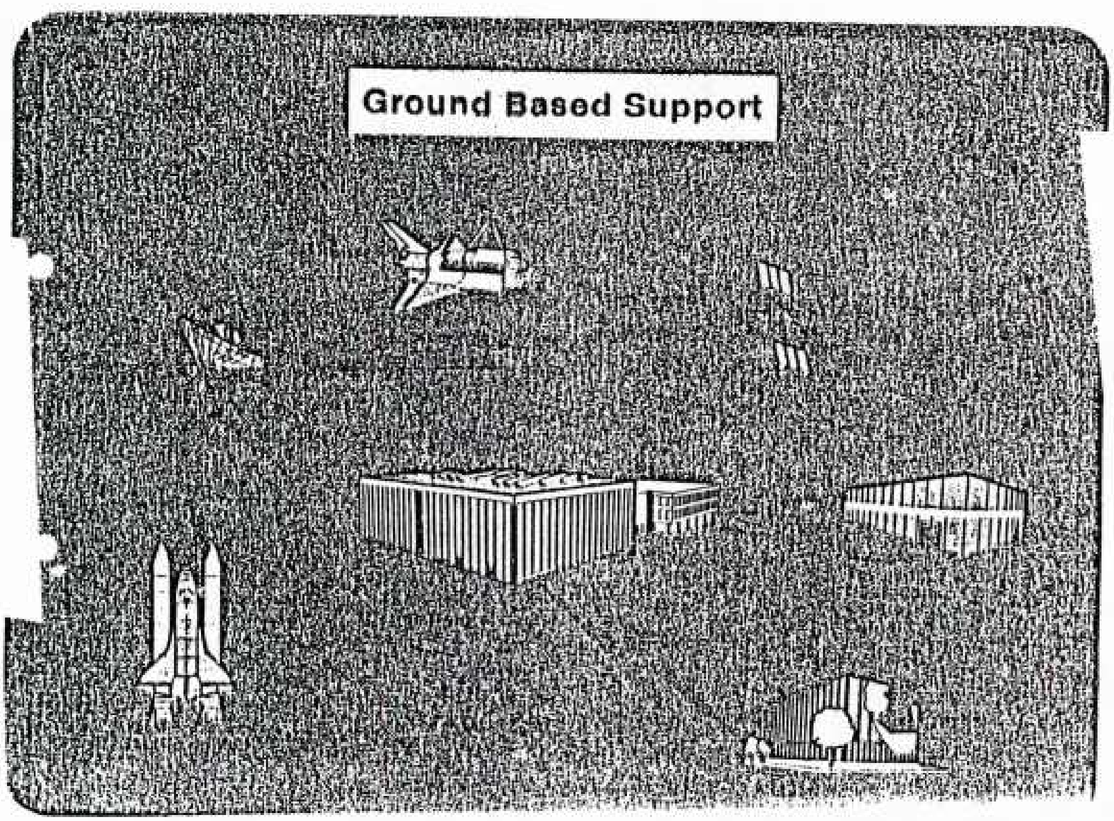
[Ref. 2/6-33]

NASA National Aeronautics and Space Administration



[Ref. 2/6-34]

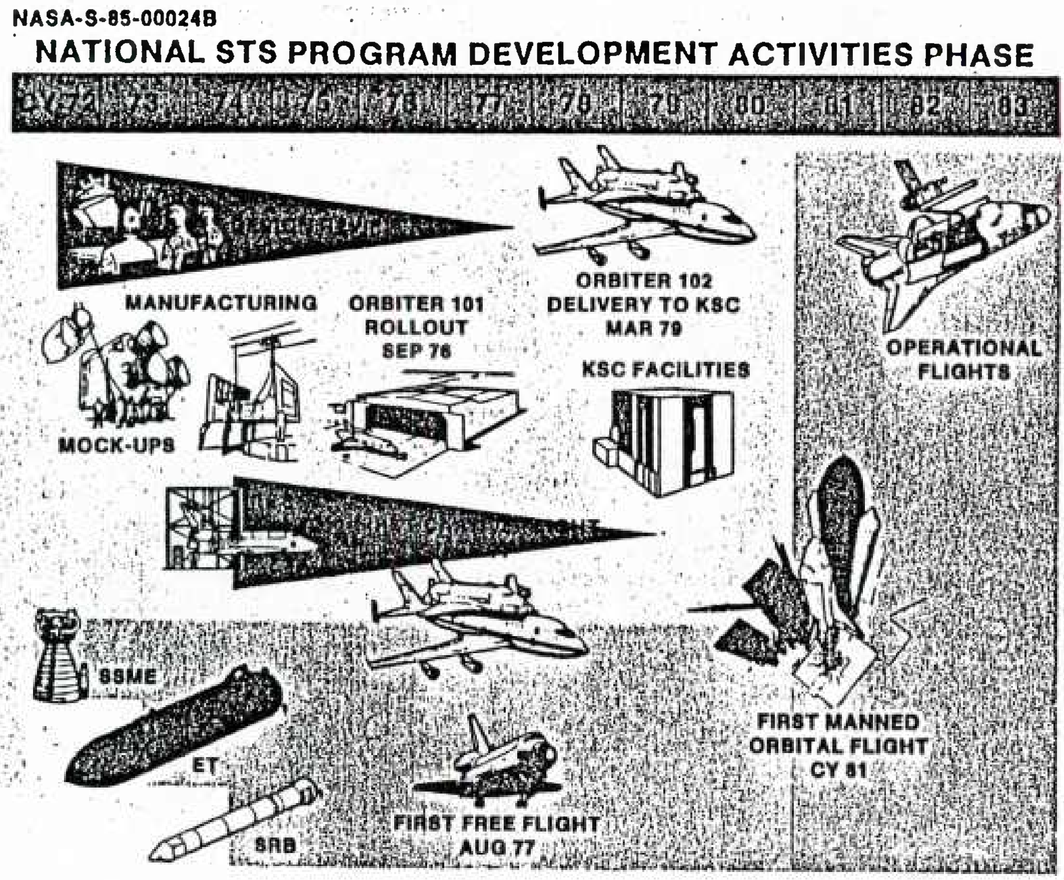
[Ref. 2/6-35]



[Ref. 2/6-36]


NASA National Aeronautics and Space Administration
S-83-02549B
NATIONAL STS PROGRAM
FACILITY AND SUPPORT LOCATIONS
BRIGHAM CITY, UTAH
• SOLID ROCKET MOTOR
U.S. AIR FORCE
NORTH AMERICAN AEROSPACE
DEFENSE COMMAND
• CONTROL FACILITY
PALMDALE/
EDWARDS AIR
FORCE BASE
• ORBITER FINAL
ASSEMBLY
• LANDING SITE
VANDENBERG
AIR FORCE BASE
• LAUNCH/LANDING
OPERATIONS
CANOGA PARK
• SPACE SHUTTLE
MAIN ENGINE
MFG., ASSEMBLY, TEST
DOWNEY
INDUSTRIAL
PLANT
• ORBITER MFG.
WHITE SANDS
TEST FACILITY
• ALTERNATE
LANDING SITE
NASA
HEADQUARTERS
MARSHALL SPACE
FLIGHT CENTER
• ET, SRB, SSME PROJECTS
KENNEDY SPACE CENTER
• LAUNCH/LANDING OPERATIONS
NATIONAL SPACE
TECHNOLOGY LABORATORIES
• SPACE SHUTTLE MAIN ENGINE
TEST FIRING FACILITY
MICHOUD
ASSEMBLY
FACILITY
• EXTERNAL TANK
JOHNSON SPACE CENTER
• NSTS PROGRAM OFFICE
• ORBITER PROJECT
• MISSION CONTROL

[Ref. 2/6-37]


NASA-S-85-00024B
NATIONAL STS PROGRAM DEVELOPMENT ACTIVITIES PHASE
CY 72 73 74 75 76 77 78 79 80 81 82 83
MANUFACTURING
ORBITER 101
ROLLOUT
SEP 76
ORBITER 102
DELIVERY TO KSC
MAR 79
KSC FACILITIES
OPERATIONAL
FLIGHTS
MOCK-UPS
SSME
ET
SRB
FIRST FREE FLIGHT
AUG 77
FIRST MANNED
ORBITAL FLIGHT
CY 81

[Ref. 2/6-38]

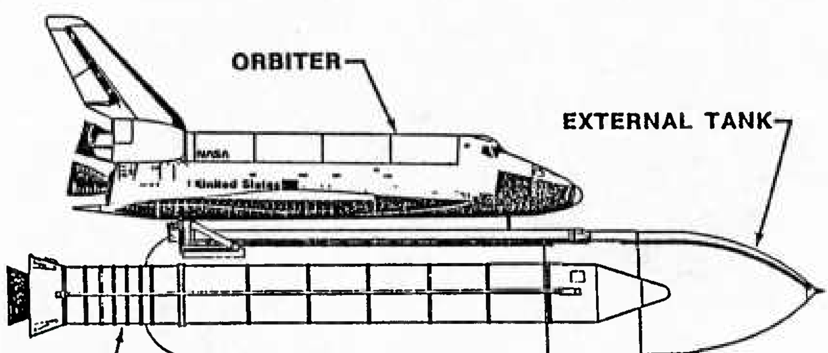
SPACE SHUTTLE

DESIGN AND DEVELOPMENT

NASA

SPACE SHUTTLE SYSTEM

S-84-01083



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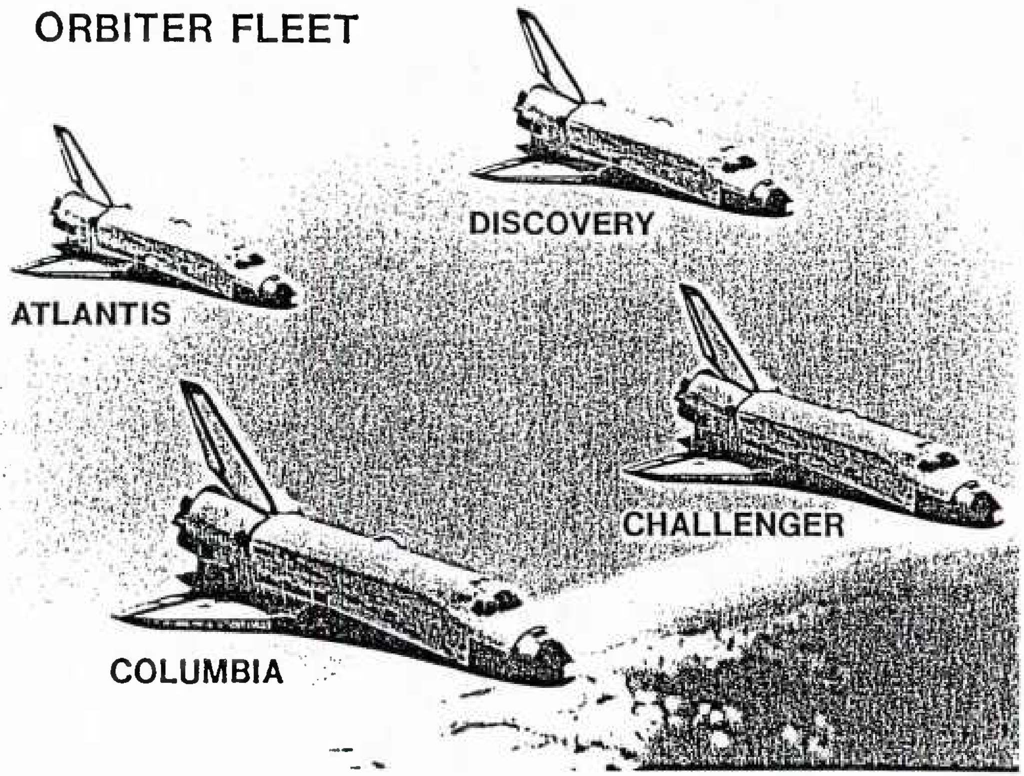

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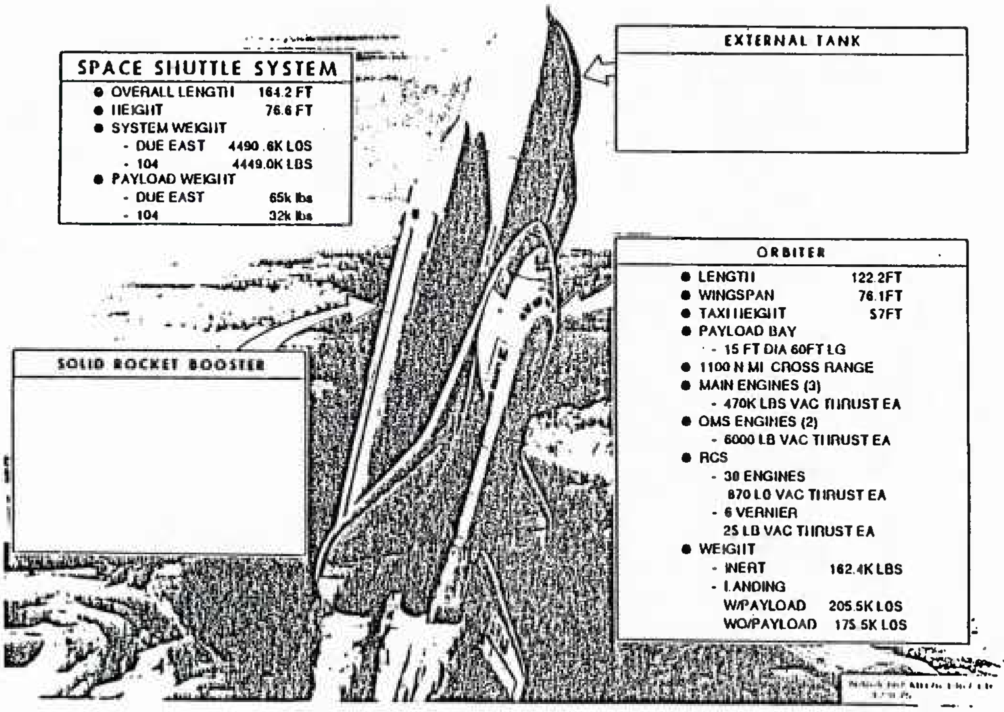
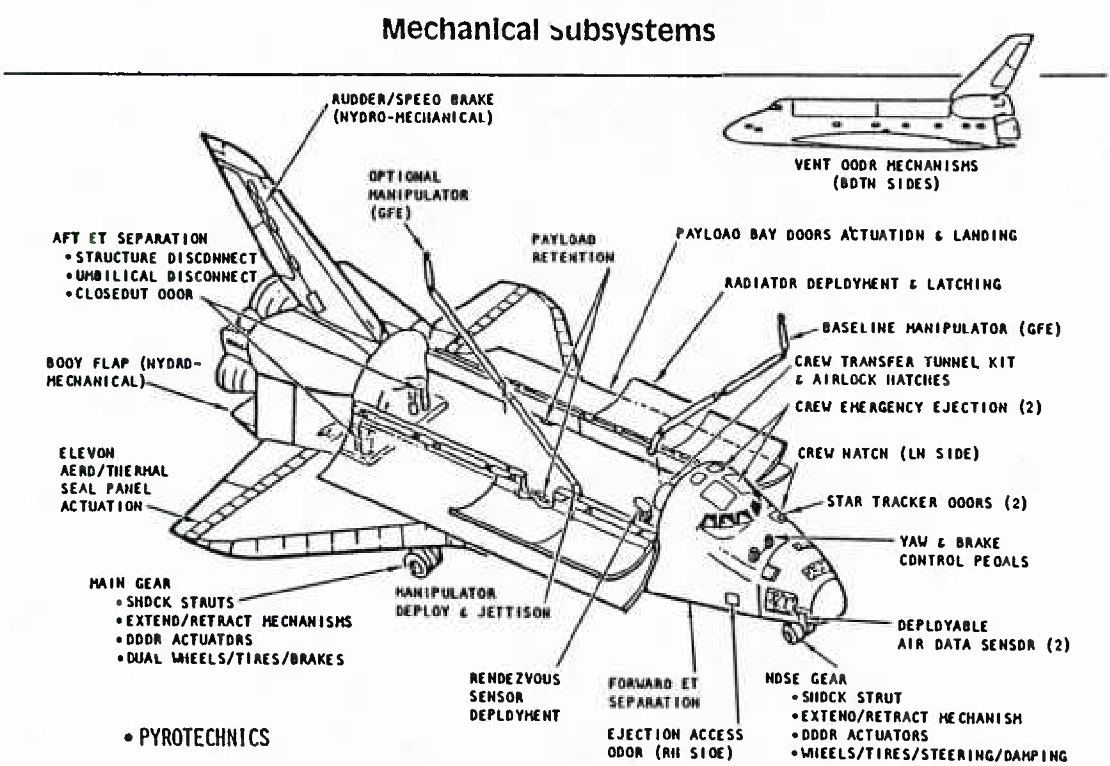


[Ref. 2/6-41]



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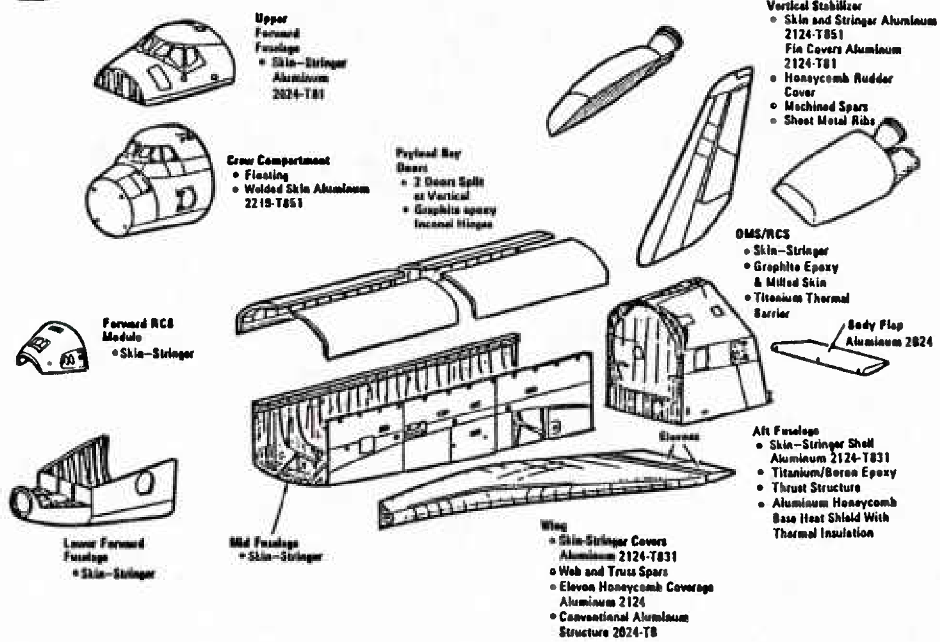
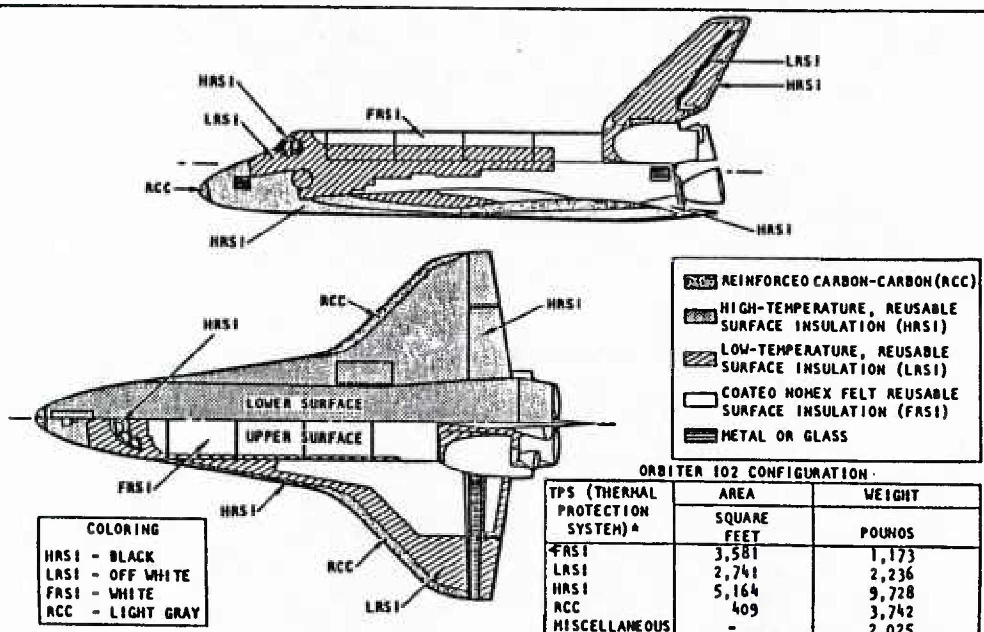


Orbiter Structural Elements

[Ref. 2/6-43]

Thermal Protection Subsystem



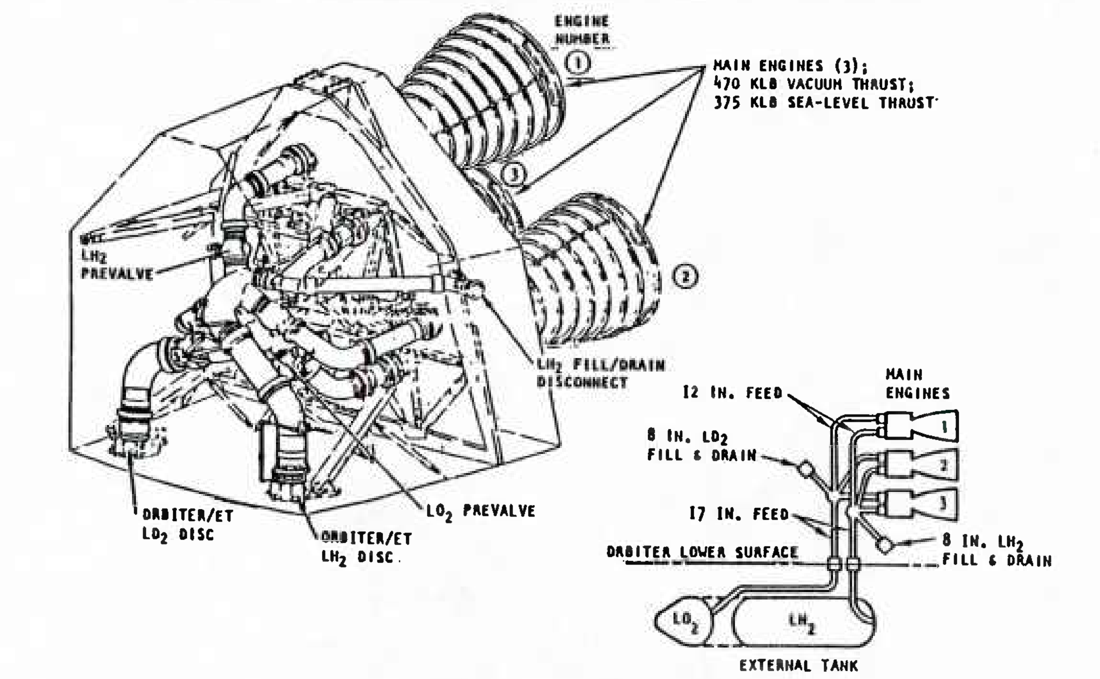
ORBITER 102 CONFIGURATION

TPS (THERMAL PROTECTION SYSTEM)*	AREA SQUARE FEET	WEIGHT POUNDS
FRSI	3,581	1,173
LRSI	2,741	2,236
HRSI	5,164	9,728
RCC	409	3,742
MISCELLANEOUS	-	2,025
TOTAL	11,895	18,904

*INCLUDES BULK INSULATION, THERMAL BARRIERS, & CLOSEOUTS

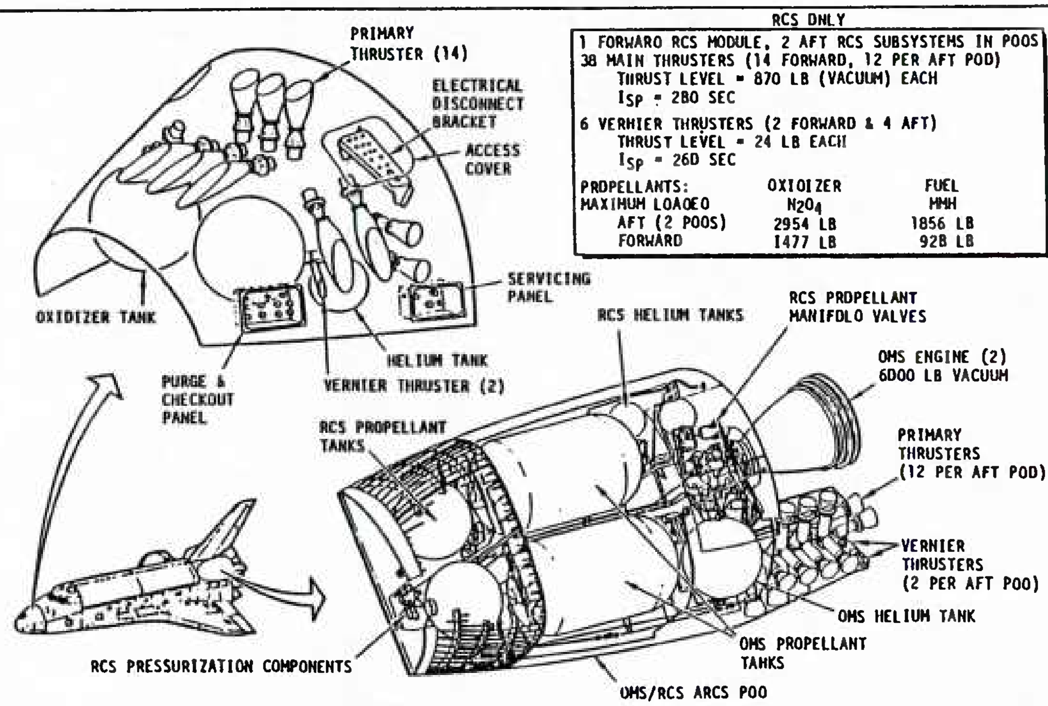
[Ref. 2/6-44]

Main Propulsion Subsystem



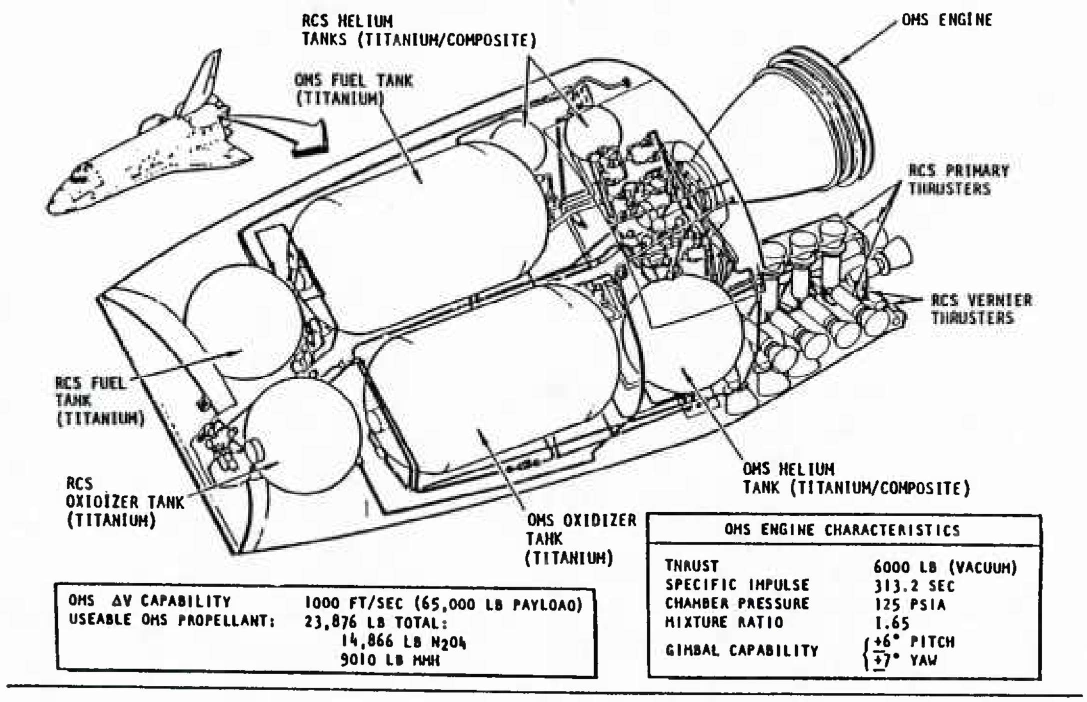
[Ref. 2/6-45]

Reaction Control Subsystem



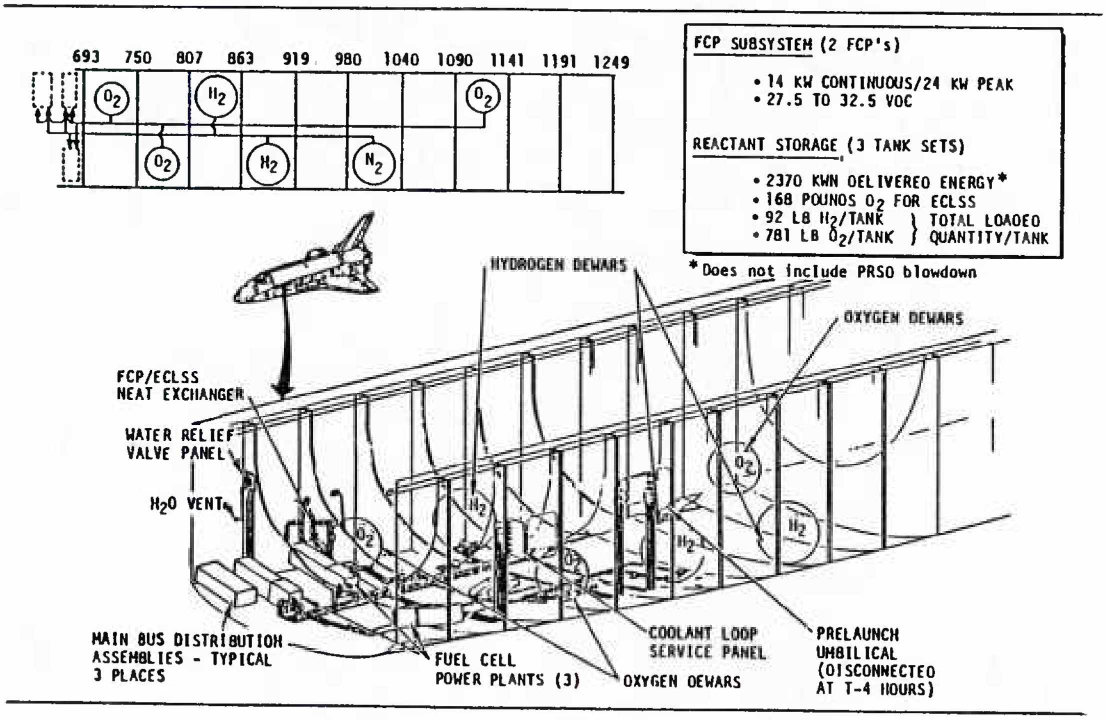
[Ref. 2/6-46]

Orbital Maneuver Subsystem



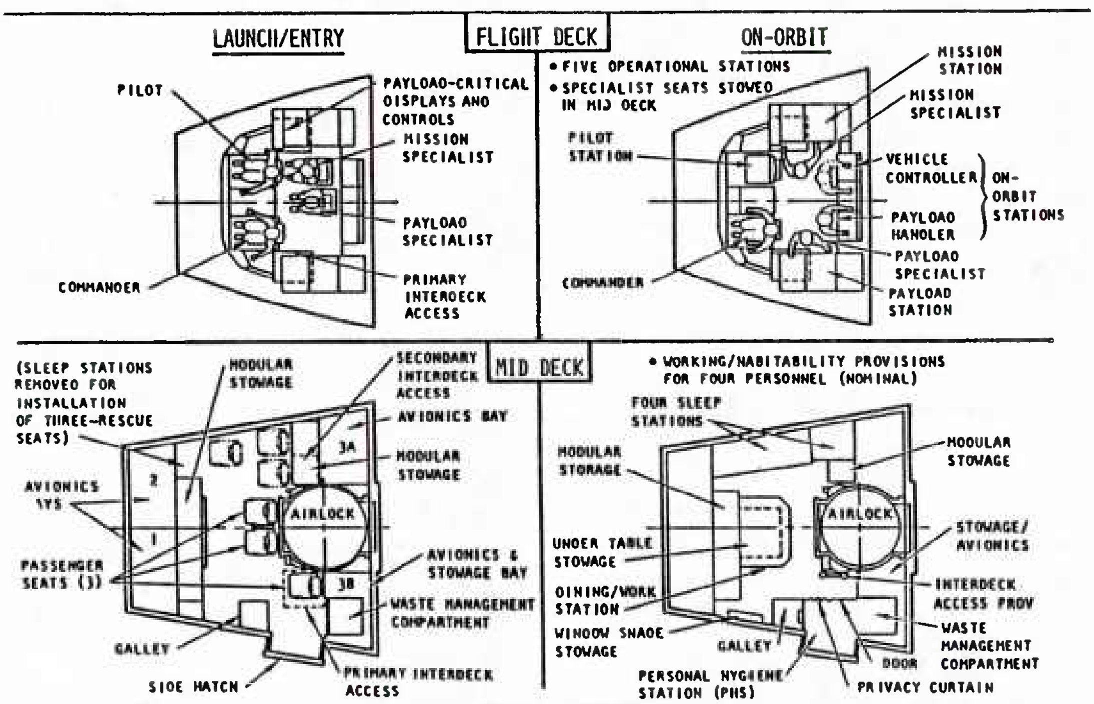
[Ref. 2/6-47]

Electrical Power Subsystem



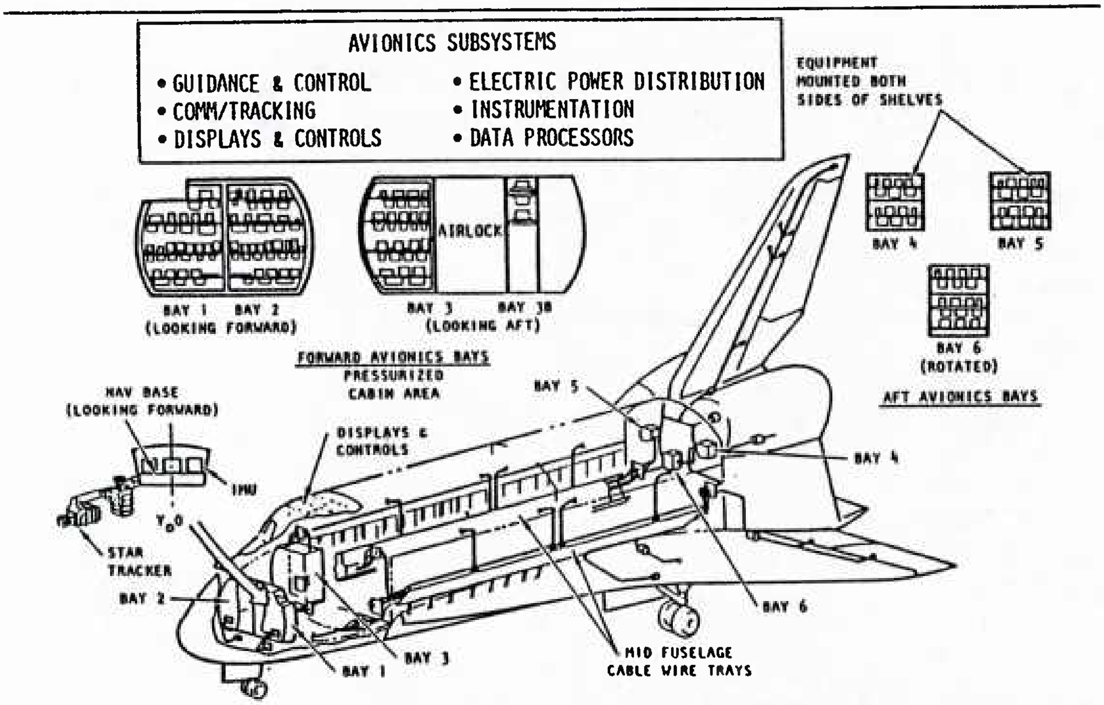
[Ref. 2/6-48]

Crew Cabin Arrangement and Crew Functions



[Ref. 2/6-49]

Orbiter Avionics Subsystem



[Ref. 2/6-50]

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PRESIDENTIAL COMMISSION ON SPACE SHUTTLE CHALLENGER ACCIDENT—THURSDAY, FEBRUARY 6, 1986

National Academy of Sciences
Auditorium
2100 Constitution Avenue, N.W.
Washington, D. C.
The Presidential Commission met, pursuant to luncheon recess, at 2:00 o'clock p.m.
PRESENT:
WILLIAM P. ROGERS, Chairman
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ALBERT WHEELON
ROBERT RUMMEL
DR. ARTHUR WALKER
RICHARD FEYNMAN
ROBERT HOTZ
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA

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AFTERNOON SESSION

CHAIRMAN ROGERS: The Commission will come to order, please.

Jesse?

MR. MOORE: Mr. Chairman, members of the Commission, we would like to continue this proceeding now. I have asked our presenters here for the remainder of the afternoon to try to make their presentations as brief as possible, particularly the background kinds of presentations, and focus as much as we can on the relevancy to the incident on 51-L.

And with that, I would like to introduce the Deputy Manager of the Shuttle Projects Office at Marshall, and that is Dr. Jud Lovingood.

Jud?

THE CLERK: Do you swear the testimony you will give before this Commission will be the truth, the whole truth, and nothing but the truth, so help you God?

DR. LOVINGOOD: I do.

CHAIRMAN ROGERS: You may proceed.

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TESTIMONY OF DR. JUDSON A. LOVINGOOD, DEPUTY MANAGER, SHUTTLE PROJECTS OFFICE, MARSHALL SPACE FLIGHT CENTER

DR. LOVINGOOD: Mr. Chairman, Committee members, what I have been asked to do today is to give you a propulsion systems overview so that it will provide you with the background that you will need in the course of your investigation, and what I have done is I have given a very brief summary of the elements that Marshall has responsibility for, which are the external tank, the main engines, and the solid rocket booster. I hope that as a result of this briefing there may be some areas that you can identify that you do want to home in on, and then we will be able to provide you additional information if I can't answer it today.

So, with that I will start out by talking about the—

(Viewgraph.) [Ref. 2/6-51]

DR. LOVINGOOD: This is the agenda, which doesn't show up very well.

Go to the next chart.

(Viewgraph.) [Ref. 2/6-52]

DR. LOVINGOOD: I think none of these are showing up on the screen.

CHAIRMAN ROGERS: Well, we have the books. We

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can follow it that way.

DR. LOVINGOOD: If you would look at those, and on these word charts, I will try to summarize basically what I am trying to say quickly.

We have two responsibilities, one in capability development, which is the early part of the program primarily, with some continuation into the operational phase which we are currently in, and then, of course, we have support to operations. We are responsible or were responsible for the development and certification of the external tank, the solid rocket booster and the Shuttle main engine. We are responsible for the propulsion system testing, which I will say more about, which is the testing of the complete propulsion system, including the external tank, the three main engines and the orbiter propulsion elements down at NSTL, the National Space Technology Laboratories. We have been involved in propulsion and ascent flight system integration activities with JSC. They have the lead, but we have been heavily involved in that activity with them because of the skills that we have at the Marshall Center.

And then performance improvements and productivity, and then in supporting the launches, we are responsible for producing the flight hardware and

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logistics support at KSC and at Vandenberg, and we are involved heavily now in the activation of the Vandenberg facility as far as processing the vehicle, and then we are also looking at oper-

ational improvements like producibility improvements, requirements reductions and simplifying the launch processing.

(Viewgraph.) [Ref. 2/6-53]

DR. LOVINGOOD: The next chart shows what I just said in pictorial form, and I won't dwell on that, and we will just go ahead and continue to the next chart.

(Viewgraph.) [Ref. 2/6-54]

DR. LOVINGOOD: The next one shows the organization that we have at Marshall for the Shuttle Projects. There is one Shuttle Projects manager that is responsible to the center director, and he has responsibility for all Marshall Shuttle activity. Under him is a project office for each element, and that is indicated down at the bottom, showing an External Tank Project Office, a Solid Rocket Booster Project Office, and a Flight Engine Project Office. In addition, we have a Development Engine Project Office which is involved in the engine improvements which Jesse Moore mentioned earlier.

Each of these project offices, if you will

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note the remark I've got in the upper right hand corner, has a chief engineer which is assigned to report directly, functionally, on a day-to-day basis, to the project manager. His institutional home is our Science and Engineering Directorate which is a major institutional organization which reports to our center director.

Proceeding to the next chart—

(Viewgraph.) [Ref. 2/6-55]

DR. LOVINGOOD:—and then this is the engine project lead-in, and then go to the following one.

(Viewgraph.) [Ref. 2/6-56]

DR. LOVINGOOD: The SSME, and of course, there has been some discussion of SSME, which is the main engine on the Shuttle. It has already been discussed to some extent by Arnie and Jess. It is a liquid hydrogen/liquid oxygen engine. It is manufactured, or its prime contractor responsible for development, certification, manufacture and launch acceptance testing is the Rocketdyne division of Rockwell. Major subcontractors are Honeywell on the controller and Hydraulic Research, on the actuators that we use for the valve controls. Test sites are the National Space Technology Laboratories. We have two single-engine test stands at the Santa Susana laboratory, which is near Canoga

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Park, where Rocketdyne is located.

(Viewgraph.) [Ref. 2/6-57]

DR. LOVINGOOD: The next chart shows the flow. As I mentioned, the engine is manufactured at Rocketdyne in Canoga Park. We acceptance test it at our designated test area at the National Space Technology Laboratories in Mississippi. We install them into the orbiter at KSC. In fact, the Marshall Space Flight Center delivers them to KSC, and then the installation in the orbiter, and then the launch processing from there on is the KSC responsibility. And between-flight maintenance is done at KSC.

(Viewgraph.) [Ref. 2/6-58]

DR. LOVINGOOD: The next chart shows some interesting characteristics, and what I want to point out on there, it is a 470,000 pound thrust engine in vacuum. We call that the rated power level. Most of our flights up until now have been at 104 percent of rated power, with a few at 100 percent.

DR. FEYNMAN: Excuse me. I am sorry to interrupt you.

I wanted to understand whether you, that is, your organization, checks the engine when it is manufactured. When it is going to be reused, is there another test made, or is the test made at the

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Mississippi site?

DR. LOVINGOOD: If an engine is brought back on an orbiter to be flown again without any changeout of parts, then our assessment is made in terms of data that we get from the flight, and any anomalies that are found from post-flight inspections, which the inspections are done under the cognizance of KSC, but we get a report on that, and we have to disposition those anomalies before we fly it. In the case of a component changeout, we are responsible for the production of that new component, the acceptance test of it.

All components are acceptance tested by hot fire on a single engine.

DR. FEYNMAN: Thank you.

DR. LOVINGOOD: FPL is full power level, and that is 109 percent of the rated power level, and we have not flown at that power level yet. In fact, the improvements that we are making are to give us more margin in operating at 109 percent.

I want to point out the mixture ratio, which is the ratio of oxygen mass to hydrogen mass consumed by the engine is six, and down there, on life at the bottom of the chart, it shows that we have a specification requirement, and I want to emphasize that is the spec requirement of seven and a half hours or 55 starts, and

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that has not yet been demonstrated.

And I have a subsequent chart that shows you what we have demonstrated.

And then we have a controller which I think has been mentioned today, and we are capable of throttling with that controller to 65 percent minimum. The controller accepts commands from the general purpose computers in the orbiter, the GPCs, and then makes the engine valves operate to provide the proper throttle setting.

Some design features we have—

(Viewgraph.) [Ref. 2/6-59]

DR. LOVINGOOD:—is that we do have a failsafe philosophy. The controller has redundant computers which control the mixture ratio and the chamber pressure, and the controller includes self-check monitoring capability to ensure proper engine operation. The design features redundancy in the engine control and the monitoring functions, and we have red lines that are established based on both analytical work and ground test experience.

The engine operation has been demonstrated in our ground test program, both development and certification. We fly the engine with the maintenance parameters and so forth, just like we have done it in

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our ground test program. We have included off-nominal engine performance, and that is by varying the mixture ratio off of the nominal value of 6.0, and we have demonstrated various abort modes. There was some discussion of that this morning. We fired engines around 600 seconds, approximately, to demonstrate one of the abort modes, and approximately 800 seconds to demonstrate another abort mode. And we have also demonstrated off-nominal engine shutdown modes. Normally the shutdown, for example, is with hydraulic power from the auxiliary power units which Arnie mentioned, which are on the orbiter. In case of an emergency, we do have a pneu-

matic shutdown system using a helium supply on the orbiter, and we have demonstrated that in ground tests.

And then, before we put an engine into the orbiter or a component, replace a component and install a new one in an engine, we do hot fire acceptance tests of those engines, as I have already mentioned.

(Viewgraph.) [Ref. 2/6-60]

CHAIRMAN ROGERS: Would it be possible to relate these functions to the Challenger?

DR. LOVINGOOD: Well, the way I would relate it to the Challenger is that we did go through the acceptance testing, our normal acceptance testing

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and data reviews, looking at any material discrepancies that come out of manufacturing to make sure that we didn't have any problem. This was done before we flew the Challenger for the first time with this set of engines In fact, on things like hardware discrepancies from the plant, we have what we call a re-review of those discrepancies. That is part of our flight readiness review process. So we do a very thorough review of the hardware that we are flying.

As far as manufacturing anomalies. We look at process changes that might have been incorporated, we look at all the acceptance test data, and if it is a reflight, then we do the review of the post-flight inspection data from the previous flight as well as the previous flight data, and we always acceptance test.

DR. FEYNMAN: For example, was this Challenger, the one we are interested in, the flight we are interested in, a reflight of an engine or a new engine?

DR. LOVINGOOD: I believe that this, all three engines were being reflown. I'm almost 100 percent sure of that. And I don't believe we changed out any major components, but I will get you that for the record because I am not certain, but I will give you exactly what component changeouts were made for this flight.

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CHAIRMAN ROGERS: Your records would show any anomalies in previous flights as far as these engines are concerned?

DR. LOVINGOOD: There could have been. We do have occasional anomalies which are sometimes dispositioned as being within our experience, something that shows up. We look at anything that looks unusual. In fact, sometimes people like to call them observations, but we always classify them as anomalies, and we thoroughly review those. So I am not certain. But I do know that whatever we saw in the data, that there is a documented rationale as to why that is no problem for flight.

CHAIRMAN ROGERS: In other words, you do have records to show any anomalies as far as Challenger is concerned?

DR. LOVINGOOD: That is correct.

CHAIRMAN ROGERS: Thank you.

GENERAL KUTYNA: Yet on this particular flight, we had less instrumentation than on previous flights, but these engines are very well instrumented, aren't they, to the point where if you saw an anomaly on climb-out, it would have registered and possibly even shut the engine down before anything disastrous occurred?

DR. LOVINGOOD: The instrumentation on this

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flight would be like it has been. It would be as much and in some cases more than we have had on previous flights. I don't think—we haven't subtracted any recently, and we have added some instrument data.

GENERAL KUTYNA: Did you see anything anomalous on climb-out on these engines?

DR. LOVINGOOD: No.

MR. RUMMEL: The engines would not have been shut down until after the accident occurred, if I understood this correctly this morning, is that right?

DR. LOVINGOOD: That is correct. The nominal shutdown time is around 500 seconds.

GENERAL KUTYNA: Let me push that point. Had there been an anomalous condition on the engine, it would have shut itself down prior to having anything disastrous happen?

DR. LOVINGOOD: There are red lines, and for the record, I will tell you what those red lines are. I have got a list of them here.

GENERAL KUTYNA: For example, we had an engine shutdown on the previous flight. It sensed something going wrong and it shut itself down before there was any problem.

DR. LOVINGOOD: Yes. I will tell you what

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that was. We have red lines on fuel, the high pressure fuel pump turbine discharge temperature. We have two temperature sensors in the discharge of that turbine, and the red line is set at 1960 degrees, roughly. It is actually different on the two gauges because of the different coolant flow we have in there.

We also have red lines on the turbine discharge temperature on the high pressure oxygen pump. We have a coolant liner pressure red line in the fuel pump turbine. There is a fuel pump turbine coolant liner that has a red line in it on pressure. We have an intermediate seal pressure that is—the seal that separates the hot gases of the turbine from the LOX that we are pumping, this is on the high pressure LOX pump, and we have a high pressure LOX pump drain pressure as a red line. Those are the five red lines we have.

Now, the problem that led to the engine shutdown in flight was a failure of two of the two temperature sensors that we have, and we have a way that the controller monitors those temperature sensors to determine whether they are good sensors or not, whether they are qualified. If the determination of the controller is that they are not qualified—and it is based on the failure rate, how fast the sensor goes off-scale high—then the controller disqualifies that

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sensor, and that sensor would not vote to cut.

What happened in the previous case when we had the shutdown was that the failure mode of that sensor was such that the controller did not recognize it as a bad sensor, and recognized it as a vote to cut, and so we ended up shutting the engine down.

GENERAL KUTYNA: But the bottom line is you really have a fail-safe system as far as those engines shutting themselves down.

DR. LOVINGOOD: That is correct. We have got redundancy, and it is fail-ops with the first one, and then fail-safe.

Okay. I think I was on Figure 11. [Ref. 2/6-60]

Prior to flight we have a ground certification test program and I have indicated here how we go about doing that. The current engines that we are flying were initially certified for ten missions, and that is taking two samples, two builds of that engine, and running through what

we call two CERT cycles, and a CERT cycle consists of 5,000 seconds of testing in 13 starts, and those CERT cycles represent the kind of mission profiles that we would fly in the missions.

If you add all that up for just one engine, that would be two cycles times 5,000 seconds, that would be 10,000 seconds, about roughly 500 seconds per

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flight. That would be equivalent to 20 missions, and what we do is we divide that by two, we allow our ground test program to exceed flight by a factor of two. So in running 20 missions on the ground on two engines, we certify ready to fly ten missions.

When we change, make an engineering change in a component, generally or typically we require two samples of that, one CERT cycle, and say that qualifies the component for ten missions operating in that engine system that we have already got certified. But each change receives a thorough review by both NASA and the contractor to decide what kind of certification requirements there should be, and that is put into—that is documented in the paperwork, and that is a requirement that we complete that certification requirement or we must get a waiver with supporting rationale, if we do not, before we fly.

And then the ground test program develops parameters that we use in our maintenance, the post-flight inspections that we use, the inspection intervals, and then any removal and replacement schedule based upon life limits on certain piece parts in the engine that we know have a life limit which is less than what we have certified the basic engine for. And all of that, of course, is documented and it is documented in

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our files as well as at KSC as far as what all of those between-flight inspections, maintenance and removals are.

And then this last bullet just says that we use a factor of two in our ground test over our flight.

(Viewgraph.) [Ref. 2/6-61]

DR. LOVINGOOD: And then the next chart shows with our current engines that we are flying, with our ground test program using a factor of two, we are certified to fly 15 flights for each engine that we put into the field at a mixture of 100 and 104 percent of rated power level.

This program did include some testing at 109 percent, some certification testing at 109 percent, and that certifies us to fly seven flights of that 15 at 109 percent.

And then the last two bullets down there just shows that we did that on Engine 2010 and Engine 2014 with a ground test of 40 missions on Engine 2010 and 30 missions on 2014, and then we take the smaller of those two numbers and divide by two to get to 15.

CHAIRMAN ROGERS: Is there anything about the testing of the Challenger engines that caused you any concern or which seemed to be different than previous tests?

DR. LOVINGOOD: No, I don't recall anything.

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In fact, when the question came up this morning concerning this launch, I was trying to think, the question was asked whether there was anything other than weather considerations that made you more concerned, and that went through my mind at that time, and I don't know of anything off hand.

VICE CHAIRMAN ARMSTRONG: Do I understand correctly that 2010 and 2014 are engines that are used for testing only?

DR. LOVINGOOD: Engine 2010 and Engine 2014 were new engines that we had in our planning to use as ground certification engines, and that is a very controlled program. We don't do

development testing on those engines. If we've got a new part, we don't put it on there. It is a very controlled program, and we use the same specifications, so to speak, as far as maintenance and inspections are concerned, that we use when we fly. I mean, that is the intent of that, to fly the same way we do that certification program.

VICE CHAIRMAN ARMSTRONG: It is functionality and reliability kind of testing?

DR. LOVINGOOD: Exactly.

VICE CHAIRMAN ARMSTRONG: Thank you.

DR. LOVINGOOD: Okay.

That is really all I had to say about the

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engine.

I wanted to talk about this propulsion system test that we do, and that is what Figure 13 is relating to. This test, which is done on a test stand down in Mississippi, which includes a flight type external tank, and it has got the orbiter aft structure simulated, but it has got all the valves and the plumbing in the aft end of the orbiter for the propulsion system. And then it has got a cluster of three main engines on it.

Before we flew the first time we performed 12 successful tests in the time period I have indicated there, and then we also performed these, in addition to the static firings and hot firing, we performed the special propellant tanking test which had to do with loading procedures at KSC. And then our current plan, we have not run a test of that cluster at 109 percent. So the current plan is to run two static firings at 109 percent of rated power level, and then after we complete that we intend to convert that to another single engine test stand and convert that facility to another single engine facility.

That is all I plan to say about the engines and the main propulsion tests, and if there are no questions, I will go on to the solid rocket booster.

DR. FEYNMAN: I would like to know a little

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bit more about the actual engines used on the Challenger. What new items had to be replaced after the engines had been used; if the engine is a reused engine, were there some parts that had to be replaced, or what kind of condition is it in relative to were there some special problems?

DR. LOVINGOOD: Are you talking about this particular flight?

DR. FEYNMAN: Yes.

DR. LOVINGOOD: I don't recall for sure. My recollection is—I have got to get that data. I recall that we didn't change anything, but I will provide that data to you, and if we changed out anything, I will tell you why we changed that out.

(Viewgraph.) [Ref. 2/6-62]

DR. LOVINGOOD: On the solid rocket booster, there were several questions raised this morning about that. Let me see, I have got some notes here.

We did make a change—well let me talk about the booster description first.

Go to Figure 15, and then I will try to respond to some of the questions that came up during Jess and Arnie's discussions.

(Viewgraph.) [Ref. 2/6-63]

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Figure 15 shows an expanded view of the booster, and starting on the lefthand side of that chart and working your way across, you will note that we have the nosecap, which contains the pilot and drogue chute, and then we have the frustum, which contains the three main para-

chutes. We have the forward skirt, which has the forward attach fitting to the external tank, and we also have avionics.

Then the next, moving on across there after the forward skirt, to the right of the forward skirt is the forward segment, and that is a motor case segment that is cast as shown there. In that configuration that was 327.5 (on the forward segment) inches long, and it has a forward bulkhead which is a pressure dome, and then we have what we call the forward mid segment or forward center segment, and then the aft mid segment, and then the aft segment, and the aft segment is shown there with a nozzle attached to it. And then we have the aft skirt, which contains the separation module, the thrust vector control system, and I think Arnie pretty well discussed that today.

Now, these segments are transported overland, and assembled by KSC. They are transferred from Morton-Thiokol in Wasatch to Kennedy Space Center, and the assembly is done there. I think the next chart

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shows who the contractor——

GENERAL KUTYNA: Would you point out the previous problem you had with this booster, with the SRB and explain how you fixed that? What gave you confidence that that problem would not reoccur? You had problems with the nozzle and your burnthrough of the nozzle, as I recall.

DR. LOVINGOOD: We had on—it was STS-8. We had some pocketing in the nozzle, and that was—I don't recall exactly where it was. I think it was on the throat inlet. It was prior to the throat, upstream of the throat.

And we had made a process change prior to that time. We went back to our old process, and there was also some suspect material, a particular manufacturer of material, and we had extensive analysis and test data which supported the fact that that particular supplier of this material might have had volatiles in there or other parameters which could have led to this pocketing.

CHAIRMAN ROGERS: I don't understand that. Could you explain it a little to me? It doesn't have much meaning to me.

DR. LOVINGOOD: Well, I am not sure I can explain it.

GENERAL KUTYNA: Maybe we should try and say

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you had air pockets in the material. Is that right?

DR. LOVINGOOD: Well, there were gases. I think there was just the chemical constitution of the materials, too, that indicated that this one supplier had components in there or constituents which were not good as far as this pocketing problem is concerned. The gas pocket, I think, is one of the things that led to the mechanism, and I am not familiar with the mechanism.

What I suggest we do, we could give you a detailed briefing, because that is all documented, and if you would like, we can give you a detailed briefing on exactly what we found.

GENERAL KUTYNA: As far as NASA is concerned, that problem is resolved? You found the problem was not a factor in this particular incident?

DR. LOVINGOOD: Thus far we don't see that as being a factor.

MR. HOTZ: Did you change manufacturers?

DR. LOVINGOOD: No. When I mentioned something about a supplier, we had two suppliers of this material, and the analysis showed that this one supplier's product was better, and we are using strictly that supplier's product. So there is no change.

MR. HOTZ: No, but you did drop a supplier, then?

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DR. LOVINGOOD: In that particular area of the nozzle.

CHAIRMAN ROGERS: Was that based upon negligence of the supplier?

DR. LOVINGOOD: No. It was within specification. And I think that it was just on one side of the spec in the way he had been manufacturing it, and we felt like if we could eliminate that, and we did go back to our old process, too, for curing the nozzle, and doing that, we could eliminate a problem, and we haven't had a recurrence like we did on that flight.

CHAIRMAN ROGERS: Is there a report on that? Did you make an inquiry and file a report on that whole incident?

DR. LOVINGOOD: Yes, we can get you a report.

CHAIRMAN ROGERS: And that is available to the Commission, I presume.

DR. LOVINGOOD: Yes.

CHAIRMAN ROGERS: Thank you.

DR. WALKER: Are you planning to discuss the way in which these sections are joined?

DR. LOVINGOOD: I had not planned to go into any detail on that. This is the aft attach ring to the external tank, and I think that was mentioned by Arnie. There is a field joint approximately right here, and the

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field joint is what we call the joint between two segments that are cast individually, separately, and that joint is made at KSC.

There are factory joints. These segments, I believe, this particular segment here is about 27 feet long, so about halfway up, 13 and a half feet or so, there is a factory joint that is made at Thiokol. These have two O-rings in the joint. When you have a field joint, we have inhibitors there that inhibit the propellant burning on the face at that joint.

In the case of a factory joint, we have insulation that comes all the way across that, and we don't use the inhibitor.

GENERAL WALKER: Are these VITON O-rings?

DR. LOVINGOOD: I am not sure. I believe they are, but I am not certain. Yes, that is correct.

What we can do is, I had not planned to focus—the instructions I had for this was to just give you an overview. I had not planned to focus on any particular area, and that is why I am not prepared to do that.

CHAIRMAN ROGERS: Well, we can come back to that. We appreciate that we didn't give you much notice of the meeting, and so, proceed. We will be able to get that information.

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GENERAL KUTYNA: How about the operating limits on this motor? Are you the proper one to discuss that?

DR. LOVINGOOD: What is the question?

GENERAL KUTYNA: How about the operating limits on this motor? It says in the manual that it ought to operate between about 40 and 90 degrees Fahrenheit. Of course, it was a lot colder than that.

DR. LOVINGOOD: The requirement is on propellant mean bulk temperature, and in fact I had that on a chart that it is one of our requirements, and it was predicted that the mean bulk temperature would be 55 degrees at launch, and it has been reported to me that that is what it was, about that value. So we do have a requirement to be between 40 and 90, and we were within that range.

DR. KUTYNA: Do you have instrumentation that would give you that temperature, or do you predict it, or how do you know that the mean bulk was what it was?

DR. LOVINGOOD: It is calculated based upon ambient.

(Viewgraph.) [Ref. 2/6-64]

DR. LOVINGOOD: Okay, Figure 16 shows the major suppliers on the booster. Of course, the motor is made by Morton-Thiokol. The booster assembly is—United Space Boosters Production Company, they are

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currently called, does the assembly work of the aft skirt and of the forward skirt and the parachute frustum area and the nosecap, and I have got the suppliers down there for structures of the motors and so forth, and you can read through that list, and then we have done our testing at Morton-Thiokol's Wasatch Division as far as the large motor static firings are concerned. All that testing was done out there, and this is just a highlight, by the way. And then at Marshall Space Flight Center, we have done the structural testing on the booster, and also TPS, Thermal Protection System development and testing.

DR. WALKER: Could I just ask a question on terminology? Solid rocket motor refers to the fuel itself?

DR. LOVINGOOD: Let me show you on the next chart.

(Viewgraph.) [Ref. 2/6-65]

DR. LOVINGOOD: What I have got here, I thought this was going to be in color, but the solid rocket motor, our terminology for that is the part that is the responsibility of Morton-Thiokol, and that would be all of the segments from this forward bulkhead back including the nozzle, and this includes the casting of the propellants into those sections, and then there is

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also a systems tunnel that runs along the motor case, and that is a Morton-Thiokol responsibility, so we call all this the solid rocket motor.

Now, when we put the aft skirt on with the thrust vector control system and the avionics, booster separation motors, when we add—which is a USBPC responsibility, and then when we add the forward skirt, the frustum and the nosecap with the parachutes, the avionics, the separation motors, and so forth, we call that whole assembly a solid rocket booster. So then there are two of these per mission. Does that explain our terminology?

DR. WALKER: Yes.

(Viewgraph.) [Ref. 2/6-66]

DR. LOVINGOOD: The next chart I don't plan to dwell on. It shows the characteristics. The main point there is, we have a mean thrust of 2,400,000 pounds per booster.

(Viewgraph.) [Ref. 2/6-67]

DR. LOVINGOOD: Then the next chart, Figure 19, I do have a thrust time trace that I will show you which is a typical trace, and that will come up next, but I want to leave this chart up here until I get ready to talk about that, and I will show you how that is specified as a requirement. And then we have a thrust

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vector requirement to be able to gimbal the nozzles for control during the first stage boost of plus or minus 88 degrees, and these were qualified with five development test static firings out at Thiokol and with four qualification test firings.

Then I have just listed, and I am just trying to highlight here sort of our approach on this motor. The structural integrity, as far as the design criteria is concerned on the hardware, we

have a 1.4 time limit load. That is, the limit load is the maximum predicted load from pressures, aerodynamics, engine thrust that you will see in flight, and we take a load 40 percent higher than that, and then that is what we design to, and then we do an ultimate load test, testing that structure to that value to make sure that it doesn't break.

And then on the propellant we have got a factor of two times the maximum expected load; and we verified that with subscale test and analysis. And on the insulation, the case insulation, we have a 1.5 factor times the predicted erosion, and on the nozzle insulation it is a factor of two times the predicted erosion.

DR. FEYNMAN: Excuse me. Predicted erosion is predicted erosion. The question is, in your experience

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in measuring erosion, how much variation from predicted erosion is the average degree of variation to be expected? How good is the prediction?

DR. LOVINGOOD: The prediction is real good, with the exception of one case that we talked about earlier where we had that pocketing, and we are staying pretty much right in that same area.

DR. FEYNMAN: What is real good, 5 percent, 10 percent?

DR. LOVINGOOD: Like on the nozzle with a factor of two. That means you know you are good for another flight. You would have been good for another flight. I think we may have come off just a little bit.

DR. FEYNMAN: How good are the predictions for the amount of erosion, 5 percent accurate, 10 percent accurate?

DR. LOVINGOOD: I would say within 10 percent. It may be more accurate than that. Okay, and then I have got listed on this chart the fact that we do have a design environment for the propellant mean bulk temperature, a range of between 40 and 90 degrees.

DR. RIDE: Can I ask you a question just, I guess, relate it to the design environments? You must have a set of launch commit criteria for the SRBs and

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the motors. Could we get those available to us, or do you have them?

DR. LOVINGOOD: Yes, I will make a list, and that was one of the questions that came up this morning. We do have an LCC, Launch Commit Criteria, on some temperatures in batteries, and I don't know what those values are or what the particular batteries are. We have got them on batteries. We have got them on the tank. We have got a nose cone temperature limit, but we can provide you a complete listing of that, and of course you know what that means. That means if you violate the LCC you don't launch or you get a waiver with supporting rationale which is documented in order to go ahead with the launch. So, we can provide a list of those.

CHAIRMAN ROGERS: Can you determine the temperature of the booster, inside temperature of the fuel in the booster?

DR. LOVINGOOD: Just by calculation. We don't have any measurement.

CHAIRMAN ROGERS: No instrument?

DR. LOVINGOOD: No.

MR. ACHESON: May I ask, in the design environment here, the temperature 40 to 90, does that mean it is designed to operate at that temperature, or

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does it mean that it is designed not to undergo a physical or chemical change within those temperatures?

DR. LOVINGOOD: I will have to get you an answer to that. I don't really know. I don't know what the genesis of that requirement is and what the design criterion is based on it. Let me mention here, too, something that came up this morning. We did do a motor case redesign. We reduced the wall thickness approximately 6 percent, the wall thickness.

CHAIRMAN ROGERS: You are speaking about the Challenger now?

DR. LOVINGOOD: Yes, but I will tell you now we made that change on STS-6, which means that we have had 18 flights, successful flights, if I did my arithmetic right, and of course that is two boosters, so 18 times two is 36, but we did make a change, and we reduced the wall thickness about 6 percent to lighten the case weight, and we did motor firings. We also did two motor firings, one development motor firing and one quality motor firing.

We didn't do it in order to certify that redesign. We did it because we made some additional—another change to get more performance out of the motor. We call it our high performance motor, and that was effective on STS-8, and what we did there was to

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decrease the nozzle diameter by—I think it was a half an inch or quarter of an inch. No, a half an inch. I have got to get that for you. I forgot. And we extended the nozzle ten inches.

We also changed—we cut back on the inhibitor in the radial direction. We made the inhibitor less in order to get higher thrust at liftoff. So we made those changes, and we can provide you exactly what we did for the record, what those changes were.

DR. FEYNMAN: What is the inhibitor, a liner of some kind that goes around the propellant?

DR. LOVINGOOD: The inhibitor is at the field joints, where we cast the propellants in separate segments, and then the inhibitor is there to keep the surface, let's say the forward facing surface of the propellant from burning, and it is NBR. It is an NBR rubber, the material.

DR. WALKER: Does that inhibitor form a seal between adjacent sections?

DR. LOVINGOOD: No, it is not a seal. It is an insulation protection for the face of the propellant.

DR. WALKER: So the four sections of propellant are really separate entities?

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DR. LOVINGOOD: That is right.

DR. WALKER: They don't connect with one another? They burn separately?

DR. LOVINGOOD: Well, it burns from the inside out, is the way the solid rocket motor burns, so all the segments are burning simultaneously that way.

DR. WALKER: But each is burning separately?

DR. LOVINGOOD: Yes, that is correct.

GENERAL KUTYNA: Jud, do you have a slide of the joints where these segments are joined? Do you have the technical detail of that?

DR. LOVINGOOD: No, I had planned to have some detail as backup but we didn't have backup for this briefing. That is the normal way we do things, and I thought it might come up.

GENERAL KUTYNA: Have you looked at these post-mission after you recovered them from the ocean to see if there is any damage at those joints from the previous flights?

DR. LOVINGOOD: We have seen some evidence of what we call blow by of those seals, some erosion of those seals. The primary seal. We have never seen any erosion of a secondary seal, but we have seen evidence of soot in between the two seals.

GENERAL KUTYNA: Was that any cause for

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concern?

DR. LOVINGOOD: Oh, yes, that is an anomaly, and that was thoroughly worked, and that is completely documented on all the investigative work that we did on that, and we can get that for you.

CHAIRMAN ROGERS: If a committee or subcommittee of the Commission visits your operation, would you have the information there that you could answer specific questions about this more conveniently, and particularly about the Challenger as distinguished from the overall operation?

DR. LOVINGOOD: Yes, we would have more data there that we could get, plus we would have our experts in these areas that could talk much more intelligently than I can on the subject.

CHAIRMAN ROGERS: Well, we do not expect you—I mean, we understand that you didn't have much notice, and that you were to give an overview so you don't have to be apologetic, but we are just trying to figure out how to get the information ourselves, and that certainly would be one way we could do it, isn't it?

DR. LOVINGOOD: Yes, I think that would be a way.

CHAIRMAN ROGERS: Thank you.

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(Viewgraph.) [Ref. 2/6-68]

DR. LOVINGOOD: The next figure shows the thrust time trace, and there is a higher thrust, as you can see, for about the first 20 seconds of flight, and then the thrust drops off. By the way, the two outside lines represent the band that we have to be within in order to achieve the proper performance on the motor, and these numbers here, I think the artist took a little license in the way this was plotted. These aren't exactly right. But anyhow, generally we lie right in the middle of that band. Sometimes we come up fairly close to the edge at some points.

But we have never to my knowledge gone outside of that band. So we have that kind of trace where the pressure drops down. The pressure here starts about 1,000 psi, and in this region here it drops down to about 600 psi chamber pressure, and then it starts back up nominally, and then it goes back down and then starts to tail off here, and then we separate when it gets to 50. That gives a signal to the GPCs to separate.

DR. WALKER: How uniform is the pressure inside of the motor?

DR. LOVINGOOD: I don't know. I know we have done a lot of analysis trying to understand, and I think early in the program we had some, maybe some acoustical

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measures, measurements up on the forward dome, but that is information that we could give you, and I really don't have good knowledge of that.

(Viewgraph.) [Ref. 2/6-69]

DR. LOVINGOOD: The next chart, Figure 21, shows some considerations that I thought would be worthwhile to put out here. The fact that since this is a manned space flight program, that our designed safety factors relative to other solid rocket motors have been applied differently as indicated on that chart.

Like on the structures, we have 1.4 times the limit load, which is the maximum expected flight load, and generally on military weapons systems that is 1.25 or 1.15, and then on the insulation 1.5 times the predicted requirement on the case, two times the predicted requirement on

the nozzle, and that is usually one and a quarter on military systems, and we do proof test all of these segments to 112 percent of their maximum expected flight pressure.

And what that amounts to is—that is 80 percent of the 1.4 safety factor, and that is the convention in solid rocket motor technology.

VICE CHAIRMAN ARMSTRONG: Excuse me. On what do you apply this 112 percent proof test?

DR. LOVINGOOD: Segments.

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VICE CHAIRMAN ARMSTRONG: Each production segment?

DR. LOVINGOOD: Each segment.

VICE CHAIRMAN ARMSTRONG: Thank you.

MR. RUMMEL: Does the term "limit load" apply to the ultimate strength of the material or the yield point or what?

DR. LOVINGOOD: That would be ultimate. That would be breaking up. The requirement is that you don't break up at less than 1.4 times the maximum expected load. You don't have an ultimate failure.

MR. RUMMEL: Thank you.

DR. LOVINGOOD: Then we have done x-ray and we did 100 percent x-ray of the propellant in the first 68 segments that were manufactured, and through that verified that the casting process that we were using provides proper propellant strength. Currently we use the process control that we verified with those 100 percent x-rays, and we do a random monthly x-ray of a segment, and then whenever we have a process anomaly or a process change or design change, then we do an x-ray for the segments, and then we still do a 100 percent x-ray of the nozzle ablator parts to be sure that there aren't any delams or voids or cracks.

MR. ACHESON: At what times are these x-rays

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taken in comparison—that is, in relation to the dates of delivery and flight of mission?

DR. LOVINGOOD: I will have to get you that information. I can get you the information on these specific segments that we flew. I am not even sure of the manufacturing time. They may not have been 100 percent x-rayed because it may have been after we instituted this random sampling, but I will give you a typical example of when the x-ray was taken and when it was flown.

DR. WALKER: Are the three forward segments interchangeable?

DR. LOVINGOOD: The forwardmost segment, and I am not familiar with exactly how we do all of these segment changes, but the length of the forwardmost segment is longer, and also you have got a dome, a forward dome on that segment. So there is not complete interchangeability between the segments, but when we take these back and refurbish them, we do wash out the propellants and the liners and start all over again and then remake the factory joint, and I am just not sure how we can interchange those.

DR. WALKER: Thank you.

DR. LOVINGOOD: But that is kind of data that we can provide to you.

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DR. WALKER: Thank you.

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Now, that is all I had on the booster. I want to comment that there were questions earlier—I think the gentleman who was asking the question has left. There was a question earlier about, I think he phrased it, a concern by Thiokol on low temperatures.

We did have a meeting with Thiokol. We had a telecom discussion with people in Huntsville, people at the Wasatch division, and people at KSC. And the discussion centered around the integrity of the O-rings under lower temperature.

We had the project managers from both Marshall and Thiokol in the discussion. We had the chief engineers from both places in the discussion. And Thiokol recommended to proceed on the launch, and so they did recommend the launch.

We had a meeting where there was some concern about the cold temperatures.

CHAIRMAN ROGERS: When was that meeting?

DR. LOVINGOOD: That was the 27th. That started around quarter to 5:00 central time.

(Viewgraph.) [Ref. 2/6-70]

DR. LOVINGOOD: Is there anything else on the booster?

CHAIRMAN ROGERS: I guess not.

DR. LOVINGOOD: Going on to the external tank

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project—

(Viewgraph.) [Ref. 2/6-71]

DR. LOVINGOOD: Arnie has talked a great deal already about this, and I think you realize that the LOX tank is forward, the oxygen tank is forward. And we have the inner tank, which has a large cross beam, which takes out the thrust from the SRMs. The SRMs are attached on the sides here to this large cross beam, and that is where all the thrust is reacted into the external tank, through the inner tank.

And then the hydrogen tank is the aft tank, which is separated from the oxygen tank by the inner tank area. And then we have the gaseous oxygen pressurization line that runs the length of the vehicle up to the top of the LOX tank.

We also have a cable tray that runs up to the top of the LOX tank, and that cable tray has wiring, wires, electrical wires, as well as it has a linear shaped charge in it. This feed line, the oxygen feed line, comes out of the inner tank. Well, it comes from the LOX tank into the inner tank and out at this point, and feeds down the side of the hydrogen tank external to the hydrogen tank, into the orbiter.

The hydrogen feed line comes directly out of the bulkhead in the orbiter. The hydrogen

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pressurization and the oxygen pressurization lines are just adjacent to that feed line, the oxygen feed line, and then the cable tray.

And then I think Arnie has already discussed the attach structure that we have back here. This aft ring is where the orbiter loads are reacted, plus this thrust longeron, and it goes up into this next forward ring, and then the SRB rear attach points come also into that aft ring.

MR. FEYNMAN: What is the purpose of the linear shaped charge?

DR. LOVINGOOD: That is range safety destruct in the event there is a problem.

MR. FEYNMAN: Where is it located?

DR. LOVINGOOD: It's in the cable tray. I'm not really sure where the charge starts, but it runs up the vehicle. And I'm not sure of the total length, but it is actually in that cable tray.

Okay, that is really all I wanted to say about the external tank. I think we have covered that.

MR. FEYNMAN: What loads is that designed to?

DR. LOVINGOOD: The external tank loads, when we first began the program we had a safety factor of 1.4 on all loads. We had a weight reduction program in which we took 8,000 pounds out of the tank, and we used various methods to get that 8,000 pounds out.

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At the time that we did that exercise and that engineering analysis, we had already done loads testing on the standard weight tank that we started out with, and so we knew the load paths very well. And so what we did was we took loads which we considered to be well-defined loads—for example, a pressure load—and we said that since we know that load so well and with our experience at that time, plus the structural testing that we had already done, plus the proof testing that we do of those tanks, that we would design that structure to 1.25.

The other structure, which is determined by thrust, gimballing loads or aerodynamic loads, wind loads, are still—that structure is still designed to the 1.4.

MR. SUTTER: When did the lighter tanks get into service?

DR. LOVINGOOD: STS-8.

Well, I guess the next chart I had on the tank. Of course, Martin-Marietta is the prime contractor, and we've got major subcontractors as listed there. And then we did a lot of the testing, most of the testing, at Marshall, including the structural tests and modal survey tests and various thermal protection system activities, as I've got indicated there.

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And I would say, too, that the requirements—and I don't think I have that on that chart. Do I have another chart?

(Viewgraph.) [Ref. 2/6-72]

Excuse me just a minute. I think I've got my charts all mixed up here.

Okay, chart 25. Well, this really answers the question, I guess, that was just asked. I've got down there three sigma loads, and that is a statistical term that doesn't mean anything. That's the maximum predicted flight loads. That is our requirement.

And there is a loads data book, and I think Dick Kohrs is going to talk about or Tom Moser I think is going to talk about how we do that as far as the requirements are concerned. But anyhow, it is designed to the maximum predicted flight loads, and then we do qualification tests to 1.25 and 1.4, as I just mentioned, depending upon the circumstance.

And then in that testing, we do it at cryogenic temperatures for the hydrogen tank and room temperature for the oxygen tank and the inner tank: and the propulsion system, as far as the interface requirements and delivering the proper propellants to the orbiter and to the main engine, is qualified by testing that we do.

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But in particular, the main propulsion tests, which I have already mentioned, we have run 12 of them. And then thermal protection system: that is there to maintain the propellant quality, to make sure you've got proper temperatures for engine operation and avoid propellant boil-off and that sort of thing; to thermally protect the structure in certain areas, areas of high heating, like for example that we have an ablater underneath the LOX feed line over where we do have external mold line protuberances.

And then also limit ice formation to prohibit damaging the orbiter, and we have qualified that through wind tunnel testing, both combined environments and also putting plasma arc heat sources on there to make sure that we've got the proper recession ranges.

CHAIRMAN ROGERS: As far as previous flights are concerned, has the external tank been successful or has it been a source of trouble, generally speaking?

DR. LOVINGOOD: The external tank I personally feel like we have had very good success with. We have had some problems with some pressure transducers, and these are just fairly rare occurrences.

I think we have had like two LOX LH transducer bias shifts, just very small changes.

CHAIRMAN ROGERS: What does that mean?

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DR. LOVINGOOD: Well, we have in the oxygen tank, we have four pressure transducers that measure the amount of pressure that is in the tank, and then those pressure sensors are used to control the gaseous oxygen control valves on the orbiter. And on the hydrogen tank, we also have pressure transducers, and they control gaseous hydrogen control valves, or they feed back information as to the pressure and then those valves open or close based upon what the pressure is.

The problem we have had is that we have had some—when we sit at one tanking load for a long period of time, the sensors tend to vibrate. And we're not really sure what the cause of it is, and we've found that the vibration is causing perhaps shorting between lines or contamination between the wiper and the coil.

And it has given us like a tenth of a psi or a half a psi offset. The main concern here is that we will violate a launch commit criteria, because we have—at T minus 31 seconds, we have to have three of these transducers before we go.

And so, we never have really considered that to be a problem as far as safety in flight was concerned.

DR. WALKER: Can I ask a question about venting? Are there vent valves when the tank is sitting

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on the launch pad?

DR. LOVINGOOD: Yes. But I would like to defer that question to Bob Sieck. I think he could answer it much better than I could, about what happens on the pad.

CHAIRMAN ROGERS: Okay. If there are no further questions, thank you very much.

MR. MOORE: Mr. Chairman, we will, at your request, provide you any of the detailed briefings on the specific elements of this, at the center or wherever you need, to get more detailed information on the 51-L situations of hardware.

I asked our people to make sure that they gave the Commission today a good oversight and an overview of what each of the elements of the shuttle was.

CHAIRMAN ROGERS: Well, I'm sure that all Commission members understand that. And as I said, we appreciate the fact that you have been able to assemble all of this information on such short notice. So please don't be apologetic for not being able to answer all of these questions, which we'll have plenty of opportunity to ask later on.

MR. MOORE: Thank you, sir.

Next I would like to continue on with a major element of our program. That is the grounds operations

work and getting ready for launch. The activities that you will see presented here by Mr. Robert Sieck, our Director of Shuttle Operations at the Kennedy Space Center, will be applicable to STS 51-L, as they are in terms of how we process all of the particular flights.

So, Bob.

(Witness sworn.)

AGENDA

- OVERVIEW
- SPACE SHUTTLE MAIN ENGINE PROJECT
- SOLID ROCKET BOOSTER PROJECT
- EXTERNAL TANK PROJECT

[Ref. 2/6-51]

MSFC SPACE SHUTTLE ROLE

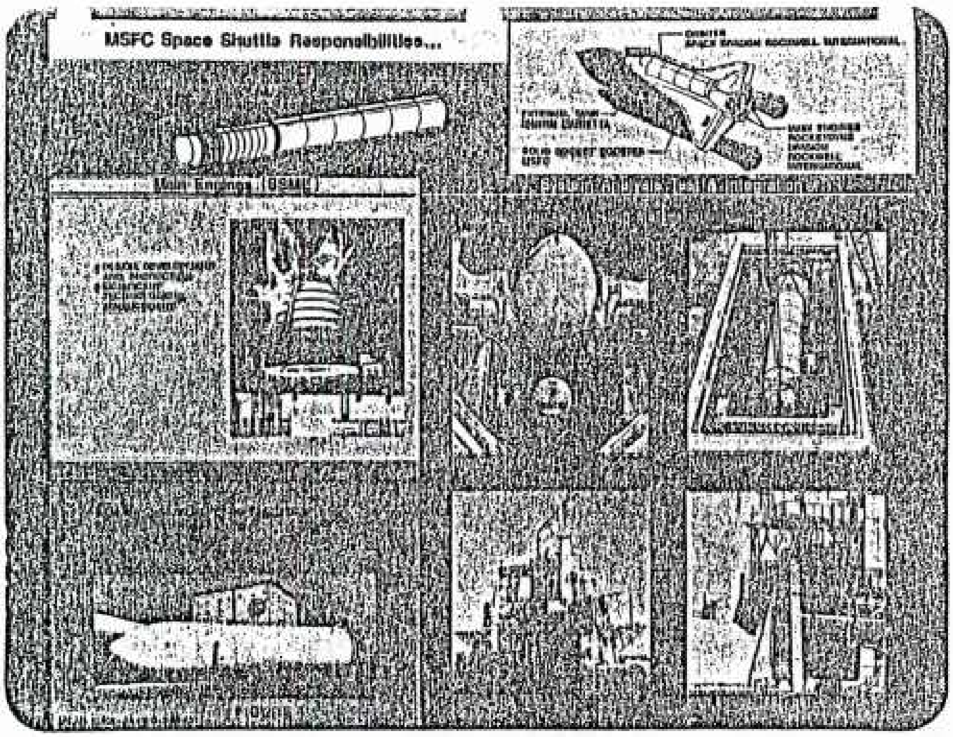
CAPABILITY DEVELOPMENT

- DEVELOPMENT AND CERTIFICATION
  - EXTERNAL TANK
  - SOLID ROCKET BOOSTER
  - SPACE SHUTTLE MAIN ENGINE
- PROPULSION SYSTEM TESTING
- PROPULSION AND ASCENT FLIGHT SYSTEM INTEGRATION
- PERFORMANCE IMPROVEMENTS
- PRODUCTIVITY

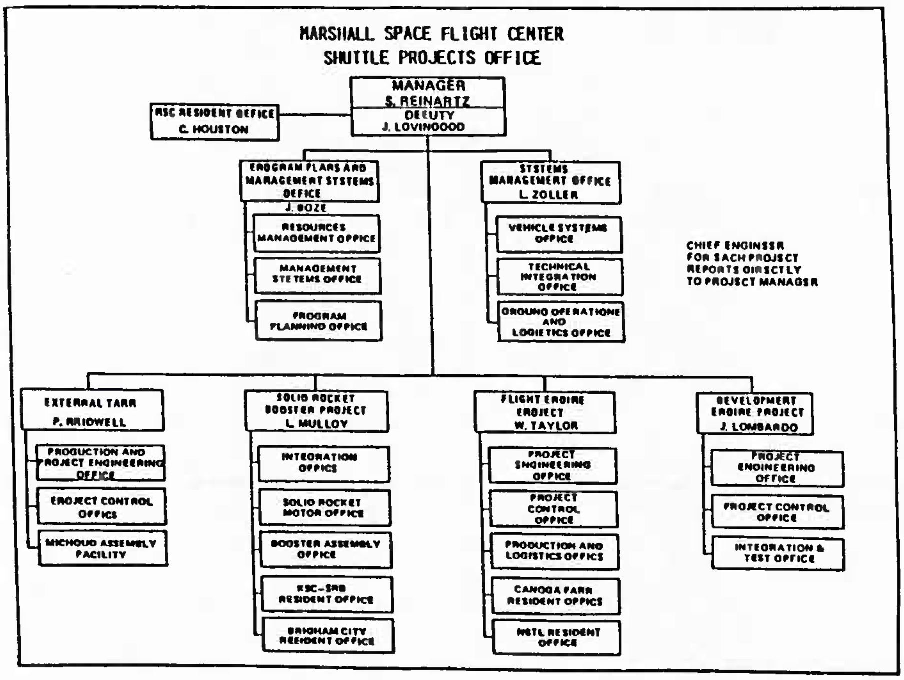
SUPPORT TO OPERATIONS

- PRODUCTION AND LOGISTICS SUPPORT OF MAJOR ELEMENTS – ET, SRB, & SSME
- LAUNCH SUPPORT FOR KSC AND VAFB
- VAFB ACTIVATION
- OPERATIONAL IMPROVEMENTS
  - PRODUCIBILITY
  - REQUIREMENTS REDUCTION
  - LAUNCH PROCESSING SIMPLIFICATION

[Ref. 2/6-52]



[Ref. 2/6-53]



MARSHALL SPACE FLIGHT CENTER
SHUTTLE PROJECTS OFFICE
MANAGER
S. REINARTZ
DEEUTY
J. LOVINOOOD
RSC RESIDENT OEFICE
C. HOUSTON
EROGRAM FLARS ARD MANAGEMENT STSTEMS OEFICE
J. BOZE
RESOURCES MANAGEMENT OFFICE
MANAGEMENT STETEMS OFFICE
FROGRAM PLANNING OFFICE
STSTEMS MANAGEMENT OFFICE
L. ZOLLER
VEHICLE SYSTEMS OFFICE
TECHNICAL INTEGRATION OFFICE
GROUND OPERATIONE AND LOGIETICS OFFICE
CHIEF ENGINSSR FOR SACH PROJSCT REPORTS OIRSCTLY TO PROJSCT MANAGSR
EXTERNAL TARR
P. RRIDWELL
PRODUCTION AND PROJECT ENGINEERING OFFICE
EROJECT CONTROL OFFICS
MICHOUD ASSEMBLY FACILITY
SOLIO ROCKET BOOSTER PROJECT
L. MULLOY
INTEGRATION OFFICS
SOLIO ROCKET MOTOR OFFICE
BOOSTER ASSEMBLY OFFICE
KSC-SRB RESIDENT OFFICE
BRIGHAM CITY REEIDENT OFFICE
FLIGHT ERGIRE EROJECT
W. TAYLOR
PROJECT SNGINEERING OFFICE
PROJECT CONTROL OFFICE
PRODUCTION AND LOGISTICS OFFICS
CANOGA PARR RESIDENT OFFICS
NSTL RESIDENT OFFICE
DEVELOPMERT ERGIRE PROJECT
J. LOMBARDO
PROJECT ENGINEERING OFFICE
PROJECT CONTROL OFFICE
INTEGRATION & TEST OFFICE

[Ref. 2/6-54]

SPACE SHUTTLE MAIN ENGINE (SSME) PROJECT

[Ref. 2/6-55]

SPACE SHUTTLE MAIN ENGINE

- **MSFC RESPONSIBILITY**

  RESEARCH, OEVELOPMENT ANO PROOUCTION OF THE SPACE SHUTTLE MAIN ENGINE, A HIGH PERFORMANCE, REUSABLE, THROTTLABLE ENGINE FOR THE ORBITER. THE 470K THRUST ENGINE BURNS LIQUIO HYOROGEN ANO LIQUIO OXYGEN.

- **PRIME CONTRACTOR**
  - ROCKETOYNE OIVISION, ROCKWELL INTERNATIONAL CORPORATION

- **MAJOR SUBCONTRACTORS**
  - HONEYWELL, INC. – CONTROLLER
  - HYORAULIC RESEARCH,INC. – HYORAULIC ACTUATORS FOR ENGINE VALVE CONTROL

- **TEST SITES**
  - NATIONAL SPACE TECHNOLOGY LABORATORIES – 2 STANOS
  - SANTA SUSANA FIELO LABORATORY – 1 STANO

[Ref. 2/6-56]

FLIGHT ENGINE FLOW

- MANUFACTURED AT ROCKETDYNE, CANOGA PARK, CALIFORNIA
- ACCEPTANCE TESTED AT MSFC TEST AREA AT NATIONAL SPACE TECHNOLOGY LABORATORIES (NSTL), MISSISSIPPI
- INSTALLED INTO ORBITER AT KSC, FLORIDA
- LAUNCHED AT KSC
- BETWEEN FLIGHT MAINTENANCE AT KSC

[Ref. 2/6-57]

[NOT REPRODUCIBLE]

[Ref. 2/6-58]

DESIGN

- FAIL OPERATE/FAIL SAFE PHILOSOPHY
- CONTROLLER REOUNOANT COMPUTERS CONTROL MIXTURE RATIO ANO CHAMBER PRESSURE
- CONTROLLER INCLUOES SELF CHECK MONITORING CAPABILITY TO ENSURE PROPER ENGINE OPERATION
    - DESIGN FEATURES REOUNDANCY IN THE ENGINE CONTROL ANO MONITORING FUNCTIONS
    - REDLINES ESTABLISHEO BASEO ON ANALYSIS ANO GROUND TEST EXPERIENCE
- ENGINE OPERATION OEMONSTRATED IN GROUNO TEST/CERTIFICATION PROGRAM
    - OFF NOMINAL ENGINE PERFORMANCE DEMONSTRATED
    - DEMONSTRATEO ABORT MODES
    - OEMONSTRATEO OFF NOMINAL ENGINE SHUTOOWN MODES
- ENGINES ARE ACCEPTANCE TESTEO (HOT FIREO) BEFORE VEHICLE INSTALLATION

[Ref. 2/6-59]

SSME CERTIFICATION
VALIDATION OF HARDWARE FOR FLIGHT

- DESIGNS VERIFIED BY GROUNO CERTIFICATION PRIOR TO FLIGHT
- BASIC ENGINE CERTIFICATION- 10 MISSIONS
    - 2 SAMPLES – 2 CERT CYCLES 5000 SEC. EACH ≈ 20 MISSIONS
- COMPONENT CHANGE CERTIFICATION
    - 2 SAMPLES – 1 CERT CYCLE 5000 SECS ≈ 10 MISSIONS
- GROUNO TEST (INCLUOE CERTIFICATION) PROGRAM OEVELOPS PARAMETERS FOR
    - MAINTENANCE
    - INSPECTION
    - REMOVAL AND REPLACEMENT
- ENGINE GROUNO LIFE CERTIFICATION PROGRAM (INCLUOING ENGINE MAINTENANCE ANO COMPONENT REMOVAL ANO REPLACEMENT OUE TO LIFE LIMITS) LEAOS THE FLIGHT PROGRAM BY A FACTOR OF 2 FOR ENGINE HOT FIRE OPERATING TIME.

[Ref. 2/6-60]

CERTIFICATION AND LIFE CERTIFICATION EXTENSION PROGRAMS
PHASE I ENGINE RESULTS

CERTIFICATION

- CURRENT FLIGHT ENGINES WITH LINE REPLACEABLE UNIT CERTIFIED FOR::
  - 15 FLIGHTS AT 100/104%
  - 7 FLIGHTS AT 109% RATED POWER LEVEL

LIFE CERTIFICATION EXTENSION

- ENGINE 2010 COMPLETED EQUIVALENT OF 40 MISSIONS
- ENGINE 2014 COMPLETED EQUIVALENT OF 30 MISSIONS

[Ref. 2/6-61]

MAIN PROPULSION TEST PROGRAM

SUMMARY

PURPOSE:

TO VERIFY THE DESIGN AND PERFORMANCE OF THE INTEGRATED MAIN PROPULSION SYSTEM AND VERIFY SYSTEMS INTERFACE COMPATIBILITY WITH RELATED FLIGHT SUBSYSTEMS AND SPECIFIC GROUND SERVICING SYSTEMS.

HISTORICAL:

- CONDUCTED 12 SUCCESSFUL STATIC FIRINGS
  - APRIL 1978 THROUGH JANUARY 1981
- PERFORMED 6 SPECIAL PROPELLANT TANKING TESTS
  - FEBRUARY 1981 THROUGH MAY 1983

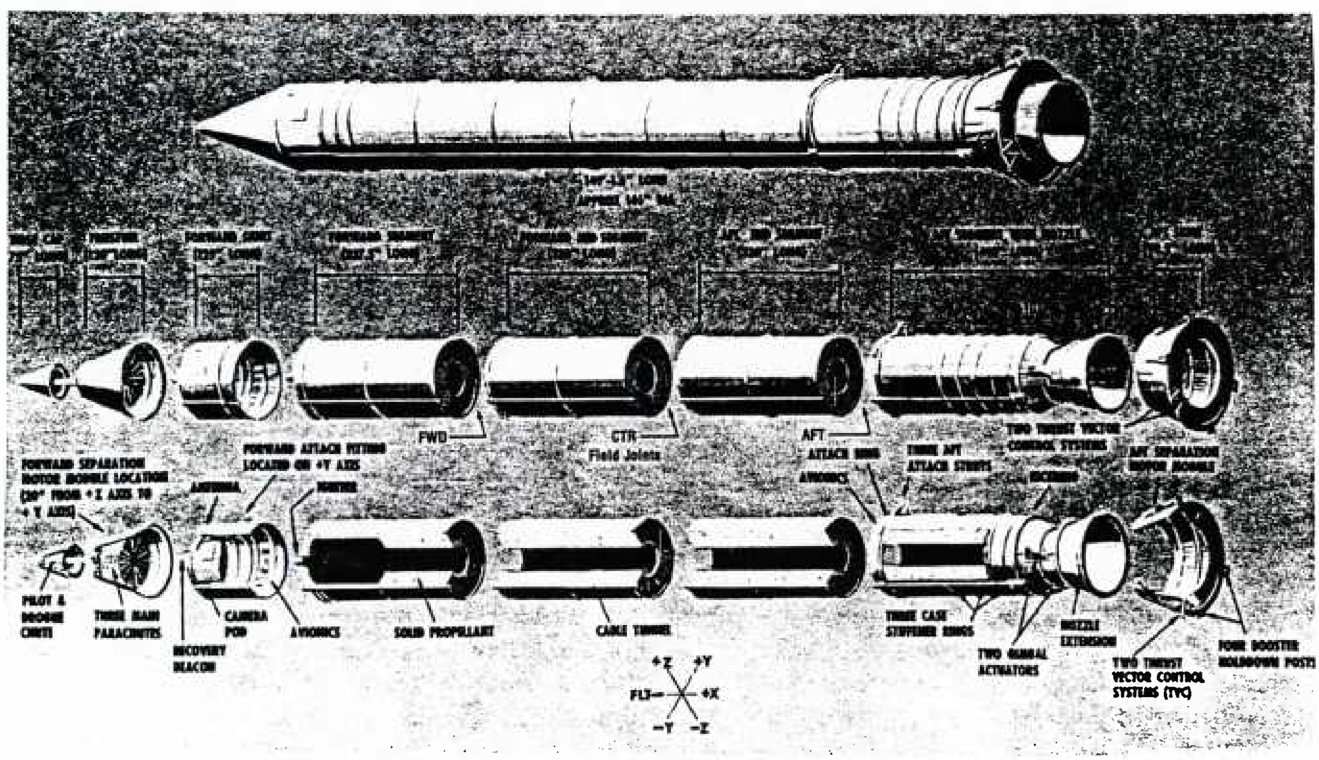
CURRENT PLAN:

- TWO STATIC FIRINGS AT 109% RATED POWER LEVEL
- CONVERT FACILITY TO SINGLE ENGINE TEST STAND

[Ref. 2/6-62]

SOLID ROCKET BOOSTER (SRB) PROJECT

[Ref. 2/6-63 1 of 2]



[Ref. 2/6-63 2 of 2]

SOLID ROCKET BOOSTER (SRB)

- **MARSHALL RESPONSIBILITY**

PRODUCTION OF THE SOLID ROCKET BOOSTER. THE PRIMARY ELEMENTS OF THE BOOSTER ARE THE 2.9 MILLION POUND MAXIMUM THRUST MOTORS: FORWARD AND AFT STRUCTURES: SEPARATION AND RECOVERY AVIONICS AND THRUST VECTOR CONTROL SUBSYSTEMS.

- **MAJOR CONTRACTORS**

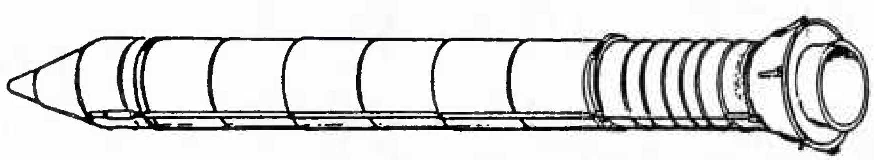
SOLID ROCKET MOTOR	– MORTON THIOKOL CORPORATION
BOOSTER ASSEMBLIES	– UNITED SPACE BOOSTERS PRODUCTION COMPANY (USBPC)
– STRUCTURES	– McDONNELL DOUGLAS ASTRONAUTICS CO.
– SEPARATION MOTORS	– UNITED TECHNOLOGY CORPORATION
– THRUST VECTOR CONTROL	– MOOG INCORPORATED & SUNDSTRAND
– INTEGRATED ELECTRONICS ASSEMBLY	– BENDIX CORPORATION
– RECOVERY SUBSYSTEM	– MARTIN MARIETTA – DENVER

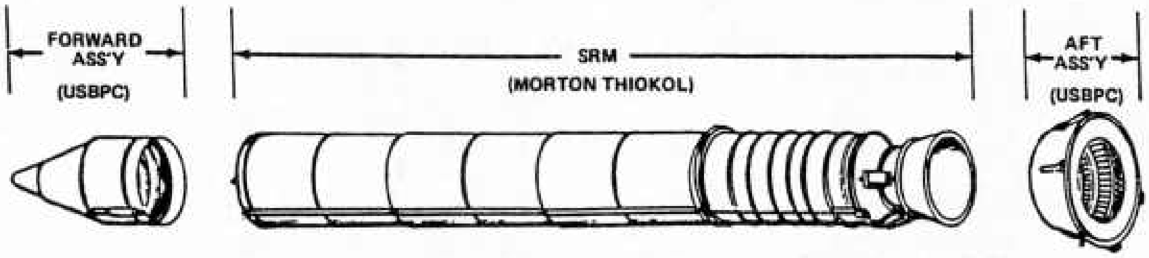
- **TEST FACILITIES**

MORTON THIOKOL	– LARGE MOTOR TESTING
MARSHALL SPACE FLIGHT CENTER	– SRB STRUCTURAL TESTING – TPS DEVELOPMENT & TESTING

[Ref. 2/6-64]

SOLID ROCKET BOOSTER
- SRB -





[Ref. 2/6-65]

SOLID ROCKET MOTOR

CHARACTERISTICS

● LENGTH	126.12 FT
● DIAMETER	12.17 FT
● PROPELLANT WEIGHT	1,110,000 LB
● TOTAL WEIGHT	1,256,000 LB
● AVERAGE THRUST	2,402,000 LB
● ACTION TIME	123.4 SEC

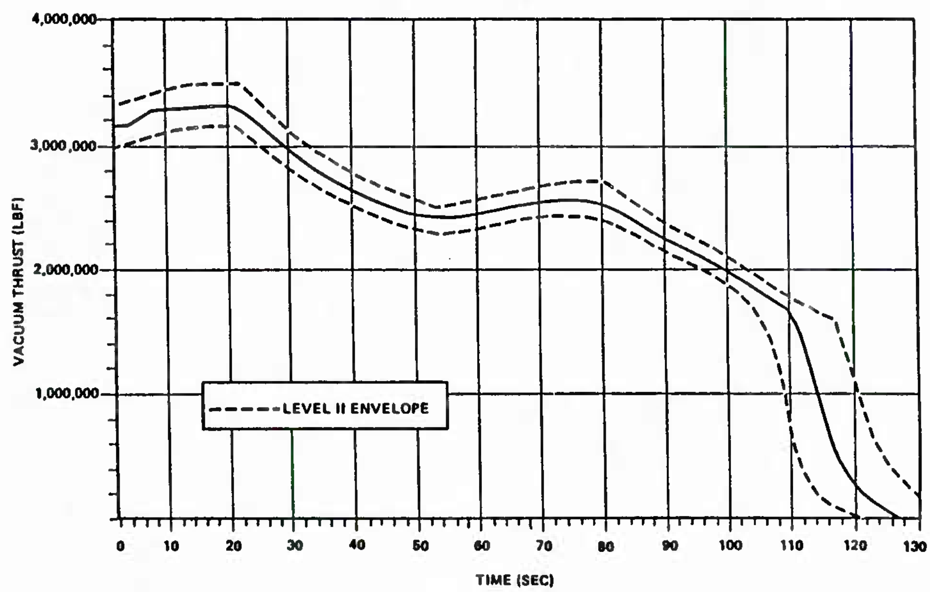
[Ref. 2/6-66]

DESIGN REQUIREMENTS AND QUALIFICATION

REQUIREMENT		QUALIFICATION
PERFORMANCE		
THRUST – TIME	FIGURE 20	DEVELOPMENT (5) AND QUALIFICATION (4) MOTOR TESTS
THRUST VECTOR CAPABILITY	$\pm 8^{o}$	
STRUCTURAL INTEGRITY		
HARDWARE	1.4 X LIMIT LOAD	ULTIMATE LOAD TEST & ANALYSIS
PROPELLANT	2.0 X LIMIT LOAD	SUBSCALE TESTS AND ANALYSIS
INSULATION INTEGRITY		
CASE	1.5 X PREDICTED EROSION	DEVELOPMENT AND QUALIFICATION MOTOR TESTS
NOZZLE	2.0 X PREDICTED EROSION	
DESIGN ENVIRONMENTS		ANALYSIS AND TESTS
PROPELLANT MEAN BULK TEMPERATURE RANGE OF $+40^{o}$F TO 90^{o}F		

[Ref. 2/6-67]

TYPICAL THRUST PROFILE



[Ref. 2/6-68]

SRM
PROJECT PERFORMANCE

O OPERATIONAL RELIABILITY CONSIDERATIONS

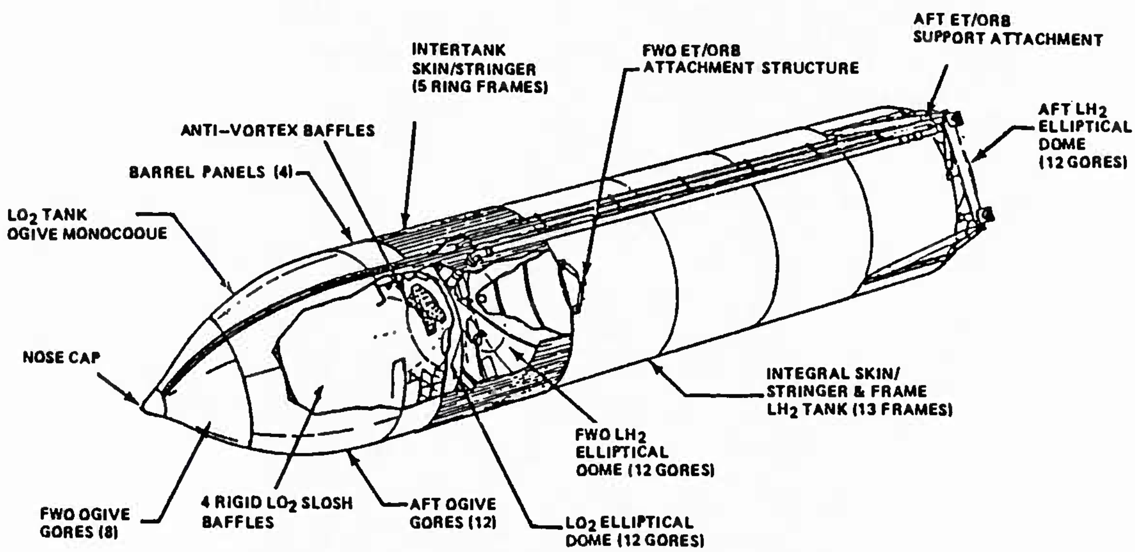
- o THE SRB HAS BEEN VERIFIED BY ANALYSIS AND GROUND TEST
  - MANNED SPACE FLIGHT DESIGN SAFETY FACTORS (RELATIVE TO OTHER SOLID ROCKET MOTORS) ARE APPLIED
    - o 1.4 X LIMIT LOAD ON STRUCTURE VS. 1.25 OR 1.15 ON MILITARY WEAPONS SYSTEMS
    - o 1.5 X PREDICTED REQUIREMENT (CASE) AND 2.0 X PREDICTED REQUIREMENT (NOZZLE) ON INSULATION THICKNESS VS. 1.25 ON MILITARY WEAPONS SYSTEM
    - o PROOF TEST TO 112% MAXIMUM EXPECTED FLIGHT PRESSURE
  - POST CASTING X-RAY VERIFIES PROPELLANT QUALITY
    - o 100% X-RAY OF PROPELLANT IN FIRST 68 SEGMENTS VERIFIED THAT PROPELLANT CASTING PROCESS PROVIDES PROPER PROPELLANT STRENGTH
    - o PROCESS CONTROL WITH RANDOM MONTHLY 100% X-RAY ON ONE SEGMENT
    - o X-RAY REQUIRED FOR ALL SEGMENTS WITH
      - o PROCESS ANOMALIES
      - o PROCESS CHANGES
      - o DESIGN CHANGES
  - 100% X-RAY OF NOZZLE ABLATIVE PARTS VERIFIES NOZZLE LINER INTEGRITY

[Ref. 2/6-69]

EXTERNAL TANK (ET) PROJECT

[Ref. 2/6-70]

SPACE SHUTTLE
LIGHTWEIGHT EXTERNAL TANK



LENGTH	153.8 FT	(46.6 METERS)
DIAMETER	27.6 FT	(8.4 METERS)
WEIGHT	66,000 LBS	(30,360 KILOGRAMS)
PROPELLANT	1.6M LBS	(736,000 KILOGRAMS)

[Ref. 2/6-71]

SA–1306

EXTERNAL TANK

MAJOR DESIGN REQUIREMENTS	QUALIFICATION
STRUCTURAL	
3 SIGMA LOADS AS DEFINED IN LOADS DATA BOOK	TEST TO 1.25/1.4 TIMES MAXIMUM SPECIFIED LOADS AT LH_2 TEMPERATURE FOR LH_2 TANK AND ROOM TEMPERATURE FOR LO_2 TANK AND INTERTANK
PROPULSION	
MAINTAIN AND DELIVER PROPELLANTS TO MEET ORBITER/SSME INTERFACE REQUIREMENTS	QUALIFY BY TEST TO REQUIREMENTS MAIN PROPULSION TESTS
THERMAL PROTECTION SYSTEM	
MAINTAIN PROPELLANT QUALITY THERMALLY PROTECT STRUCTURE FROM ASCENT AND PLUME INDUCED HEATING LIMIT ICE FORMATION TO AVOID DAMAGING ORBITER	QUALIFY BY TEST (WIND TUNNEL, COMBINED ENVIRONMENT, PLASMA ARC, ETC.) TO SPECIFIED ENVIRONMENT

FIGURE 25

[Ref. 2/6-72]

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TESTIMONY OF ROBERT SIECK, DIRECTOR OF SHUTTLE OPERATIONS, KENNEDY SPACE CENTER

MR. SIECK: Mr. Chairman, members of the Commission:

I'm Bob Sieck, Director of Shuttle Operations at the Kennedy Space Center, responsible for the conduct of the shuttle processing at Kennedy. Today I'm going to give a very general overview of that and talk about the facilities and the operations we perform within them.

(Viewgraph.) [Ref. 2/6-73]

This is an aerial map of KSC, of course, central east coast of Florida.

The next chart.

(Viewgraph.) [Ref. 2/6-74]

The two major areas, industrial area, which is primarily administrative, and some of our off-line shops and labs.

The next chart.

(Viewgraph.) [Ref. 2/6-75]

This is where I am going to focus the briefing today, which is what we refer to as Launch Complex 39.

The next chart.

(Viewgraph.) [Ref. 2/6-76]

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This is an overview of what we characterize as our flow. For the Challenger mission it was generic in terms that we used all of our major facilities. Orbiter processing facility is the primary one. We do the integration of the shuttle elements in a vertical assembly building and proceed to the launch pad.

And I apologize, this is not in your briefing. We're going to have to get you this, along with better reproductions of all of the slides and the photographs that I have here, because the quality is not good in your handout. But we will get you a good photocopy of them.

I should say a few words about the processing team at Kennedy. It is a civil service-contractor team. We have approximately 6500 contractor personnel. Lockheed is our principal shuttle processing contractor. We have subcontracts with Morton Thiokol for the solid rocket booster processing. We have Grumman, which handles our launch processing system and computers, and we have Rocketdyne for the main engines.

We also have on-site at Kennedy during all of our processing representatives from the design agencies and the design elements. That has been briefed before. Principally again, for the solid rocket boosters we have United Space Boosters and Morton Thiokol. For the main

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engines, we have Rocketdyne. For the orbiter, we have Rockwell International. And for the external tank, Martin Marietta.

And they are part of our process as we go through the flow of the vehicle at Kennedy.

The next chart.

(Viewgraph.) [Ref. 2/6-77]

A little bit more detail. On the left, the orbiter processing facility. We have two bays. We can accommodate two orbiters simultaneously. In the center, the vehicle assembly building, which is where the shuttle elements come together: the orbiter from the orbiter processing facility; the solid rocket boosters, after their refurbishment cycle, come through our rotation and processing and surge facility into the vehicle assembly building; and the external tank arrives via barge from Louisiana.

They are assembled on a mobile launch platform in one of our two integration cells, and proceed to one of our two launch pads.

The next chart.

(Viewgraph.) [ref. 2/6-78]

This is where it starts. This is our landing facility at Kennedy.

The next chart.

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(Viewgraph.) [Ref. 2/6-79]

It is basically a three-mile runway with standard aircraft navigation aids, and we have a microwave scanning beam system for the autoland capability, which the orbiter has not demonstrated yet as part of its operation.

The next chart.

(Viewgraph.) [Ref. 2/6-80]

We have had five landings of orbiters from orbit at Kennedy.

The next chart.

(Viewgraph.) [Ref. 2/6-81]

This is the way that Challenger arrived after its last mission, which was in November of 1985. It came in on our carrier aircraft to our mate-demate device.

(Viewgraph.) [Ref. 2/6-82]

And of course, put it on the runway, extend the landing gear, and we tow it to the orbiter processing facility. And that is the next area which I will address.

The next chart.

(Viewgraph.) [Ref. 2/6-83]

This is a view of the orbiter processing facility, basically two hangars with extensive checkout and access equipment. It gives us the capability to essentially totally refurbish the orbiter, with the

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exception of very major structural mods. This is where most of our work force is concentrated. Most of our activity in a turn-around is conducted on the orbiter because of the complexity of that hardware, in one of these two high bays.

Next chart.

(Viewgraph.) [Ref. 2/6-84]

A few words about the capabilities there. Essentially, we can access every compartment on the vehicle and we can test it remotely using our launch processing system.

The principal activity here, of course, is with Lockheed, primarily the refurbishment after a flight.

Next chart.

(Viewgraph.) [Ref. 2/6-85]

The operations there on the standard turn-around, approximately three to four weeks. We do all of the things that you see here, essentially in parallel to minimize the time really that we spend in this facility.

Characteristically, after a mission we safe the vehicle, and this was the case with Challenger. There were no major anomalies there. We did our de-servicing of the hazardous consumables, went into our

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main engine inspections, which is one of the critical items that we perform, and reconfiguration of the payload bay for the next mission.

All of that processing was normal. This particular turn-around flow of Challenger was a little bit longer than normal because we took the opportunity to put in some of the modifications required on Challenger to fly the Centaur interplanetary missions, which would have been in the spring of this year.

The tile operations are something that we contend with each turn-around, and we start those as soon as we roll in and they proceed through until OPF rollout.

The next chart.

(Viewgraph.) [Ref. 2/6-86]

To give you somewhat of an idea of the access in there, we totally surround the orbiter with access platforms and support equipment.

The next chart.

(Viewgraph.) [Ref. 2/6-87]

We do perform payload integration for horizontally installed payloads in the orbiter processing facility. In this case we did not do that, but we had to remove the space lab from the previous mission.

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(Viewgraph.) [Ref. 2/6-88]

There is a picture of the space lab.

The next chart.

(Viewgraph.) [Ref. 2/6-89]

Some of the tile work.

The next chart.

(Viewgraph.) [Ref. 2/6-90]

I'm going to talk a little bit about the processing of the solid rocket motors and the solid rocket booster segments.

Next chart.

(Viewgraph.) [Ref. 2/6-91]

These are the facilities that we perform that in, the triangle of buildings in the bottom of the picture. We have two surge facilities and we have what we call a rotation and processing facility.

Next chart.

(Viewgraph.) [Ref. 2/6-92]

The segments are brought in on rail car. They are horizontal. We remove them with a crane, bring them into the processing facility, perform an inspection on all of the interfaces, and then we move the segments in and stack them in sequence in one of our two surge facilities.

Next chart.

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(Viewgraph.) [Ref. 2/6-93]

This is a photograph of some of the operations in the processing facility. Again, no checkout in here, storage and inspection only.

Next chart.

(Viewgraph.) [Ref. 2/6-94]

Now I'm going to talk a little bit about our mobile launch platform. The particular one used in the Challenger mission, I would have to get you the exact number, but we have used it for approximately half of our previous launches.

The next chart.

(Viewgraph.) [Ref. 2/6-95]

Basically, it provides the launch mount for the shuttle vehicle. As was explained before, the solid rocket boosters are bolted to this launch mount and it is moved around.

The next chart.

(Viewgraph.) [Ref. 2/6-96]

That is the mobile launch platform.

The next chart.

(Viewgraph.) [Ref. 2/6-97]

This is a crawler transporter, which we use to transport the mobile launch platform to the vertical assembly building and back and forth to the launch pad.

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The next chart.

(Viewgraph.) [Ref. 2/6-98]

Now, I'm going to spend some time talking about the integration of the shuttle elements, and that occurs—the next chart—

(Viewgraph.) [Ref. 2/6-99]

—in the vehicle assembly building. This is where the shuttle hardware essentially comes together. We have two what we call integration cells, high bays, that we can stack the vehicle in. We have two other bays which we use for storage of the external tank, and in the low bay areas we have some shops and labs.

The next chart.

(Viewgraph.) [Ref. 2/6-100]

Next chart.

(Viewgraph.) [Ref. 2/6-100]

Okay, this is the mobile launch platform being brought into one of the integration cells with the crawler transporter.

The next chart.

(Viewgraph.) [Ref. 2/6-101]

We then begin the stacking of the solid rocket booster assemblies on the mobile launch platform. They are brought over with the transporter from one of the two surge facilities.

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Next chart.

(Viewgraph.) [Ref. 2/6-102]

To give you an idea of that process, we use one of our large cranes to raise the solid rocket motor assembly.

The next chart.

(Viewgraph.) [Ref. 2/6-103]

And we lower it, and we put the pins essentially in, which is approximately 150 of them, around the circumference of the solid rocket booster. A final inspection of the sealing surfaces is done at this time, again with the factory reps on board, before we do the final pinning of the segment interfaces.

Next chart.

(Viewgraph.) [Ref. 2/6-104]

MR. FEYNMAN: Excuse me. How well do they fit together? Of course, you've constructed them round and everything was okay, but they fell into the sea and so on, and then you bring them together. Did they still fit perfectly?

MR. SIECK: No, sir, they do not always fit the first time. After they are repacked with the solid propellant in Utah and they're transported to KSC, when we get ready to do this process we do an initial fit check.

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We have the capability with the sling to hold the segment with either two points or four points. We have found many times that, when we try to mate these, due to out-of-round conditions, we have to demate, change the sling positioning, and let the segment sit for some period of time, maybe even up to three hours, and then come back down and do the mate again.

So they do not always mate the first time. And again, when we give you a detailed presentation on the actual history of these segments, we shall go through that with you.

CHAIRMAN ROGERS: Why did you change the launch pad on this occasion?

MR. SIECK: This particular one, we have been working on launch pad B, which is our new pad, last used in Apollo for over a year. And it is part of our process to increase the flight rate to get two launch pads on line. This was the first opportunity to use the new launch pad. It was completed in December of 1985.

CHAIRMAN ROGERS: Was it identical with the previous one?

MR. SIECK: It is identical from the standpoint of looks and function. At the time we did the launch, there were still some differences in the buildup of the Centaur modifications to make the two

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pads identical, and the rain protection systems.

Getting into more detail, functional components on each pad, due to vendor changeout you would find some differences there, but functionally the same.

Next chart.

(Viewgraph.) [Ref. 2/6-104]

Okay. The external tank comes in via barge from Louisiana, usually many weeks, sometimes months, before we stack it in the vehicle assembly building.

Next chart.

(Viewgraph.) [Ref. 2/6-105]

Again, we use the cranes to put it in the storage cell and remove it.

Next chart.

(Viewgraph.) [Ref. 2/6-106]

And attach it to the two solid rocket boosters.

Next chart.
(Viewgraph.) [Ref. 2/6-107]
We bring the orbiter in from the processing facility.
Next.
(Viewgraph.) [Ref. 2/6-108]
Attach the slings, retract the landing gear.

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Next.
(Viewgraph.) [Ref. 2/6-109]
Lift it. Next.
(Viewgraph.) [Ref. 2/6-110]
And lower it and attach it to the external tank attach points, the three that were described before.
Next chart.
(Viewgraph.) [Ref. 2/6-111]
Once we have completed a verification of all of those new interfaces, which is a fairly small amount of time—and for the Challenger flow it was the nominal four to five days—we roll to the launch complex.
Next chart.
(Viewgraph.) [Ref. 2/6-112]
And that is what I will describe next.
Next chart.
(Viewgraph.) [Ref. 2/6-113]
Here is our two launch pads. The one to the right is launch complex B. Again, the last time we used that was in the Apollo program. But again, to repeat, the mobile launch platform, which is the launch mount for the vehicle, has been used a number of times, and we had had it to the launch pad previous for some fit and

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interface verifications before we did the stacking for this mission.
Next chart.
(Viewgraph.) [Ref. 2/6-114]

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A few words about the facilities there. The water systems, we have three water systems, sound suppression water, which is a quench to deaden the shock wave at liftoff of the solid rocket boosters. We have what we call a potable water system, which is primary safety showers and eye washes and faucets, and we have a firefighting system which we refer to as the FIREX system.

The night of the launch, our procedure in order to maintain those three capabilities was to establish a bleed through all of those systems, much as you would a water faucet when freezing conditions were eminent, and routed most of that water over to our drain system. Our drain system is not what we call a closed loop system, though.

It dumps out on some of the platforms on the west side of the service structure, and we did notice a lot of ice out there, and that was one of the reasons for the additional ice inspection we did late in the launch count on the launch day. Next chart.
(Viewgraph). [Ref. 2/6-115]

MR. SIECK: As we go up to the launchpad with our crawler transporter mobile launch platform, we have the capability to keep the vehicle level. Next chart.

(Viewgraph.) [Ref. 2/6-116]

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MR. SIECK: And set it down on the pedestals at the launch pad and remove the crawler transporter. Next chart.

(Viewgraph.) [Ref. 2/6-117]

DR. RIDE: Is there any concern for the orbiter being out on Pad B without the rain protection that it would have had on Pad A?

MR. SIECK: We did have that concern. Of course, we waterproofed the orbiter thermal protection system before the rollout from the orbiter processing facility, and our criteria since all of the rain protection modifications were not in place, that after each rain we would go out and reinspect the water protection system, which is sprayed on the tile of the orbiter, and we did that three or four times. We will have to get you the exact data on that between the time we rolled out to the launchpad on the weekend of December 21st until the launch on the 28th of January.

CHAIRMAN ROGERS: Following up on Dr. Ride's question, I gather the rain protection system on one launchpad is different from the other?

MR. SIECK: Yes, it is. The plan is to get them both the same. Launch Pad B, the one we launched from, the modifications were not complete. One of the operations we perform at the launch pad is the

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integration of the payload into the orbiter, and this was done for the Challenger mission the way it is normally done. You use a payload cannister. The interim upper stage IUS and the TDRS satellite were brought to the launchpad in a cannister such as this, and that was done before the orbiter and Shuttle arrived there. Next.

(Viewgraph.) [Ref. 2/6-118]

MR. SIECK: Next they are removed from the cannister and installed in the orbiter using our ground handling mechanism. That was the normal procedure. Next chart.

(Viewgraph.) [Ref. 2/6-119]

MR. SIECK: I would say a little bit about our launch processing system, which supports all of the Shuttle flow from the orbiter processing facility to the launchpad. Next chart.

(Viewgraph.) [Ref. 2/6-120]

MR. SIECK: The launch processing system again is primarily maintained by the Grumman Corporation. The next chart.

(Viewgraph.) [Ref. 2/6-121]

MR. SIECK: The heart of the system are these consoles. Each engineer, when they perform their systems checkouts per the design center requirements,

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use their procedures and their software. Whether the orbiter is in the processing facility, the vehicle assembly building, or the launchpad, we have four of these control rooms. Two of them are configured for launch process. The control room we launched from on the Challenger mission had been used many times before. Next chart.

(Viewgraph.) [Ref. 2/6-122]

MR. SIECK: That is the photograph of the control center. Of course, on launch day all of those positions or consoles are manned. There are approximately 150 people in the control room at launch time.

(Viewgraph.) [Ref. 2/6-123]

MR. SIECK: A little bit about the post-launch activity for the SRB retrieval. Next chart.

(Viewgraph.) [Ref. 2/6-124]

MR. SIECK: We have a disassembly area located over on the Air Force installation at Cape Kennedy. Next chart.

(Viewgraph.) [Ref. 2/6-125]

MR. SIECK: We retrieve the solid rocket boosters from the ocean. We have three retrieval ships. Currently here one of them is designated to go to Vandenberg, but there are currently three on site at

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Kennedy. Morton-Thiokol people are principally in charge of this operation of the retrieval. Next chart.

(Viewgraph.) [Ref. 2/6-126]

MR. SIECK: We right the booster assembly, and tow it back to Port Canaveral. Next chart.

(Viewgraph.) [Ref. 2/6-127]

MR. SIECK: And take it into this facility. It is lifted out of the water. It goes through a rinsedown process, and then a disassembly, a cleaning and a refurbishment process. The Morton-Thiokol people essentially finish their part of the disassembly and retrieval process at the time they turn it over to United Space Boosters and the Marshall contractor who performs the refurbishment of the segments which go back to Utah or the aft assemblies and the forward assemblies which have the electronics in it.

CHAIRMAN ROGERS: Originally the retrieval program was based upon economies, I assume. We felt it, or the country thought it was cheaper and less expensive to do it that way? Is that still the case? Is it cost effective?

MR. SIECK: I probably ought to defer to Marshall to get you the actual data on that. For us, for our operation at Kennedy, it is relatively inexpensive. How the money stacks up on the reuse and

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retest of the hardware versus new. That was the original plan, obviously. I don't know how the dollars would add up. We will have to get you that.

MR. SMITH: I was talking to the captain of retrieval operations the other day. In his calculation, they have retrieved slightly in excess of $1 billion worth of hardware.

CHAIRMAN ROGERS: A year?

MR. SMITH: Total.

CHAIRMAN ROGERS: The whole program? But I mean, I would assume that since that program started there would be improvements made so that the cost of buying new ones would have decreased. In other words, do we know whether it is still desirable from a standpoint of dollars?

MR. SMITH: The figure that I was quoting would be the cost of brand new steel cases and all, and not the total cost of the motors. It is the money saved by reuse, is what the figure should have been. We will have to verify that, but that is the figure he had.

CHAIRMAN ROGERS: I think the Commission would like to have that if we could get some accurate figures on that, on whether, if you, instead of continuing the retrieval program, you went to a program of buying original boosters, how much would it cost, and would it

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be difficult to put into effect?

MR. MOORE: Mr. Chairman, we have got economic analyses that we have done and so forth, and we will be happy to provide this Commission what we think the economic tradeoffs are relative to retrieval or not retrieving this hardware.

MR. ACHESON: How long an interval in days or hours or whatever is it between the final assembly of the system and the rollout to the launchpad?

MR. SIECK: Well, in the case of Challenger we will have to get you that data specifically.

MR. ACHESON: In the case of 51-L.

MR. SIECK: For 51-L, we completed the stacking of the solid rocket motor segments in the vehicle assembly building approximately the first week in December, and then mated the external tank, and again we rolled out to the launchpad the weekend of the 21st of December.

Now, going back further in the genealogy of those casings, whenever they were delivered from Utah, we will have to retrieve that data for you.

MR. SUTTER: After the thing splashes into that salt water, I assume they are still hot, and they get towed around by a ship, and you wash them down, but do you do any detailed structural analysis to make sure

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that the load, the design loads haven't changed?

MR. SIECK: Well, I ought to defer to Marshall on that. But there is a complete inspection done at this Air Force facility before these segments are shipped out of Florida.

MR. SUTTER: Is that just a visual inspection?

MR. SIECK: It is primarily a visual inspection and a cleanup of the insulation. The process back at Morton Thiokol in Utah, we will have to get you a briefing on that, what they do with the segment casings and the repacking of the grain.

DR. RIDE: Is all of the refurbishment done in Florida and then the refurbished casing sent back to Utah for packing, or is some of the refurbishment done in Utah?

MR. SIECK: It is split up some, Sally. There is some done here by the Marshall contractors over at Cape Kennedy, and the remainder is done back in Utah, and the same applies for those assemblies which have the electronics in it, the aft and forward. Those are primarily done here in Florida, but again by the Marshall Space Flight Center.

GENERAL KUTYNA: Bob, what you have been describing for the last 20 or so minutes is really a fantastic example of teamwork and hands-on experience in

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processing the shuttle after a flight and getting it ready for launch. In the early days of the program, that was done by the people who built and designed the Shuttle, those three element contractors. We then decided to compete to get the price down.

Could you describe how well did we do in retaining that old hands-on experience, those old pros that processed this before we changed contractors?

MR. SIECK: Well, the specific percentage of retention of the work force, I think we will have to get you the exact number there. It was approximately 85 percent. But there were some disciplines that had a higher percentage than others. When the contract change was made and the Shuttle processing contractor, Lockheed, took that over, they got predominantly all of the hands-on work force; a lot of the management and engineering percentages were less.

But the point to be made, a number of people, particularly the key ones, remained at Florida as part of the launch support services contract under the design centers, and they are still

there as part of the processing, even though they are not in line in the management function on the minute-to-minute, hour-by-hour work.

CHAIRMAN ROGERS: I notice there appeared to

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be a leak that Lockheed had some inquiry that you were conducting. When will that inquiry be finished, and will it be available to the public?

MR. SIECK: I am sorry? The inquiry on what, sir?

CHAIRMAN ROGERS: There was previous inquiry about Lockheed's performance, and it was in the paper, I guess, two days ago.

MR. SIECK: Well, maybe I ought to explain the process. We evaluate Lockheed.

MR. SMITH: Bob, let me address what Chairman Rogers is speaking of.

MR. MOORE: Mr. Chairman, if I might, let me introduce Dick Smith, center director of the Kennedy Space Center.

(Witness sworn.)

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TESTIMONY OF RICHARD SMITH, DIRECTOR, KENNEDY SPACE CENTER

MR. SMITH: We did have a handling estimate back in November, if I remember correctly. I don't remember the exact date. We completed our internal investigation of that. I approved that in around mid-December, and I don't remember the specific date right now. We typically send that to headquarters for our review up there before we release it. That process is going on at this time. I have on a preliminary basis already implemented, started implementing all of the recommendations, and we will make modifications if there will be any additional judgments to that.

CHAIRMAN ROGERS: And the question is, will that report be made public?

MR. SMITH: That report is a public document after the final approval by the headquarters people. Yes, sir.

CHAIRMAN ROGERS: Thank you.

MR. SIECK: I believe that completed my presentation. Let's see. Next chart.

(Viewgraph.) [Ref. 2/6-128]

MR. SIECK: Okay, just a few words about our off-line support facilities. A large logistics building which we just completed to maintain our spares. The

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Hypergolic maintenance facility is a special facility we use to handle the orbiter maneuvering system pods on the orbiting and the forward RCS, because of the nature of the Hypergolic fuels. We do not do maintenance on those systems in the orbiter processing facility. We remove them off line, and the parachutes from the solid rocket boosters, we retrieve those, clean them, repack them, and reuse them.

That completes my presentation.

DR. WALKER: Could you say something about the venting of the gases from the external tank during launch?

MR. SIECK: During our launch countdown process, when we load the external tank liquid hydrogen and oxygen, the hydrogen tank venting is contained through an arm with a disconnect that essentially is cut loose at liftoff, and all that hydrogen venting is contained, and it goes through a burnoff system which in the case of Launch Complex B we call a flare stack, and it is

contained in that system throughout the loading, and we have sensors around that umbilical and at the interfaces with the orbiter where we load it to detect any leakage.

The oxygen system. The oxygen, liquid oxygen tank is on the top. We have what we call a beanie cap

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that fits over the top of the external tank, and it has an inflatable bellows to contain all of that oxygen, and it also is vented through an arm to the outside at the same level as the top of the external tank.

DR. WALKER: Is the hydrogen vent valve closed at launch?

MR. SIECK: Yes, we close the hydrogen vent valve when we pressurize the tank at approximately two minutes before launch.

CHAIRMAN ROGERS: If there are no further questions, thank you very much. We appreciate it. I think we will have a ten-minute recess now, please.

(Whereupon, a brief recess was taken.)

CHAIRMAN ROGERS: May I have your attention, please?

We plan to complete the hearing today by 4:30, quarter to 5:00, and that will finish the work that had been planned, the testimony that had been planned that we were hearing from NASA.

Tomorrow we will meet at 9:00 a.m. in the Old Executive Office Building in a closed session. The Commission will continue its work, including making plans and setting up some procedures for future work, and we also may take some testimony from witnesses, possibly classified information.

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In order to explain to the media our general attitude about the future work of the Commission, let me say that we all would like to provide as much information as we can to the public, and obviously all of the information will eventually be provided to the public.

Secondly, we have to have some of our sessions in closed meetings. President Reagan has asked us to consider the evidence that we have and testimony in a calm and deliberate fashion, and in order to do that and to have a free exchange of ideas, we must meet in private session from time to time.

We have no plans to announce today as to future meetings, but as soon as we make those decisions, we will let you know.

Now we will go ahead with the rest of the testimony.

MR. MOORE: Mr. Chairman, continuing on with our planned agenda, we will talk about the design and development process for both hardware and software as well as the review process and safety process. We will try to abbreviate this process to try to give the Commission a flavor of it and to show that it is in general applicable to all the flights, but also applicable to 51-L, the Challenger mission.

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I would like to introduce Thomas L. Moser, director of engineering at the Johnson Space Center.

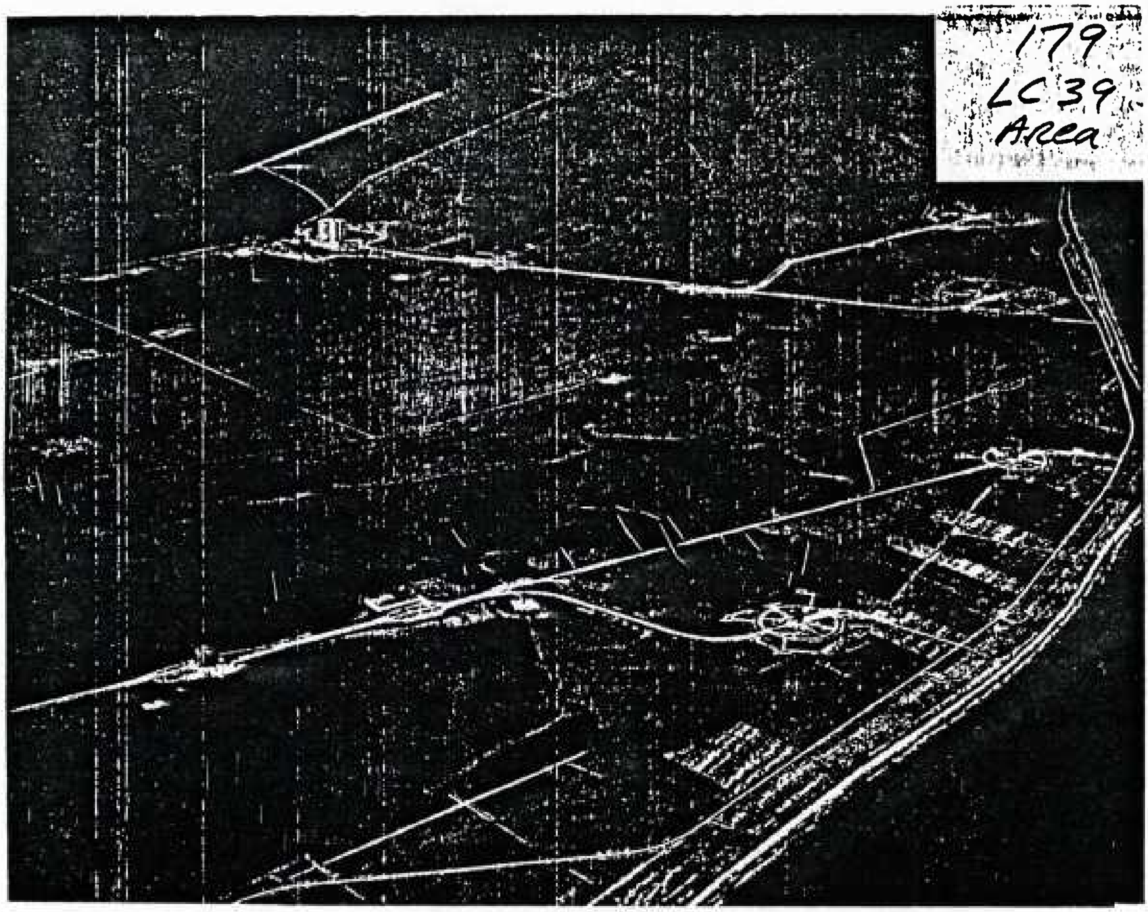
CHAIRMAN ROGERS: Mr. Moser, go right ahead.

(Witness sworn.)

[Ref. 2/6-73]

[NOT REPRODUCIBLE]

[Ref. 2/6-74]



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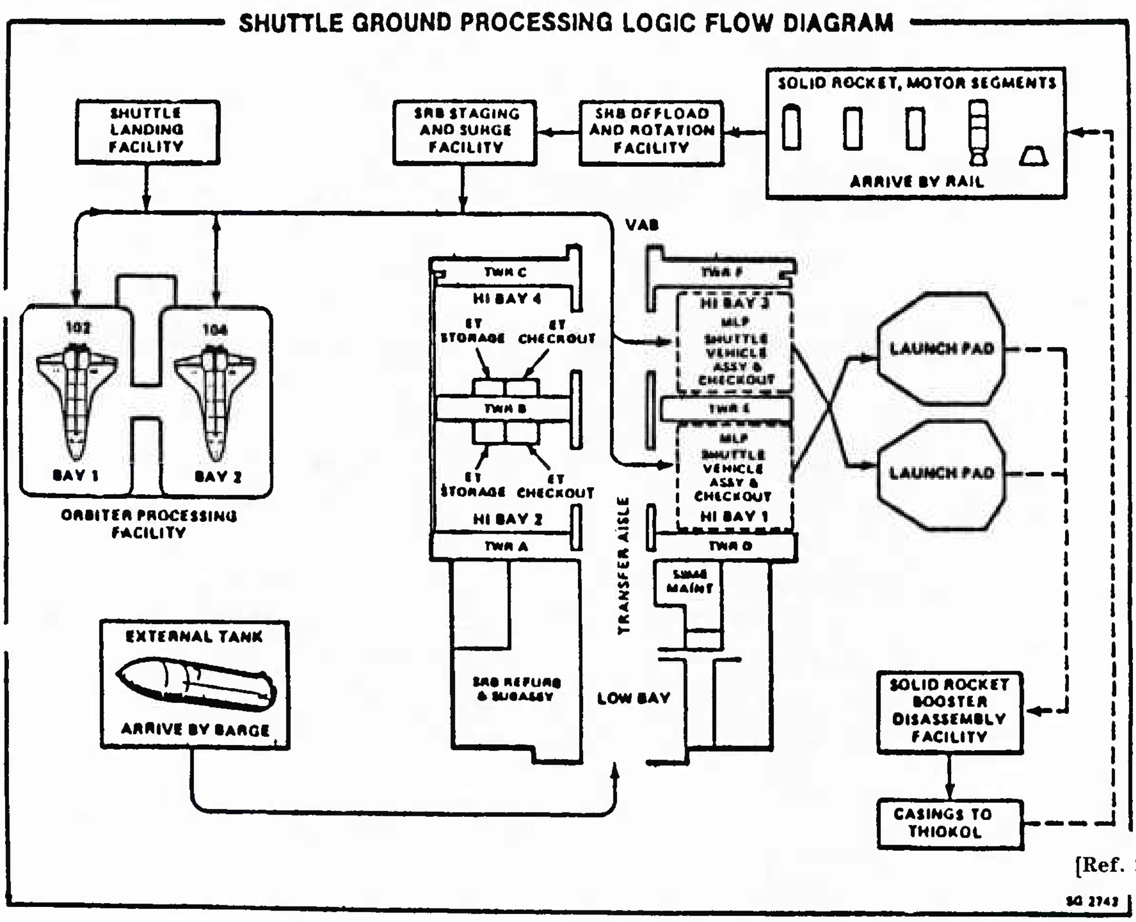

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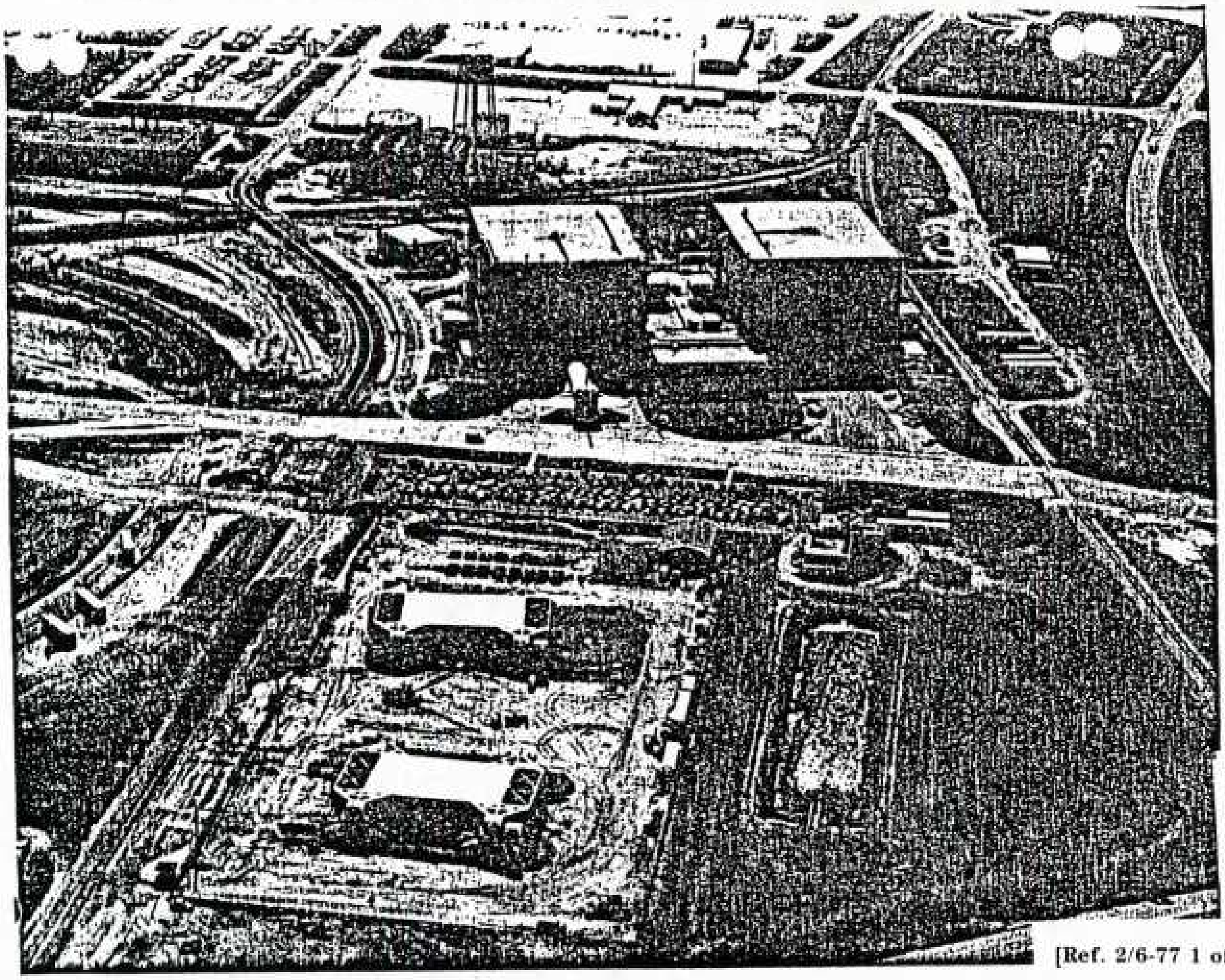


[Ref. 2/6-77 1 of 3]

ORBITER PROCESSING FACILITY (OPF)

- DESCRIPTION
  - TWO IDENTICAL HIGH BAYS CONNECTED BY A LOW BAY WITH AN OFFICE ANNEX
  - CONTENTS
    - ACCESS PLATFORMS WHICH SURROUND THE ORBITER
    - TWO ROLLING BRIDGES EACH WITH TWO TELESCOPING PLATFORMS FOR CARGO BAY OPS
    - EMERGENCY EXHAUST AND FIRE PROTECTION SYSTEMS
    - ZERO "G" COUNTERWEIGHT DEVICE FOR CARGO BAY DOOR OPERATIONS
    - LPS INTERFACE EQUIPMENT WITH CONTROL ROOMS
    - SHOPS, MATERIAL SERVICE CENTER
    - MECH AND ELECT SUPPORT EQUIPMENT

[Ref. 2/6-77 2 of 3]

ORBITER PROCESSING FACILITY (OPF)
(Continued)

OPERATIONS

- INITIAL ACCESS AND SAFING
- POST FLIGHT TROUBLESHOOTING
- MAIN PROPULSION SYSTEM REVERIFICATION
- CARGO BAY OPERATIONS
  - DOWN CARGO REMOVAL
  - MISSION KIT RECONFIGURATION
  - HORIZONTAL CARGO INSTALLATION
- ORBITER MODIFICATIONS
- POWER ON SYSTEM REVERIFICATION
- SCHEDULED MAINTENANCE ACTIVITIES
- TPS (TILE) OPERATIONS
- ORBITER CLOSEOUT

[Ref. 2/6-77 3 of 3]

STS LANDING FACILITY (SLF)

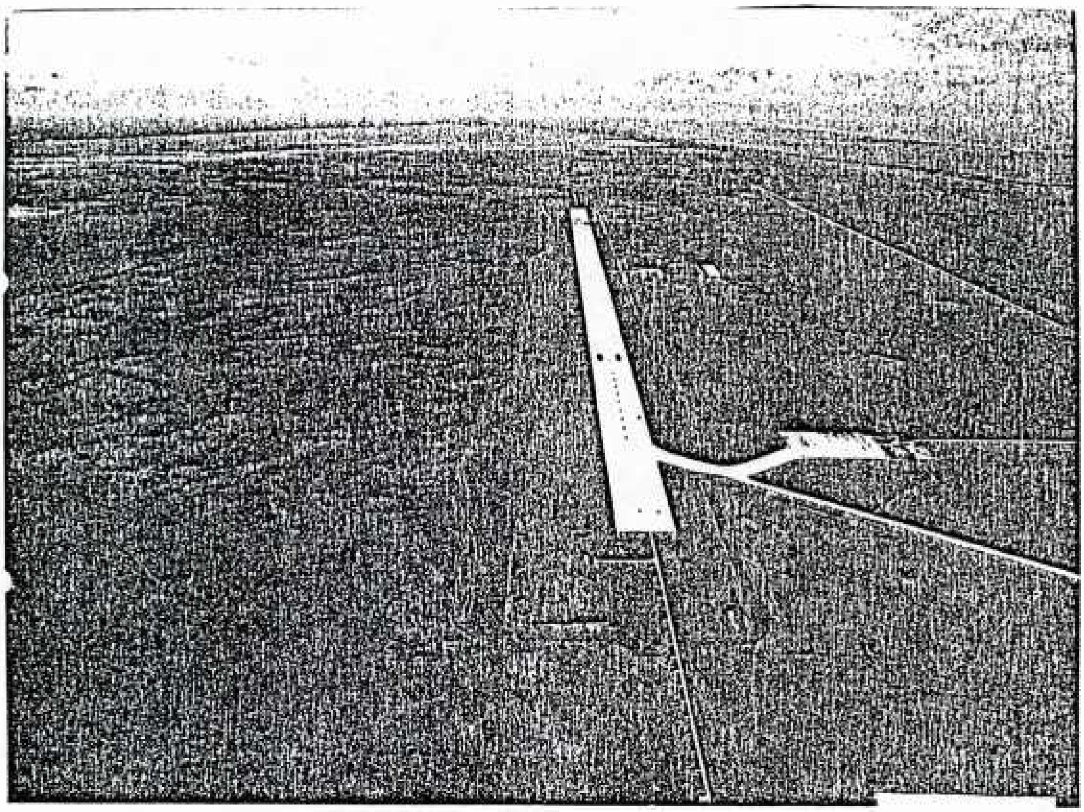
DESCRIPTION

- THE RUNWAY IS 15,000 FEET LONG, AND 300 FEET WIDE WITH A 1,000 FOOT PAVED OVERRUN AT EACH END WITH LANDING SYSTEM AIDS
- LANDING AIDS CONTROL BUILDING IS LOCATED NEAR PARKING APRON WHICH SUPPORTS THE LANDING CONTROL OPERATIONS
- THE MATE/DEMATE DEVICE LOCATED AT THE RAMP OF THE SLF

OPERATIONS

- ORBITING LANDING AFTER A MISSION AND LANDING OF THE ORBITER PIGGY-BACK ON A 747 CARRIER AIRCRAFT
- MICROWAVE SCANNING BEAM LANDING SYSTEM (MSBLS) AND TACTICAL AIR NAVIGATION (TACAN) ARE USED FOR ORBITER LANDINGS. ALSO, VISUAL AIDS PRECISION APPROACH PATH INDICATORS (PAPI) ARE INCLUDED.
- ORBITER CONVOY OPERATIONS FOR CONTINGENCIES AT TIME OF LAUNCH AND SAFING THE ORBITER, EGRESSING THE FLIGHT CREW AND PREP THE ORBITER FOR TOW TO THE OPF AFTER A MISSION
- THE MATE/DEMATE DEVICE IS USED TO EITHER REMOVE AN ORBITER FROM THE BACK OF THE 747 AIRCRAFT OR TO PLACE AN ORBITER ON A 747 AIRCRAFT.

[Ref. 2/6-78]



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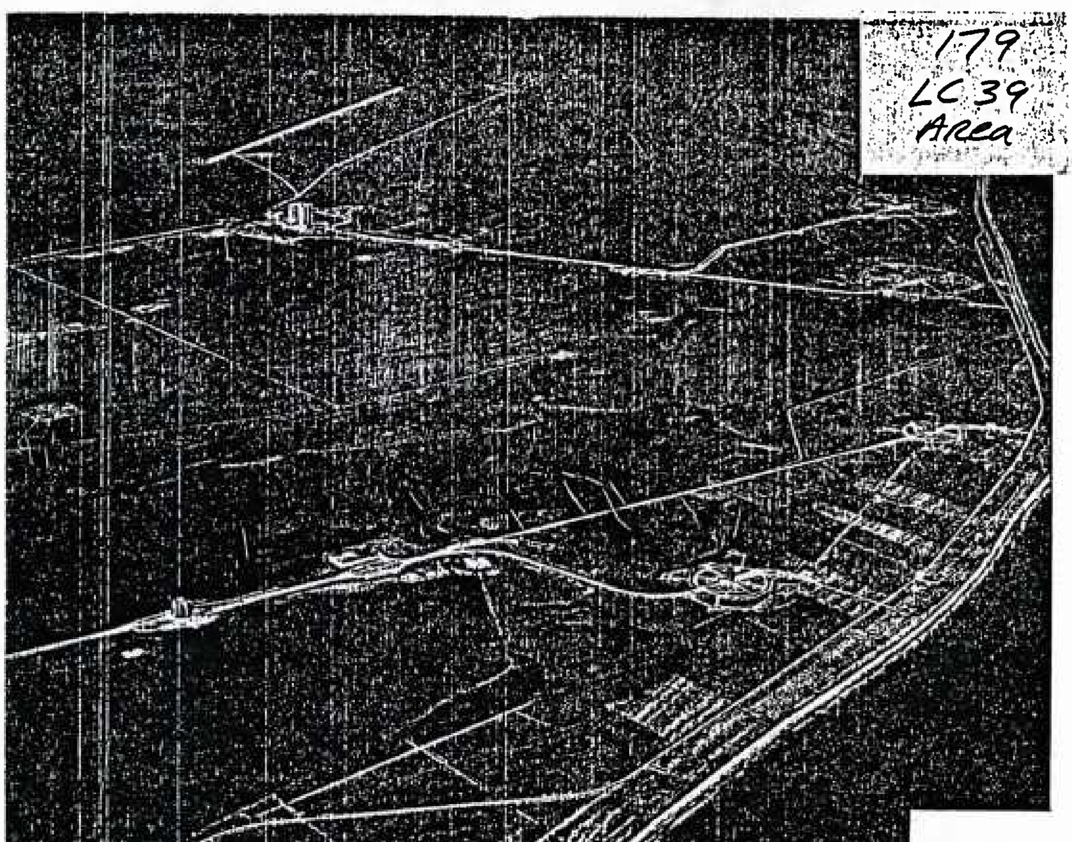

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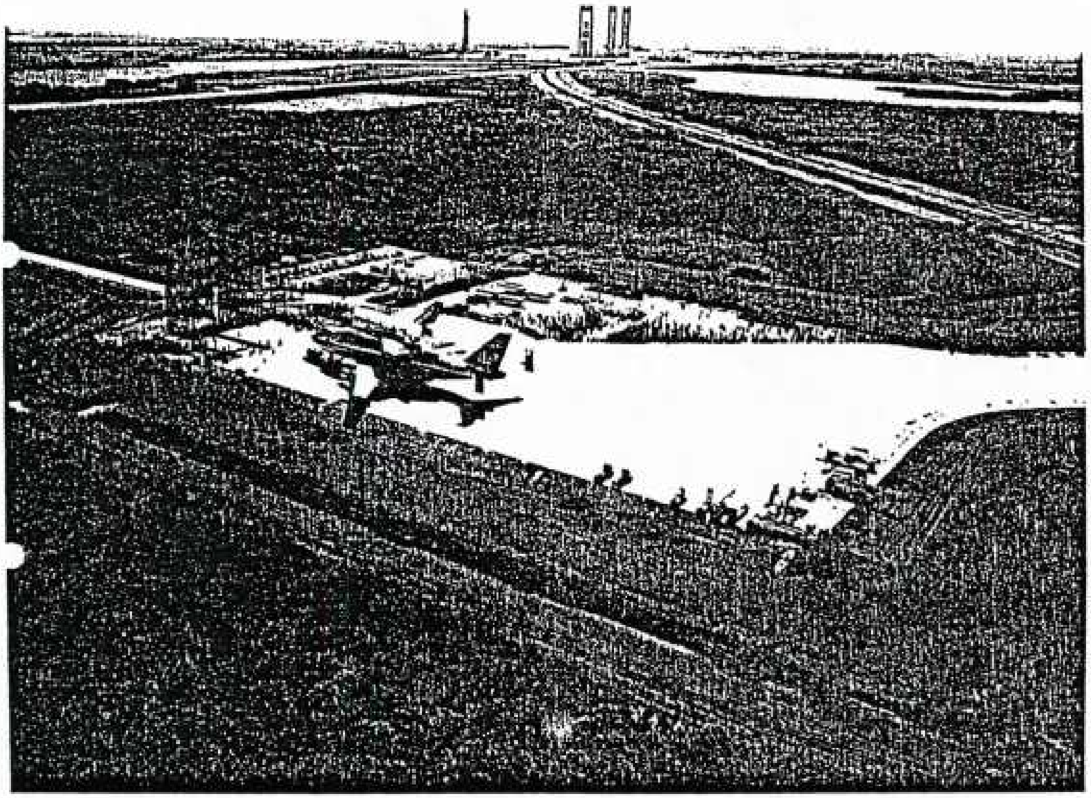

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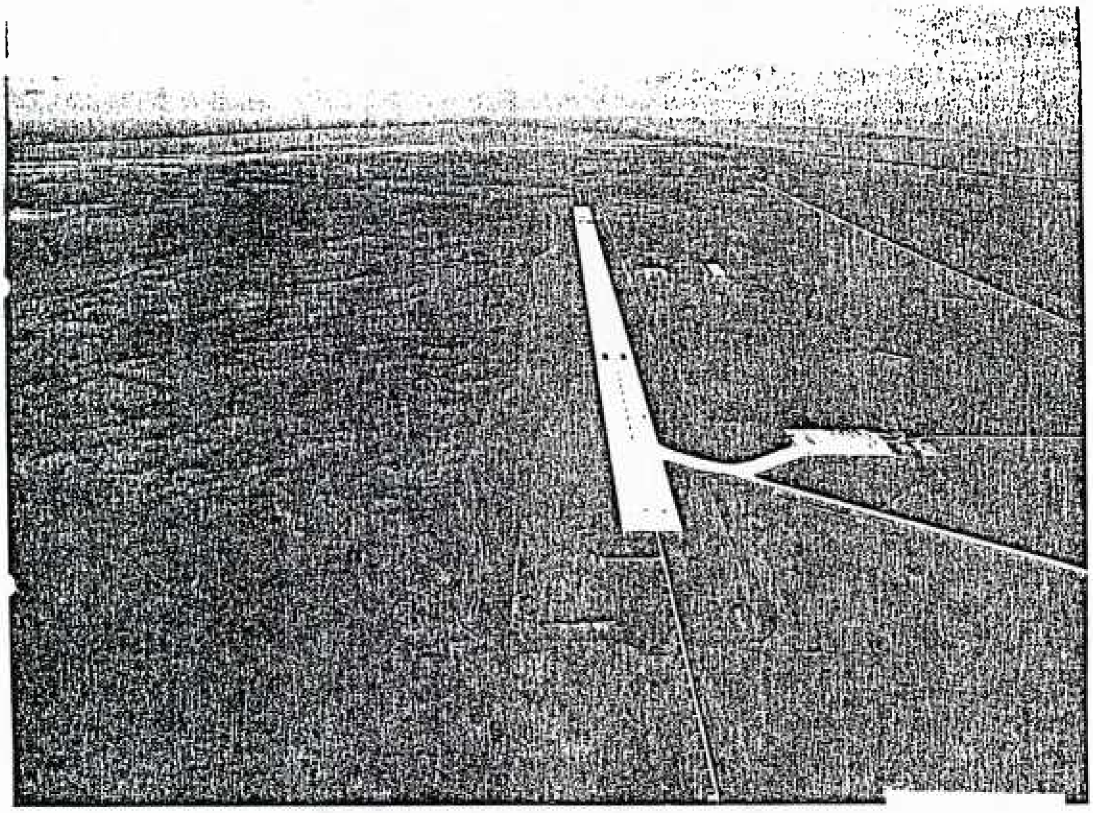
179
Orbiter
Towed

[Ref. 2/6-82]



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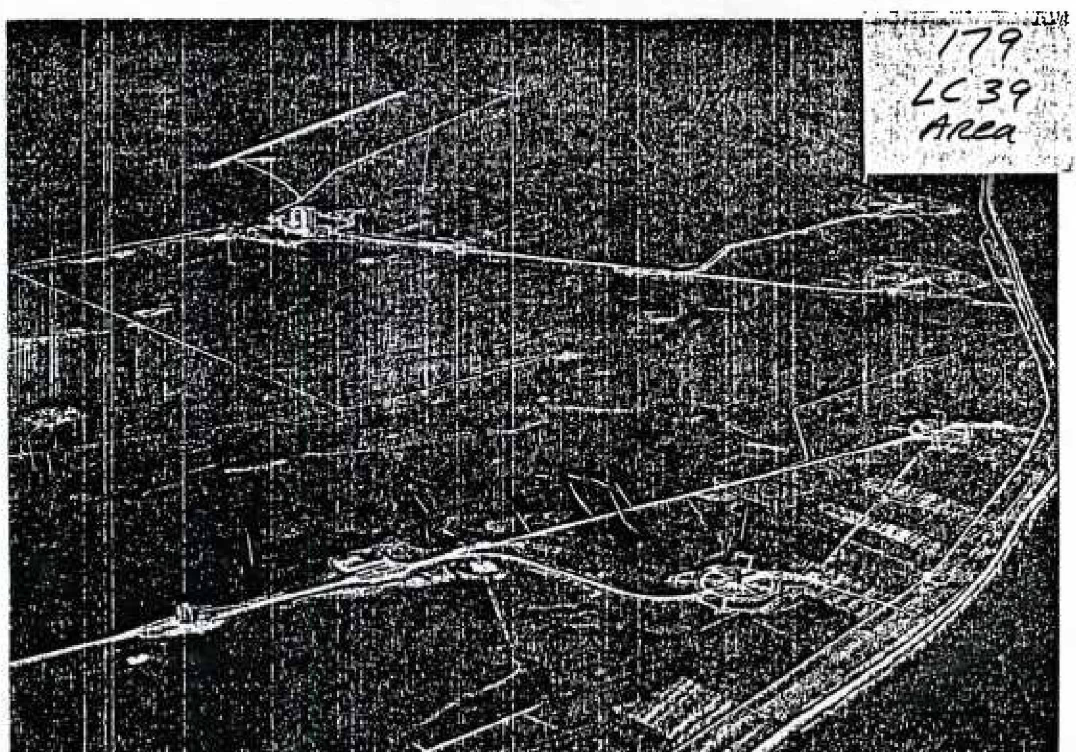

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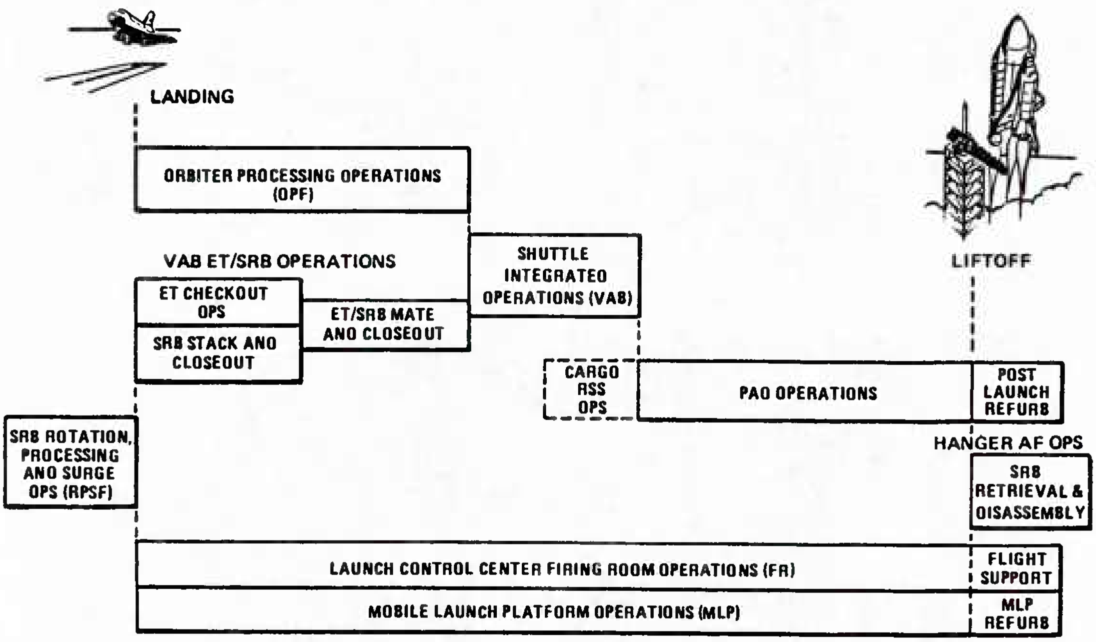
KSC SHUTTLE GROUND TURNAROUND OPERATIONS



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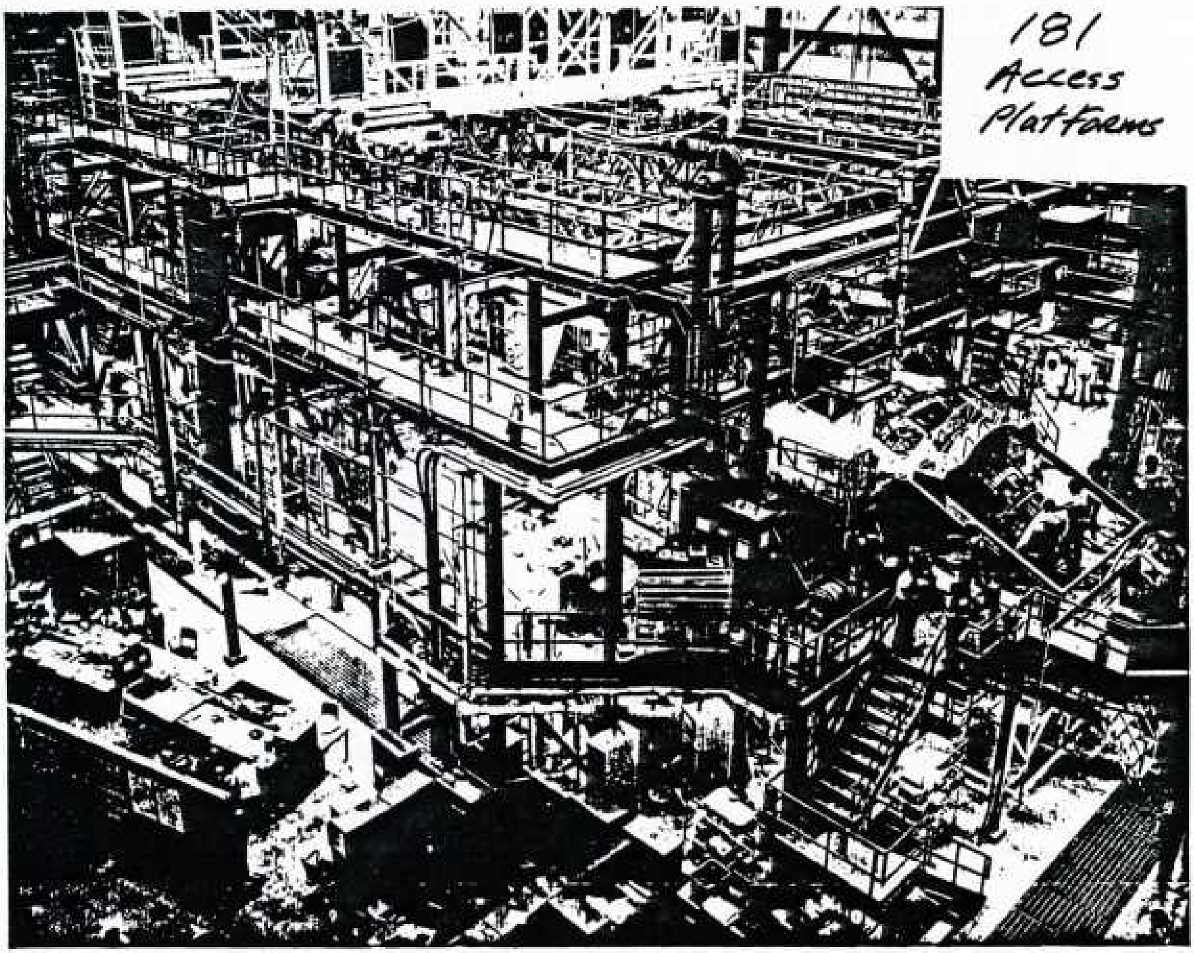
181
Access
Platforms



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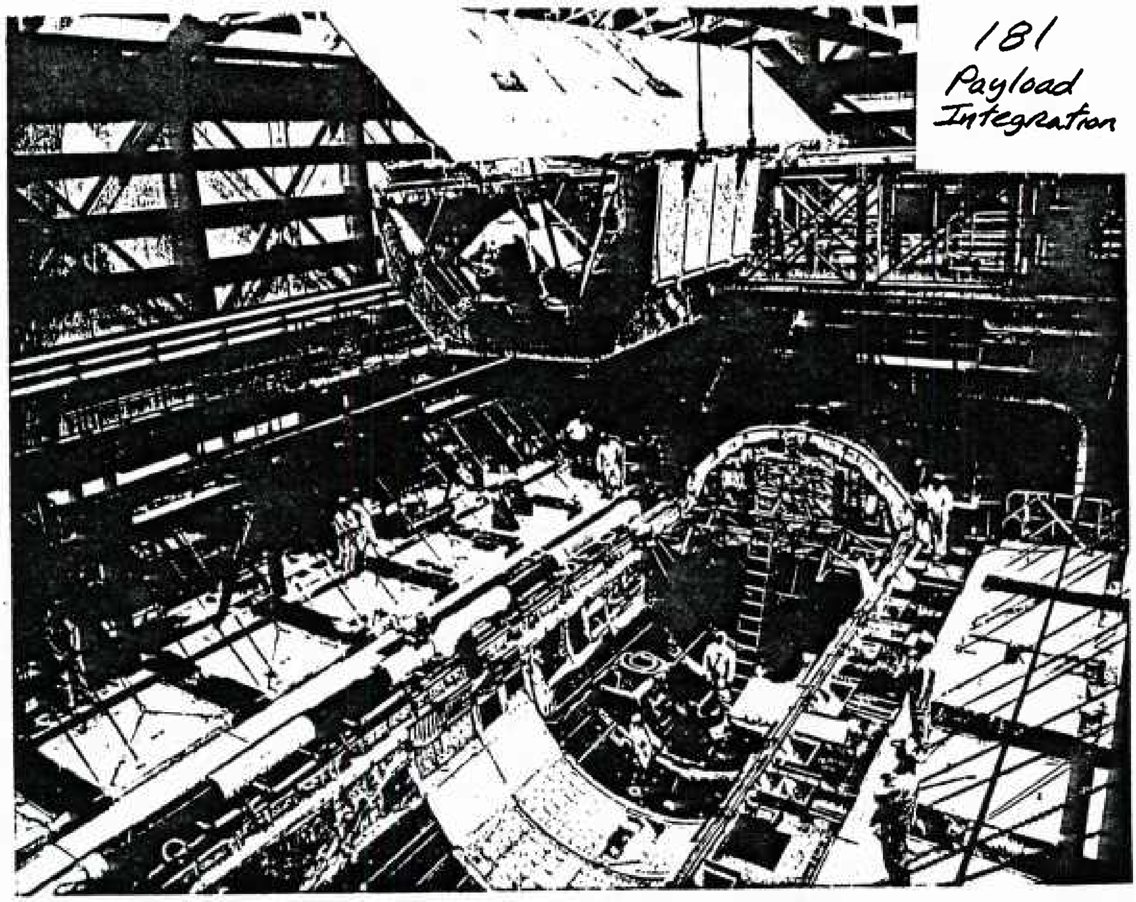
181
Payload
Integration



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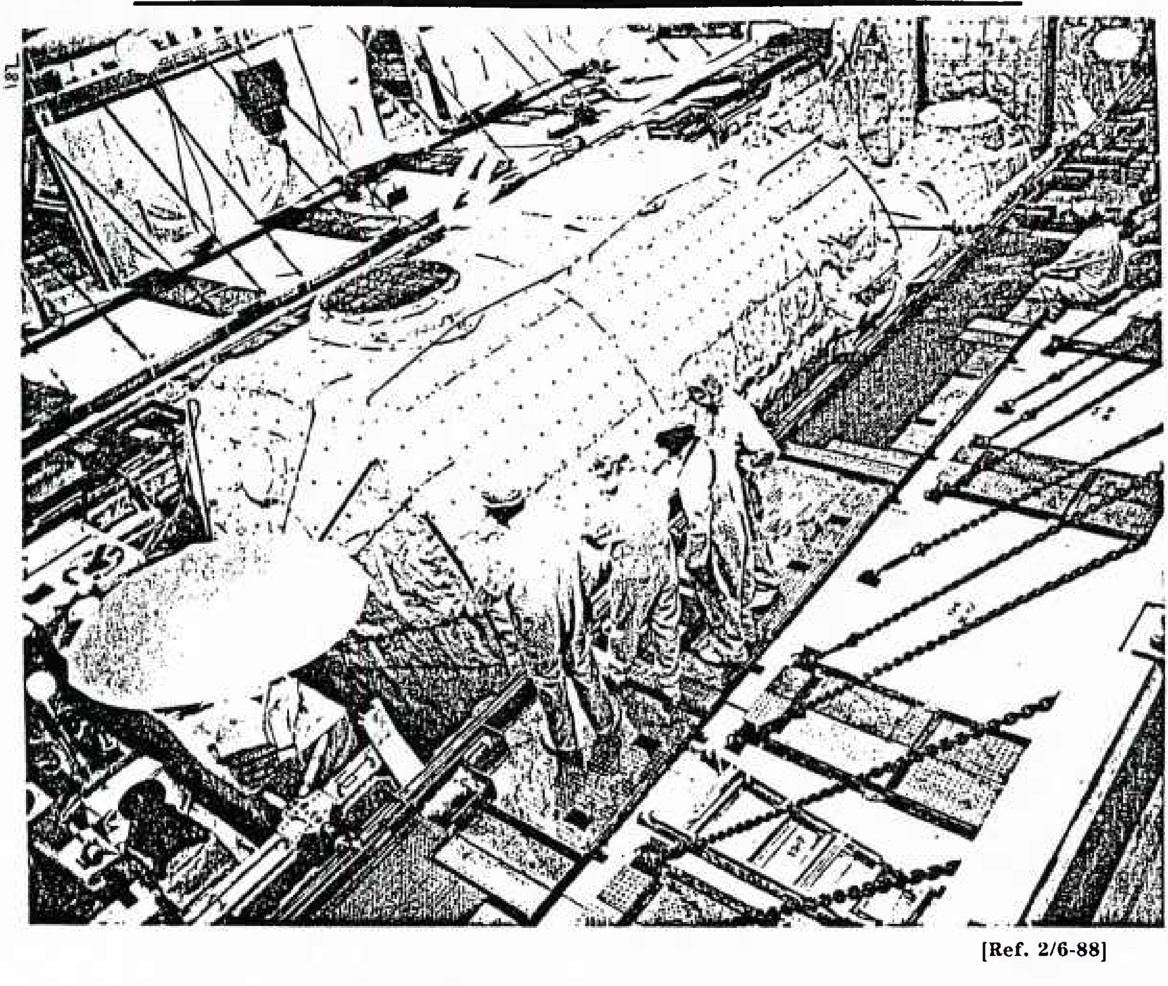

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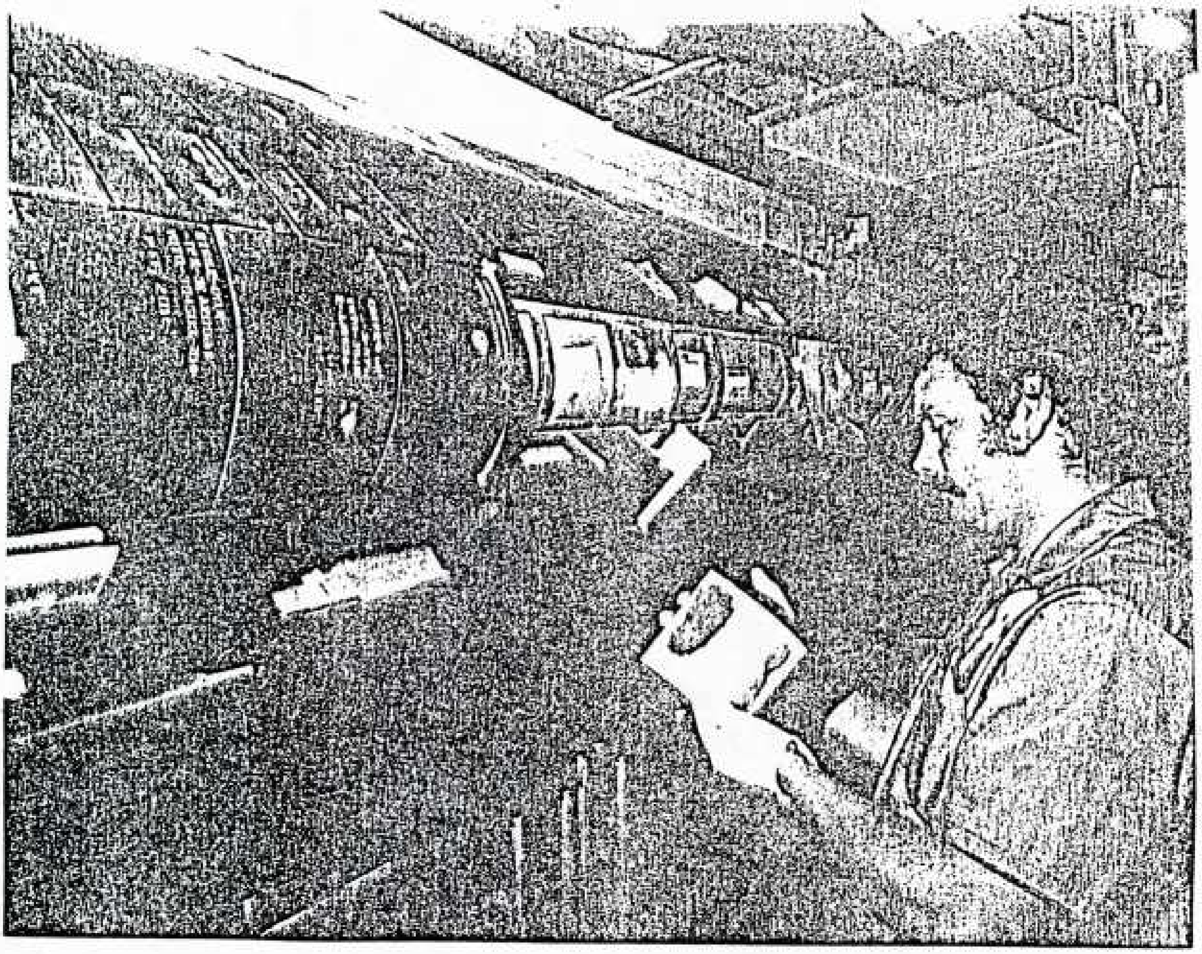
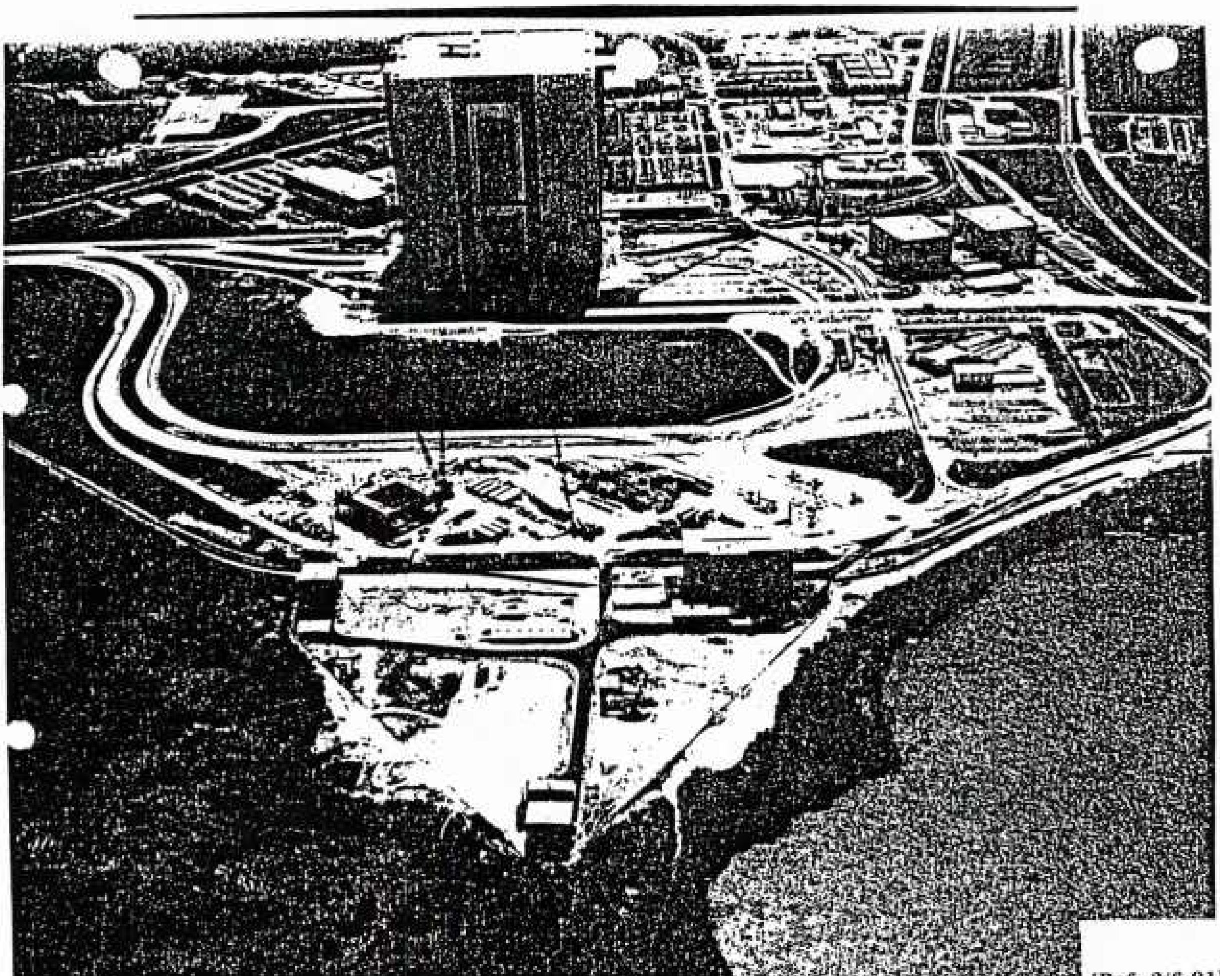


[Ref. 2/6-89]

SRB ROTATION, PROCESSING AND SURGE FACILITY (RPSF)

- DESCRIPTION
  - CONSISTS OF FOUR BUILDINGS LOCATED NORTH OF VAB
    - OFFLOAD, ROTATION AND PROCESSING BUILDING CONSISTING OF AFT BOOSTER BUILDUP WORKSTANDS, 200 TON OVERHEAD BRIDGE CRANES, AND A RAIL TRACK WHICH TRAVERSES THROUGH THE BUILDING
    - TWO SURGE BUILDINGS FOR STORAGE OF PROCESSED SRM COMPONENTS PRIOR TO STACKING
    - SUPPORT BUILDING
- OPERATIONS
  - OFFLOAD, ROTATION AND PROCESSING BUILDING
    - RECEIVING, OFFLOAD, ROTATION AND INSPECTION OF SRM SEGMENTS ARRIVING FROM MANUFACTURER IN UTAH VIA RAILCAR
    - PLACEMENT OF PROCESSED SEGMENTS ON TRANSPORTATION AND STORAGE PALLETS

[Ref. 2/6-90]



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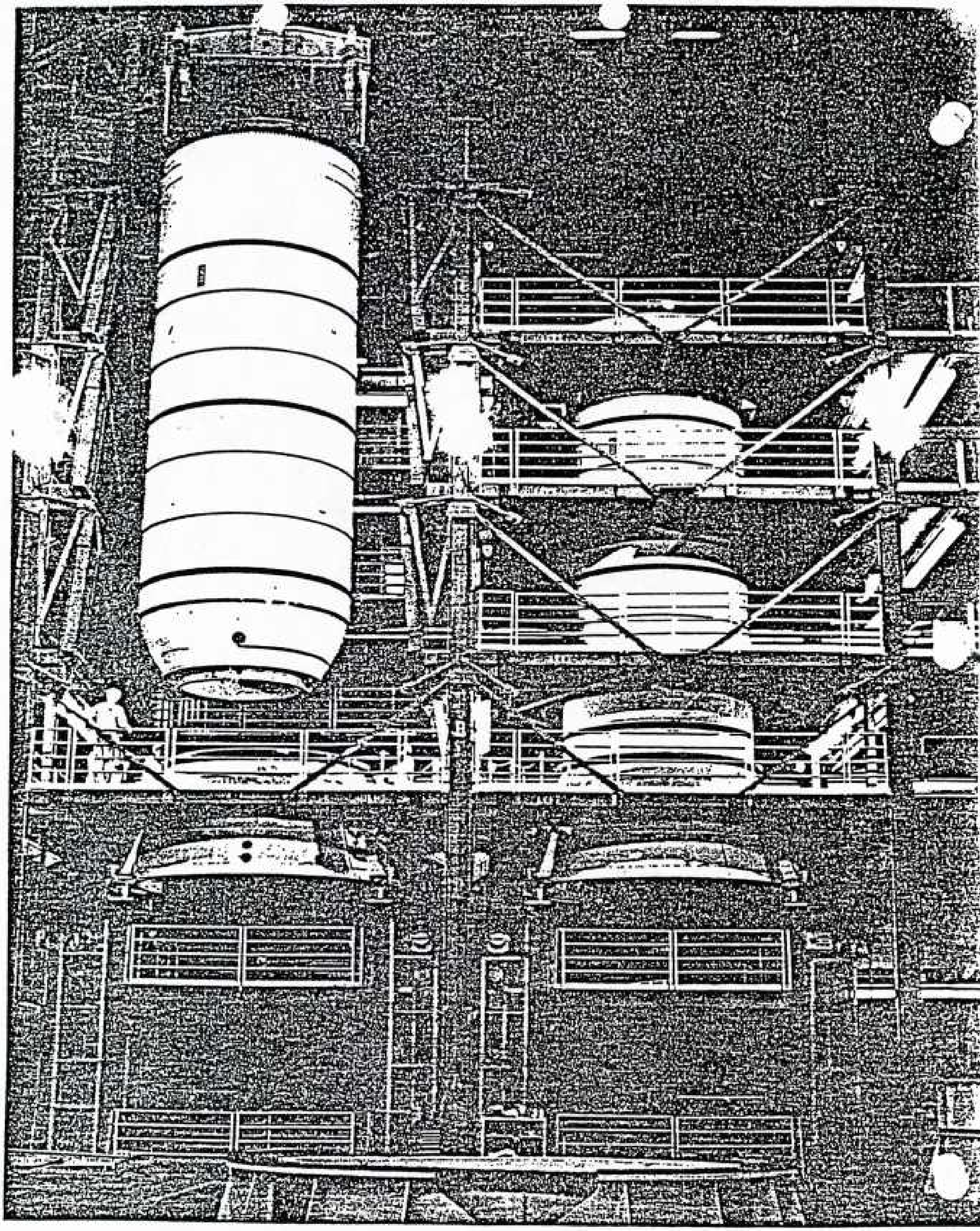

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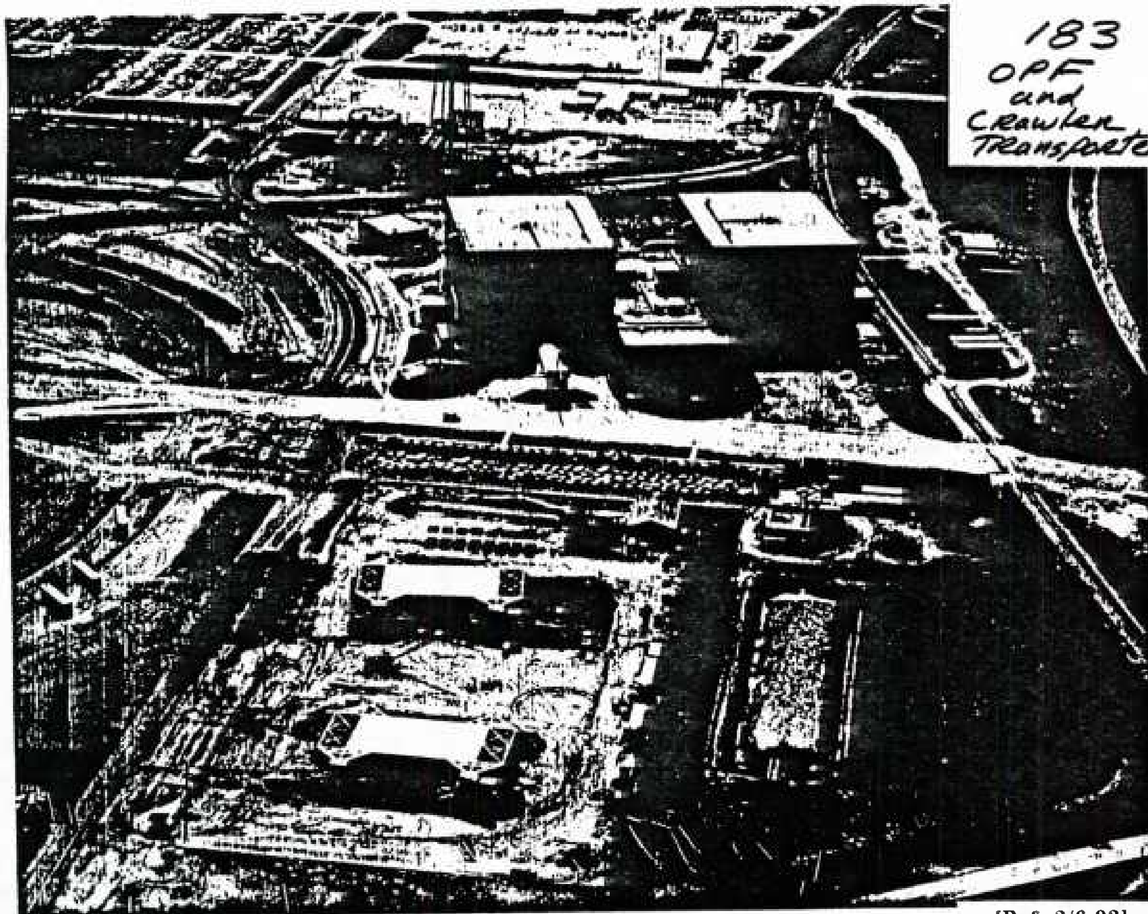


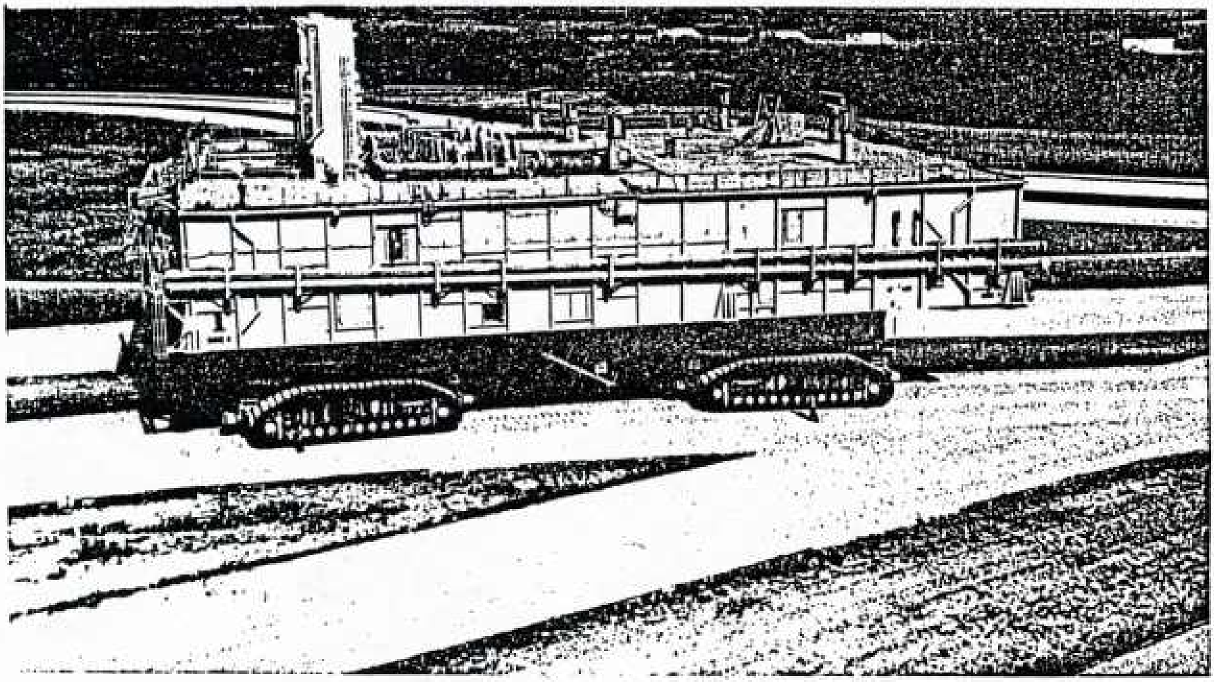
183
OPF
and
Crawler
Transporters

[Ref. 2/6-93]

MOBILE LAUNCHER PLATFORMS (MLP)

- DESCRIPTION
  - TWO STORY TRANSPORTABLE LAUNCH BASES
  - EXTERIOR PROVIDES
    - SRB HOLDDOWNS
    - TAIL SERVICE MASTS
    - SOUND SUPPRESSION WATER NOZZLES
  - INTERIOR PROVIDES GROUND SUPPORT EQUIPMENT FOR SHUTTLE CHECKOUT, SERVICING AND LAUNCH
  - TWO MLP'S OPERATIONAL, ONE UNDERGOING MODS
- OPERATION
  - BUILDUP AND MOVEMENT OF SHUTTLE TO PAD
  - LPS HARDWARE INTERFACE TO ORBITER, ET, AND SRB
  - PROPELLANT LOADING
  - SOUND SUPPRESSION, OVERPRESSURE REDUCTION, AND MLP PROTECTION DELUGE
  - REFURBISH AFTER LAUNCH

[Ref. 2/6-94]



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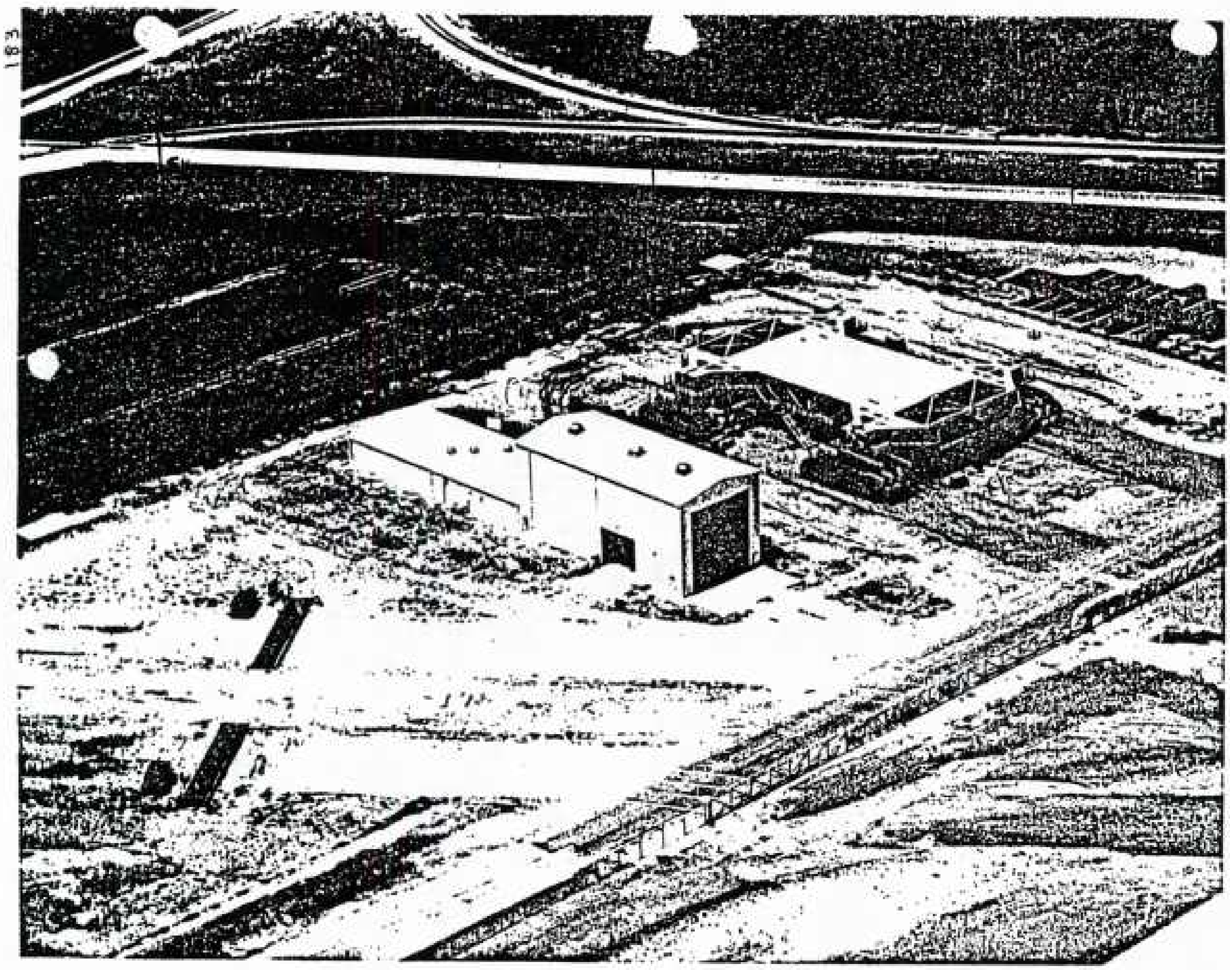

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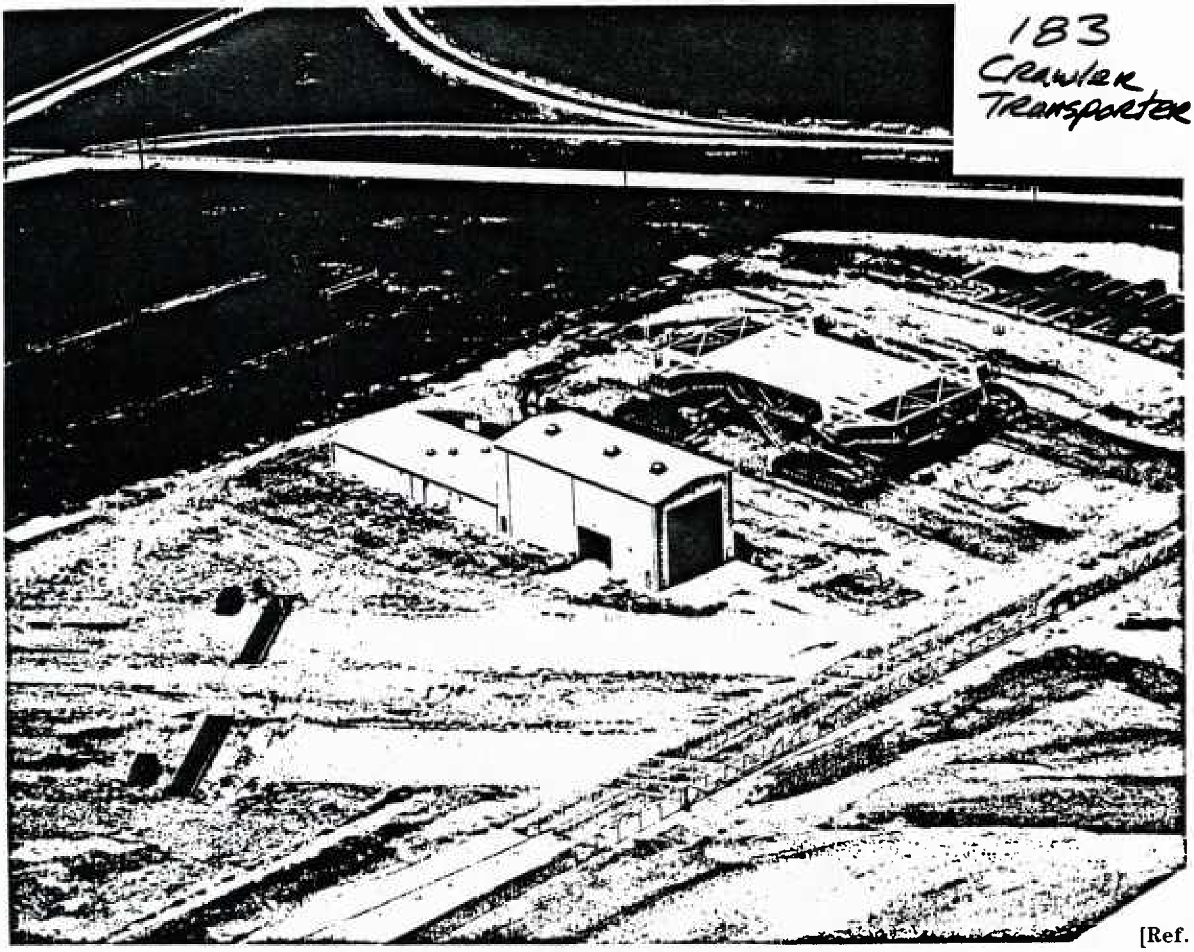


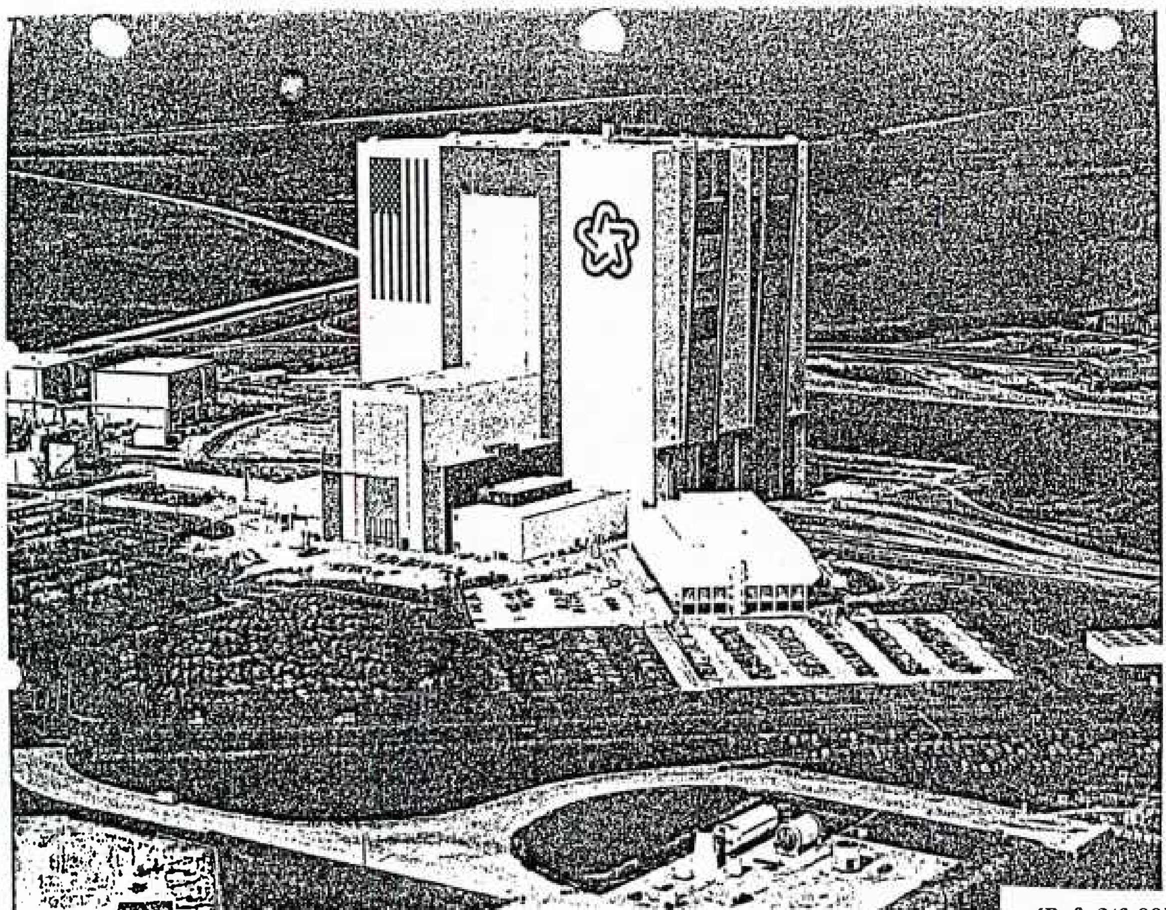
183
Crawler
Transporter

[Ref. 2/6-97]

VEHICLE ASSEMBLY BUILDING (VAB)

- DESCRIPTION
  - HIGH BAY
    - FOUR SEGMENT BAYS WITH TWO BAYS ON EACH SIDE OF TRANSFER AISLE
    - HB 1 & 3 WITH EXTENDABLE PLATFORMS USED FOR SHUTTLE ASSEMBLY AND INTEGRATION ON A MOBILE LAUNCH PLATFORM
    - HB 4 CONTAINS SRB AFT BOOSTER BUILDUP STANDS
    - HB 2 & 4 EACH CONTAIN AN ET CHECKOUT CELL AND A STORAGE CELL
    - HB 2 CAN ACCOMMODATE ORBITER STORAGE
  - LOW BAY USED FOR SRB COMPONENT REFURBISHMENT AND SUBASSEMBLY; SSME REPAIR AND MAINTENANCE SHOP
  - TRANSFER AISLE AND HIGH BAYS CONTAIN OVERHEAD CRANES FOR SHUTTLE ELEMENT AND CARGO CANNISTER MOVEMENT

[Ref. 2/6-98]



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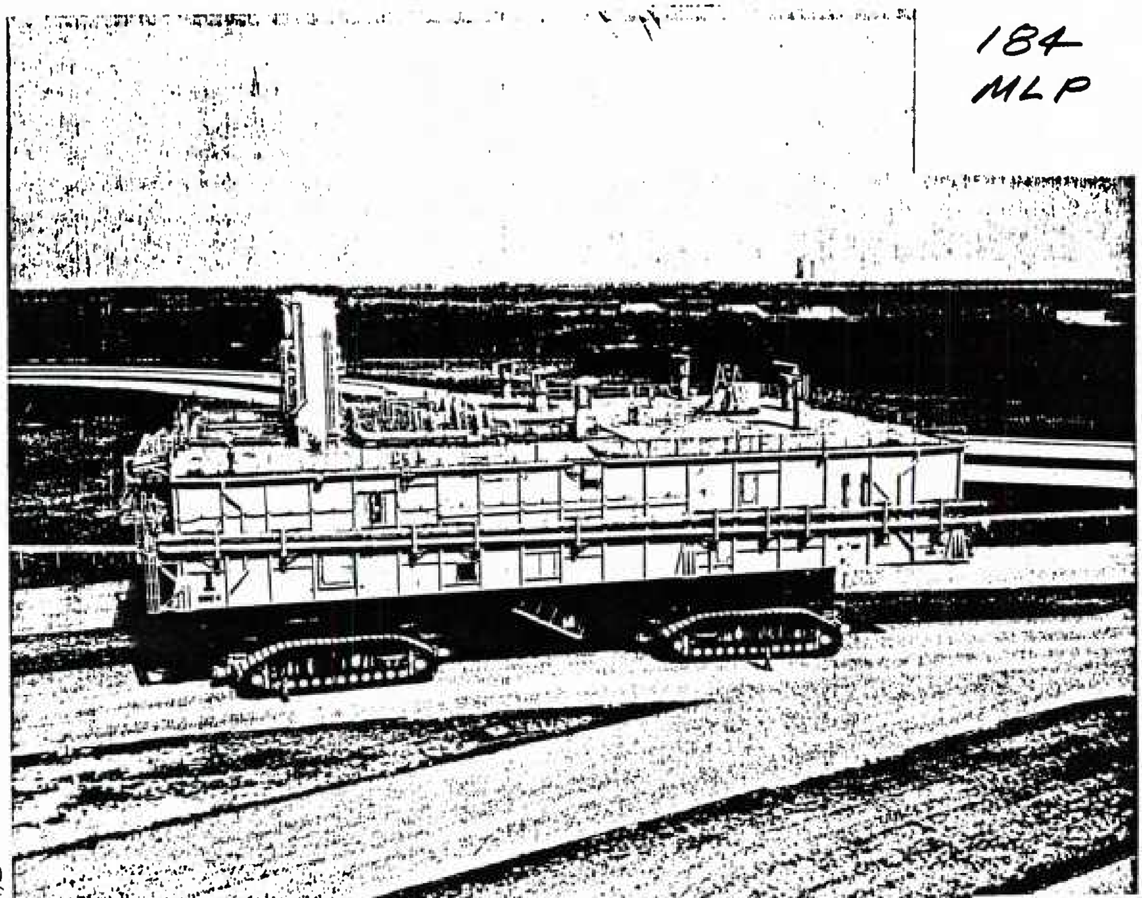
184
MLP



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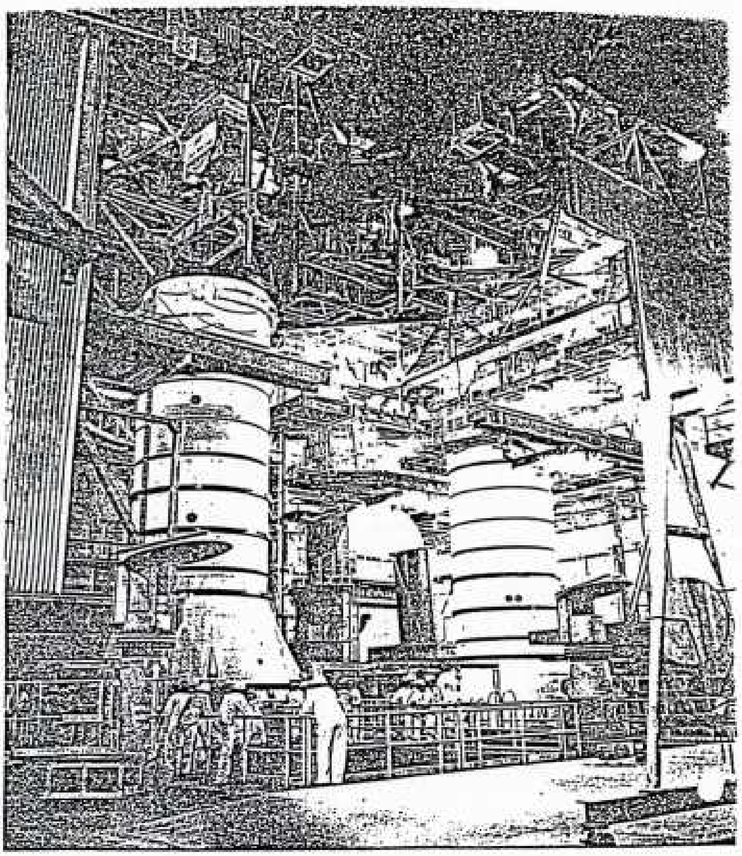

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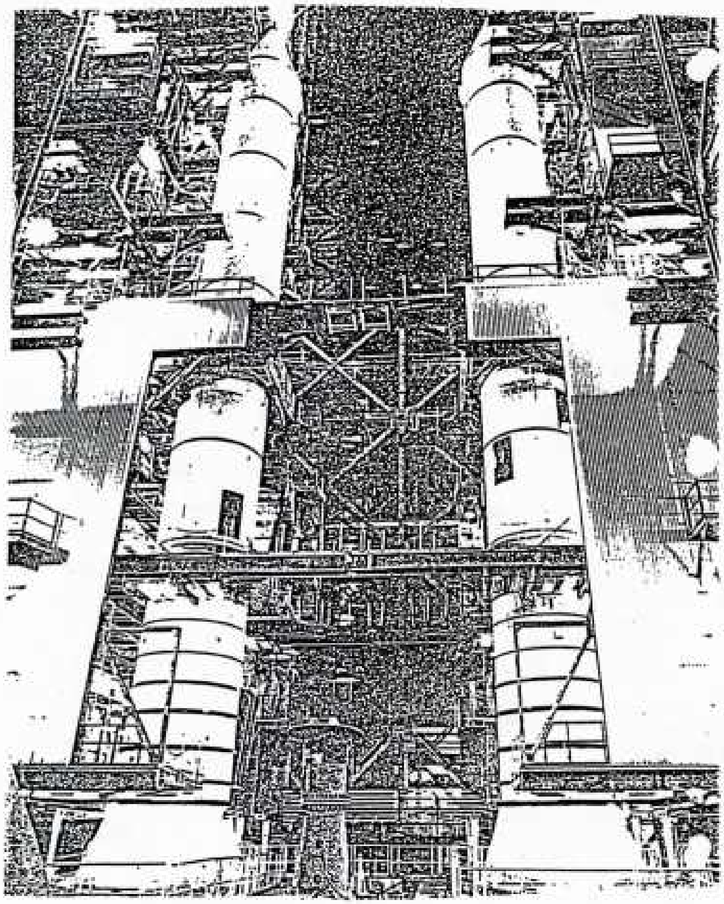

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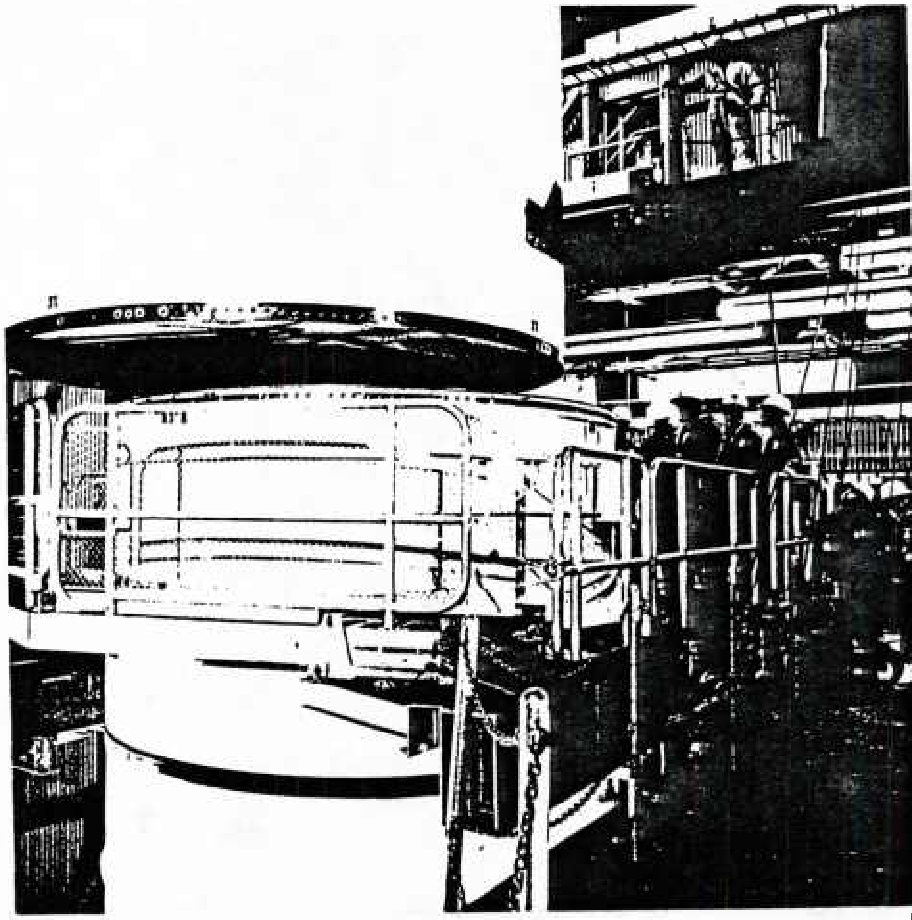

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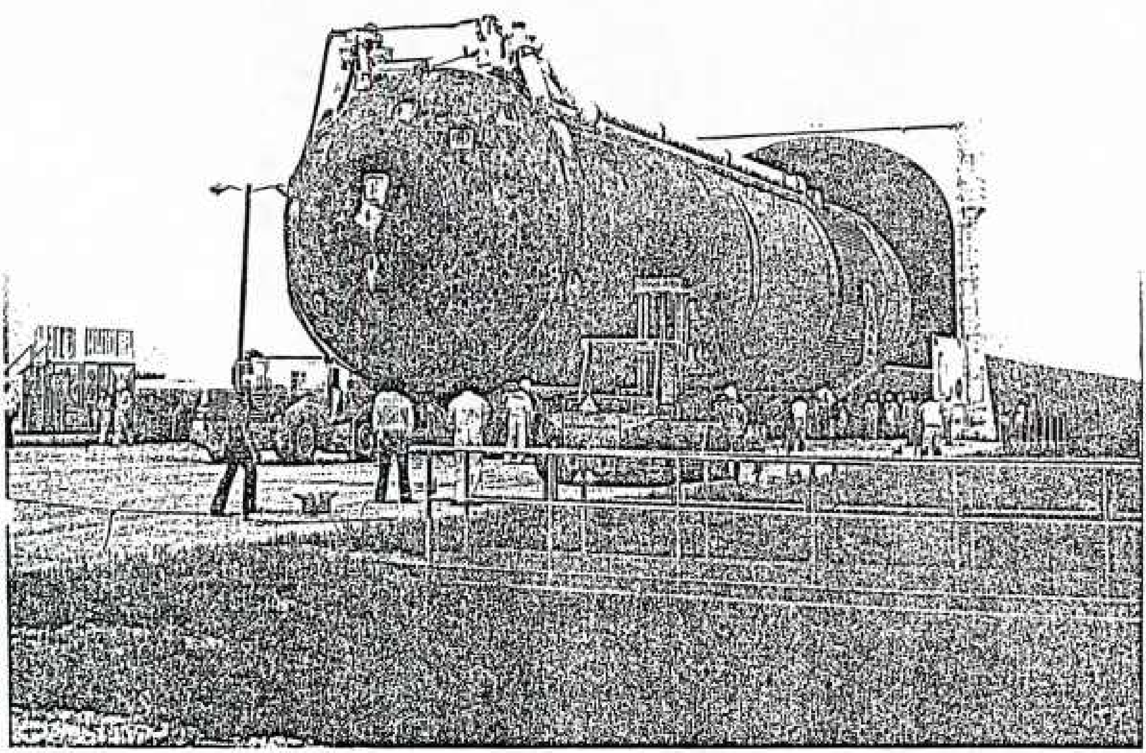

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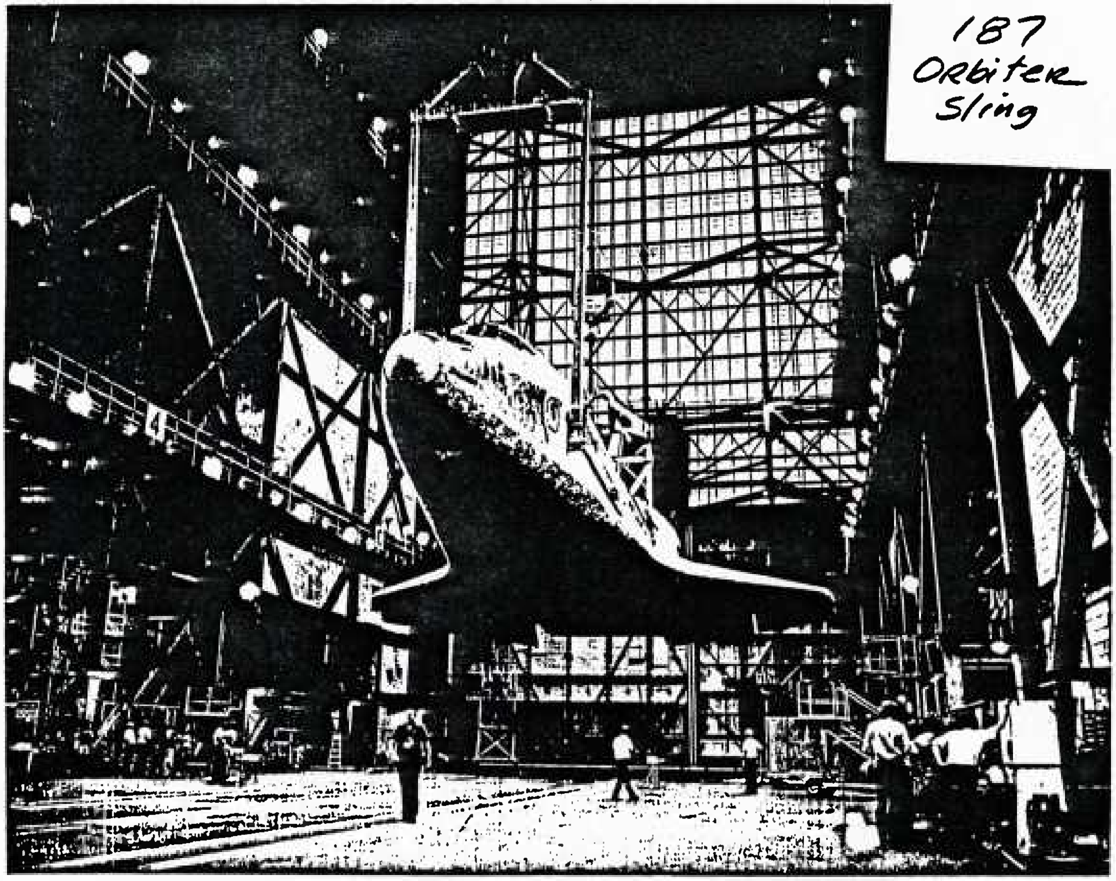
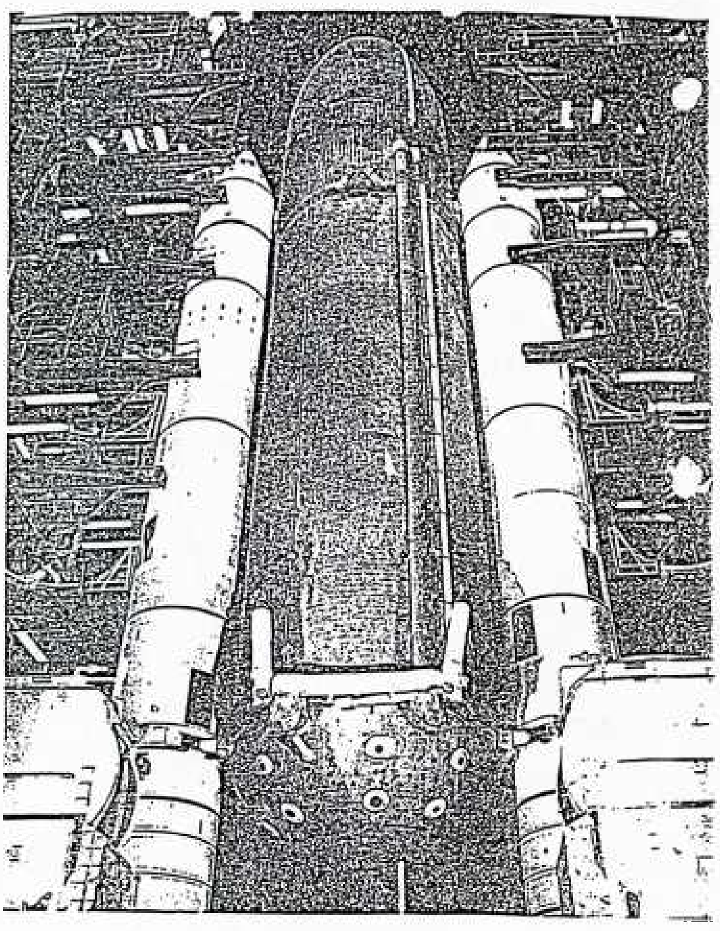


187
Orbiter
Sling

[Ref. 2/6-105]



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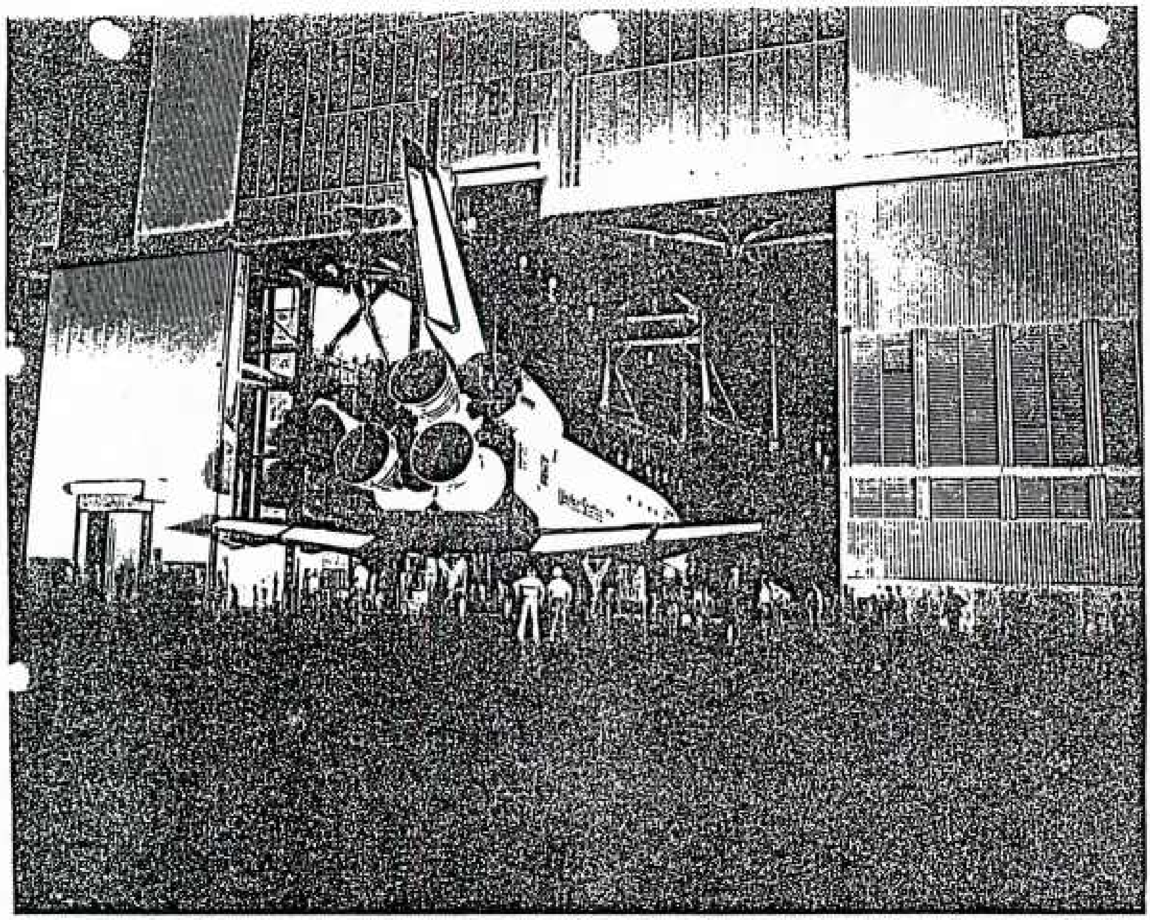
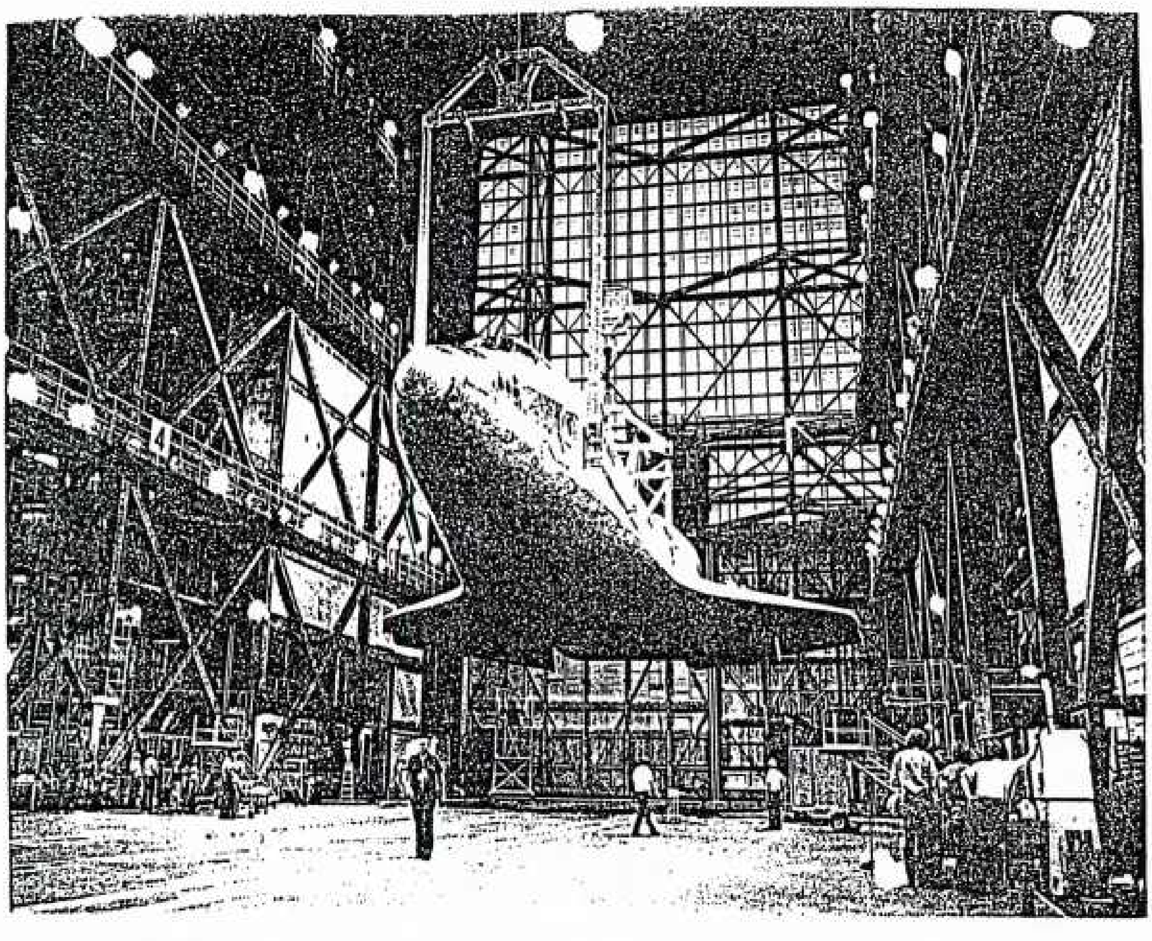


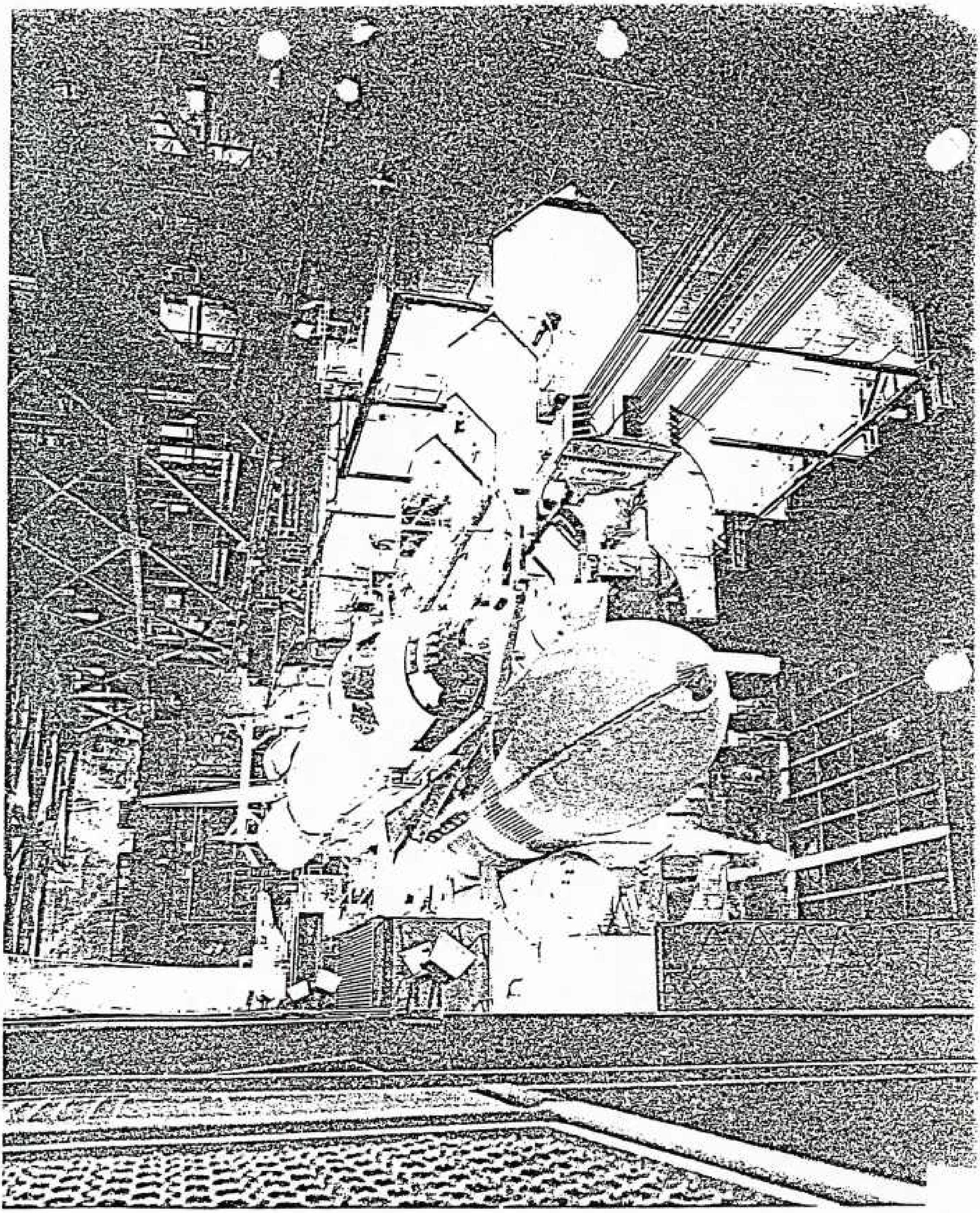
[Ref. 2/6-107]

187



[Ref. 2/6-108]

[Ref. 2/6-109]



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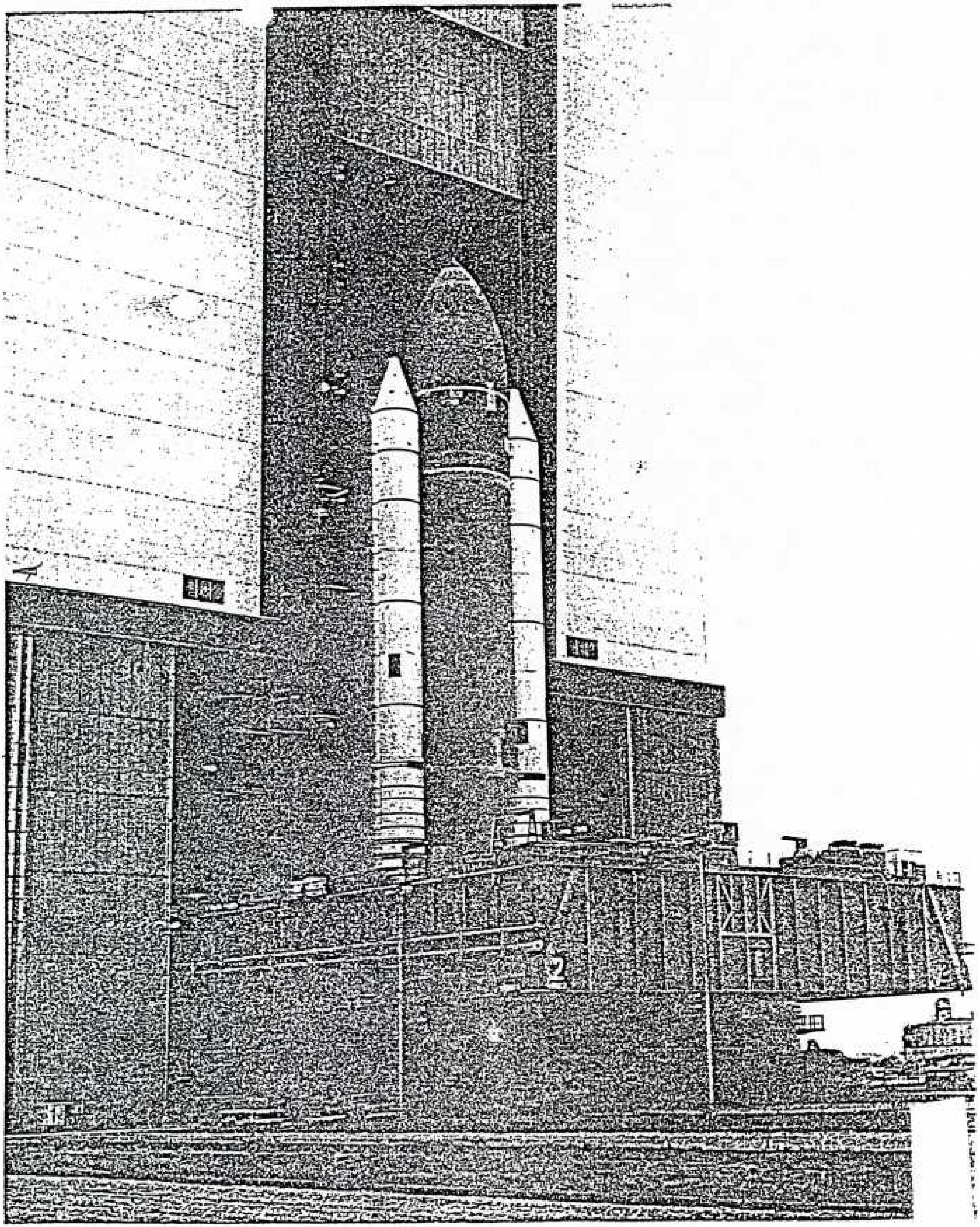


[Ref. 2/6-111]

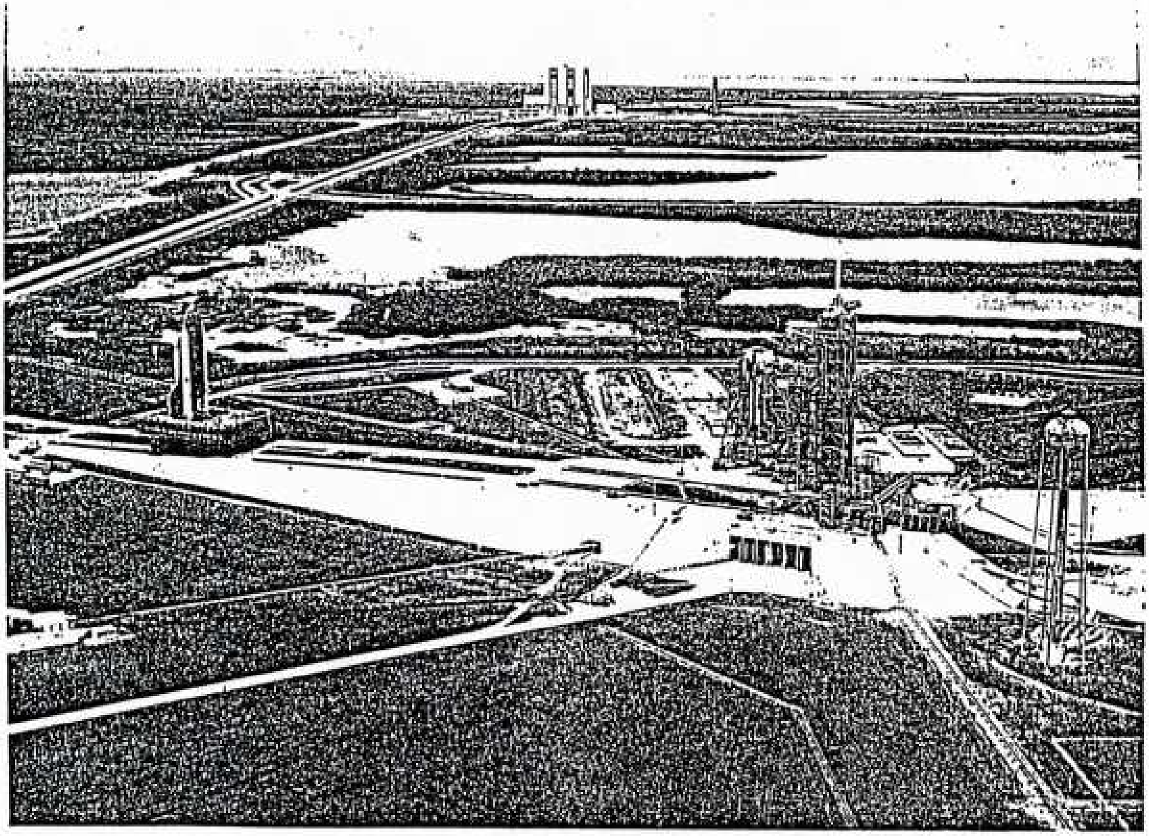
LAUNCH PAD 39A AND 39B

- DESCRIPTION
  - BOTH PADS ARE OCTAGONAL IN SHAPE AND CONTAIN ABOUT 1/4 SQUARE MILE LAND.
  - EACH PAD CONTAINS SIX PEDESTALS TO SUPPORT THE MOBILE LAUNCHER PLATFORM (MLP), FUEL AND OXIDIZER SUPPORT AREAS, FIXED SERVICE STRUCTURE (FSS) AND A ROTATING SERVICE STRUCTURE (RSS)
- OPERATIONS
  - PAYLOAD TRANSFER FROM P/L CANNISTER TO PAYLOAD CHANGE OUT ROOM (PCR)
  - SHUTTLE/PAD SYSTEM VALIDATION
  - PAYLOAD TRANSFER FROM PCR TO ORBITER USING PAYLOAD GROUND HANDLING MECHANISM (PGHM)
  - PAYLOAD/ORBITER INTERFACE TESTING
  - SHUTTLE HAZARDOUS SERVICING INCLUDING HYPERGOLIC FUEL AND CRYOGENIC FUEL AND OXIDIZERS
  - COUNTDOWN PREPS AND COUNTDOWN OPERATIONS FOR LAUNCH
  - PAD REFURBISH AFTER LAUNCH

[Ref. 2/6-112]

[NOT REPRODUCIBLE]

[Ref. 2/6-113]



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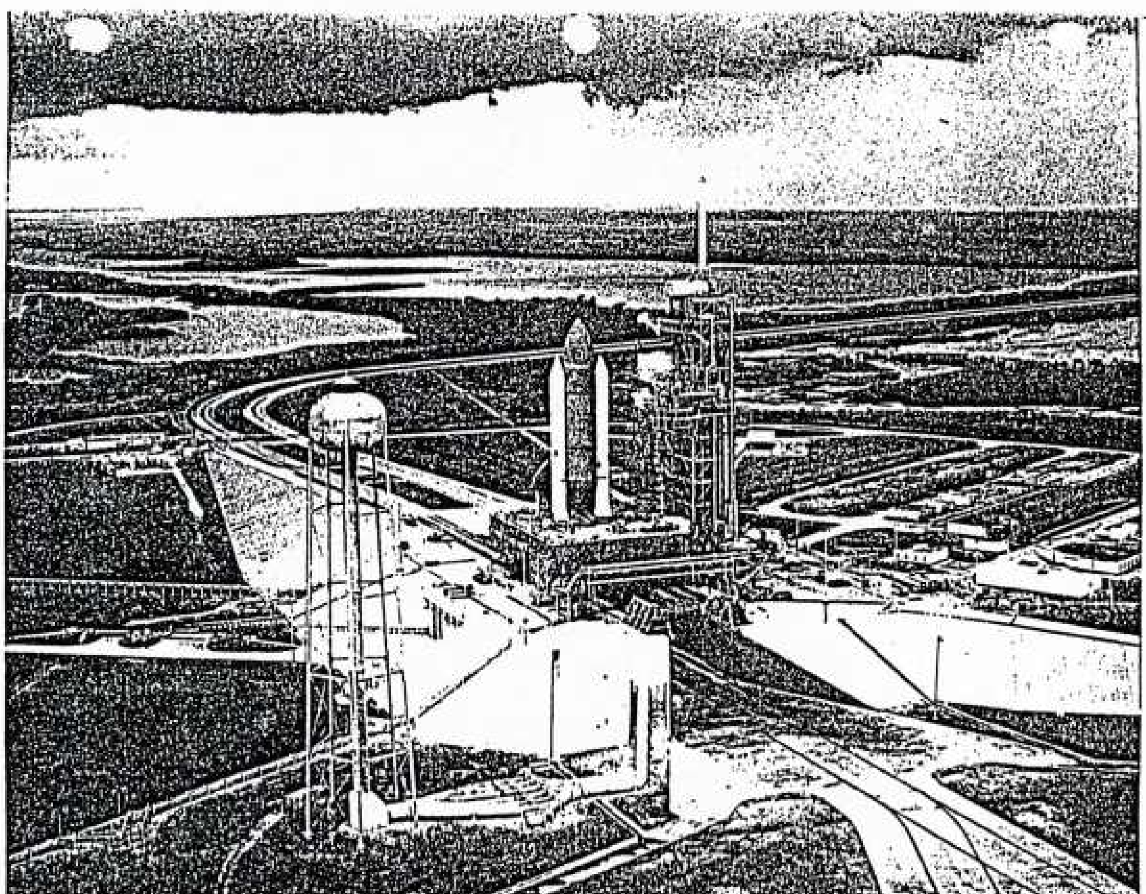

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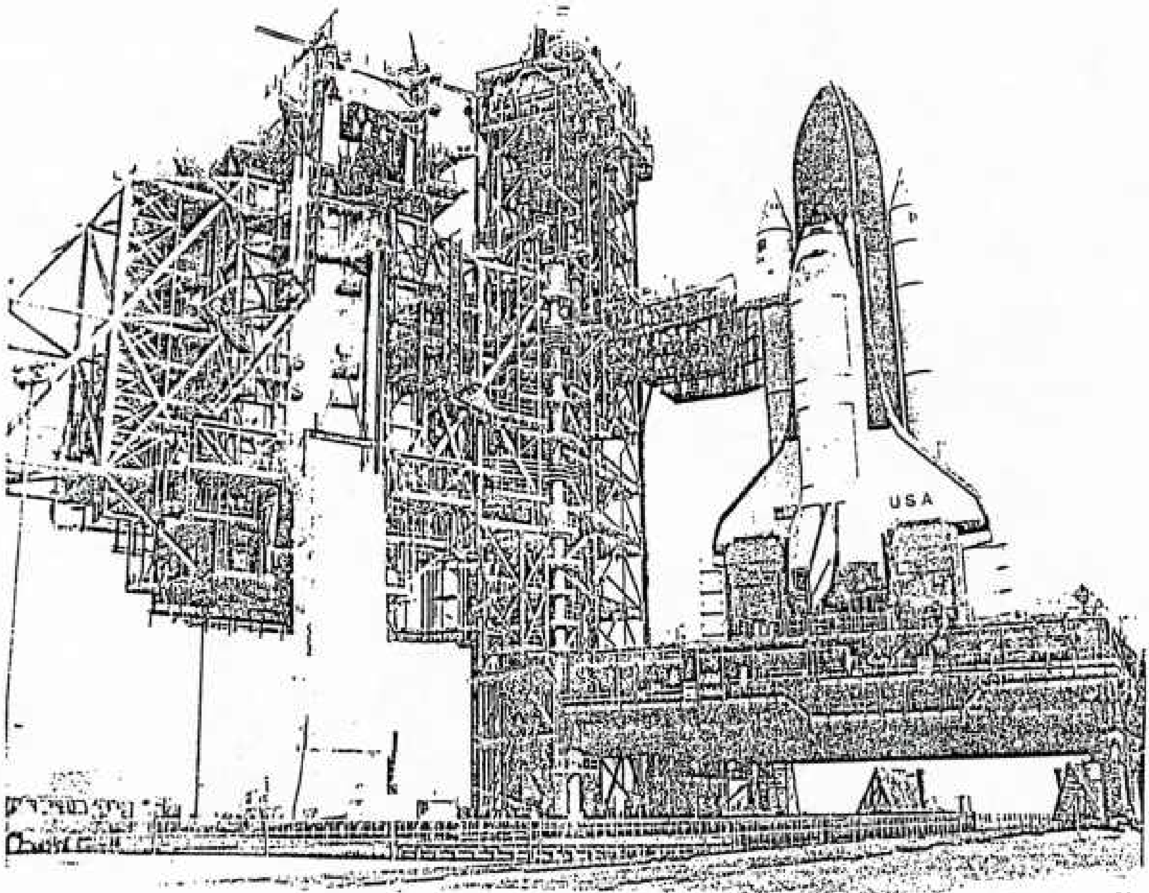

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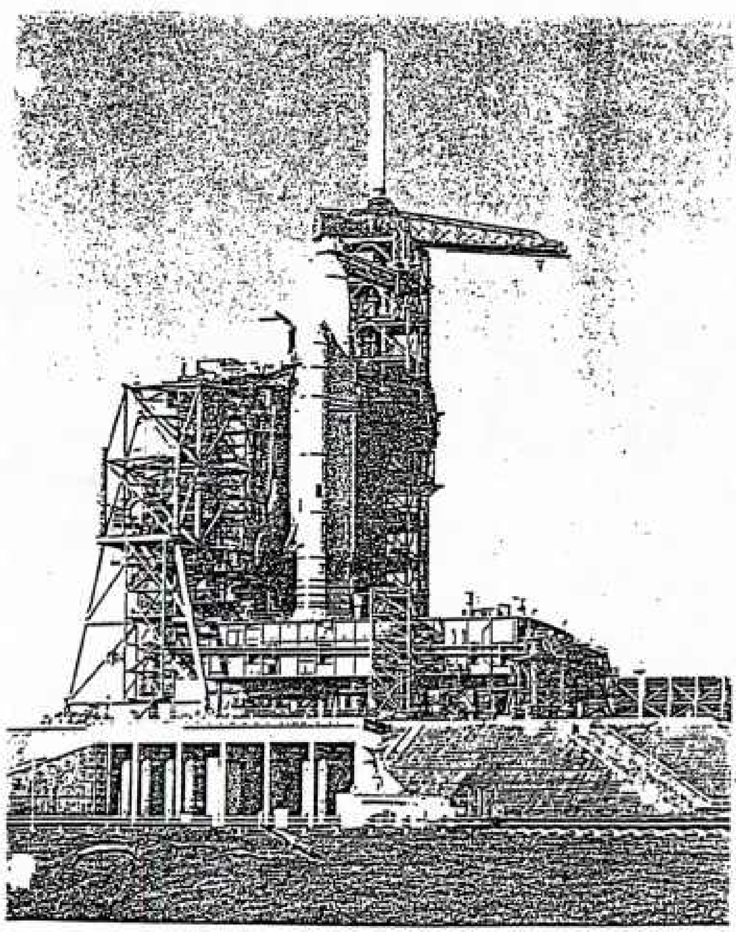
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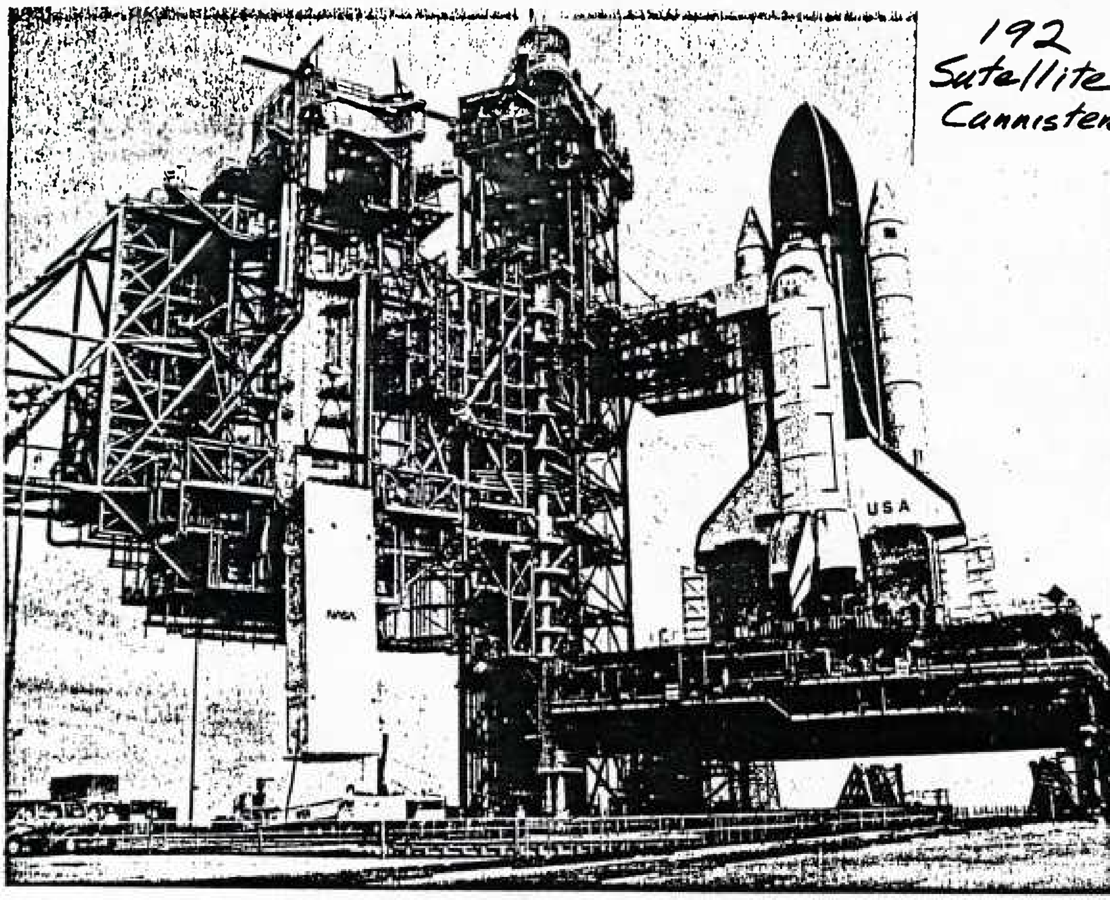
192
Satellite
Cannister



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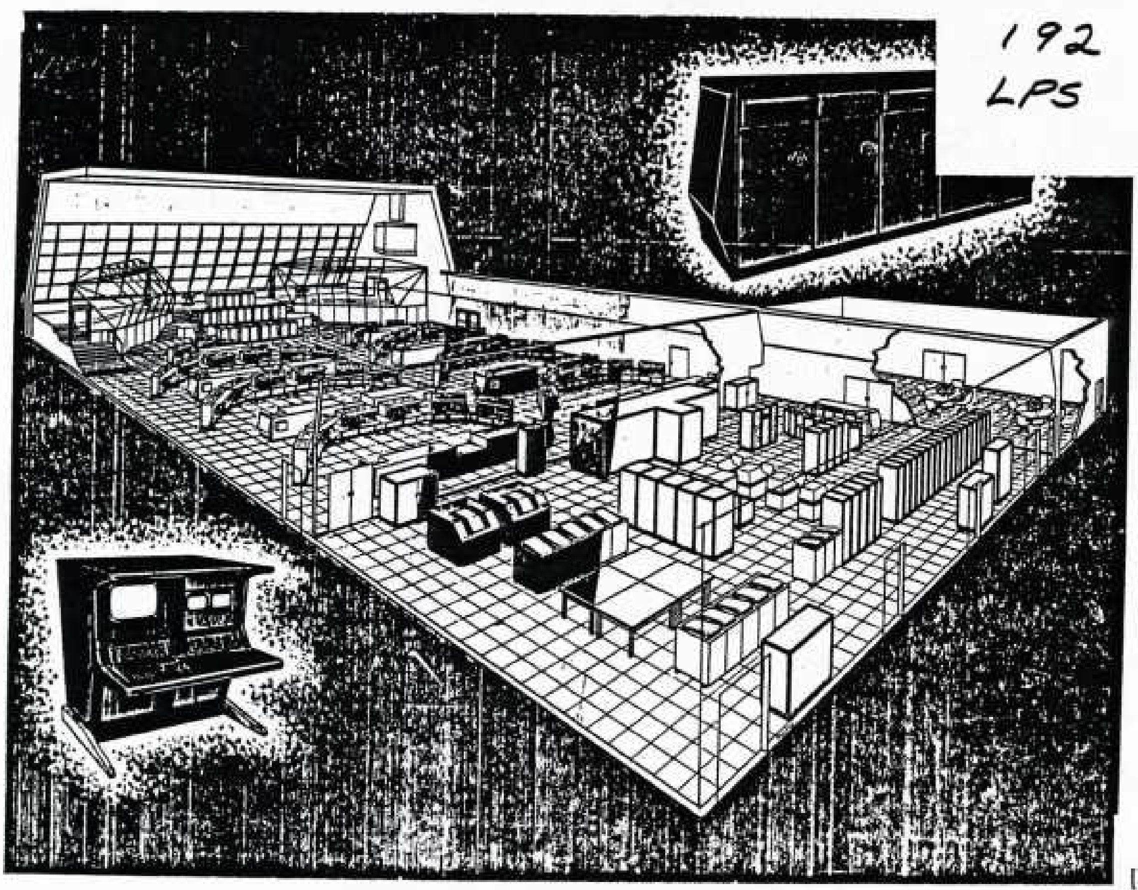
192
LPS



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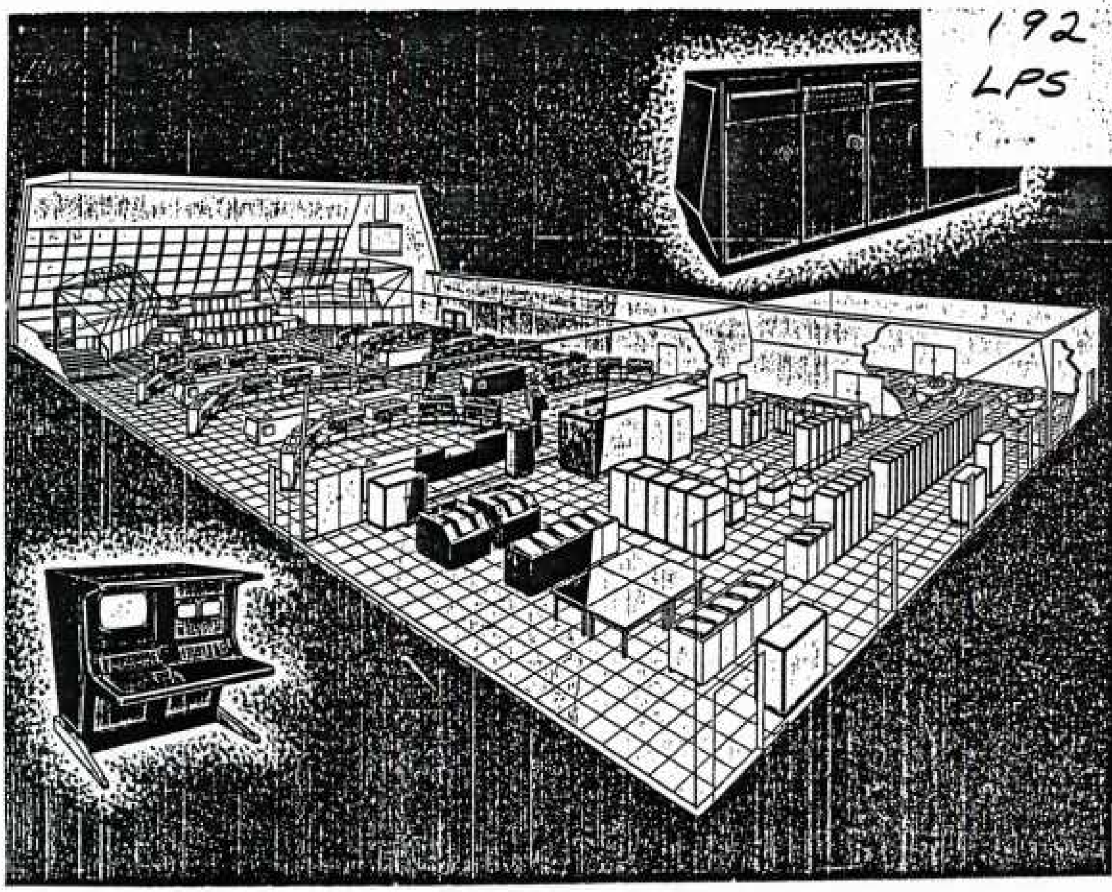
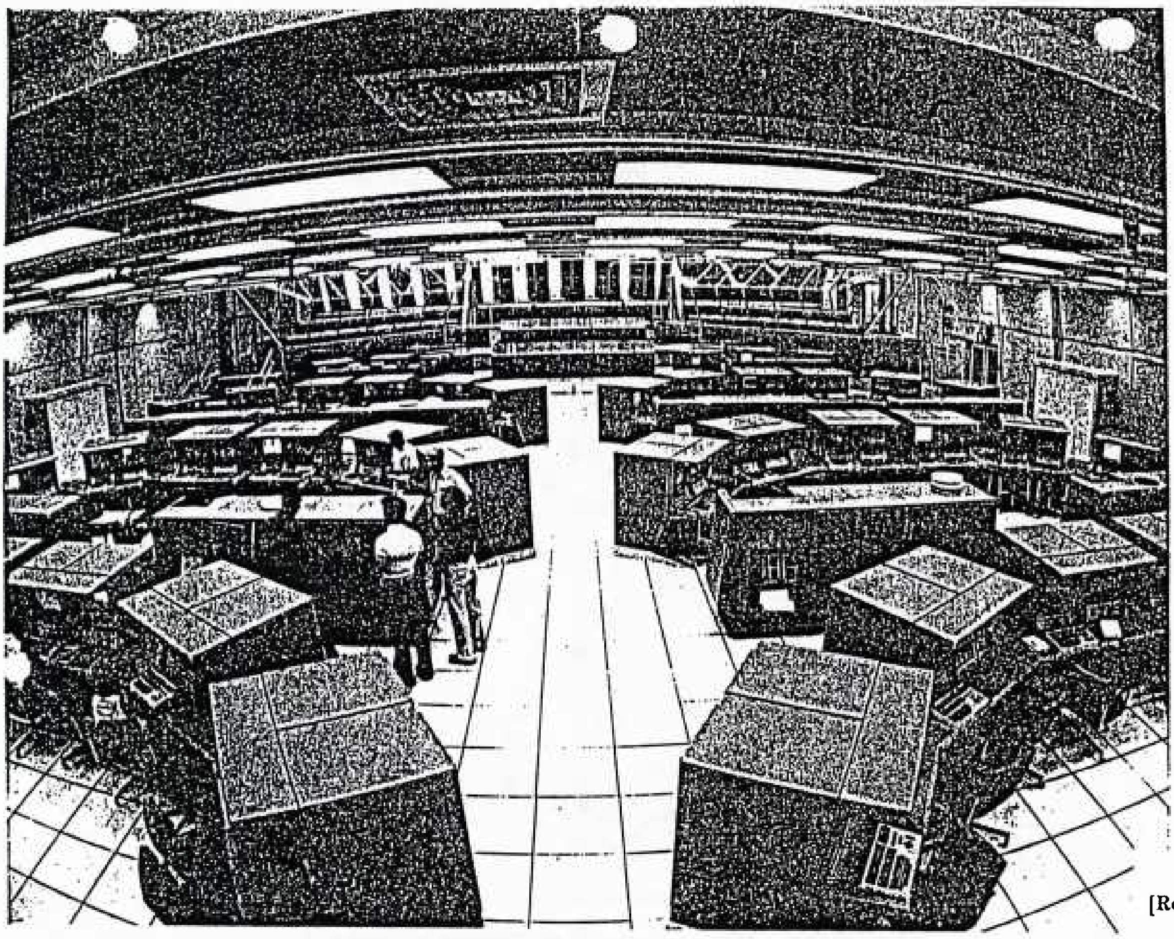


[Ref. 2/6-120]

[Ref. 2/6-121]

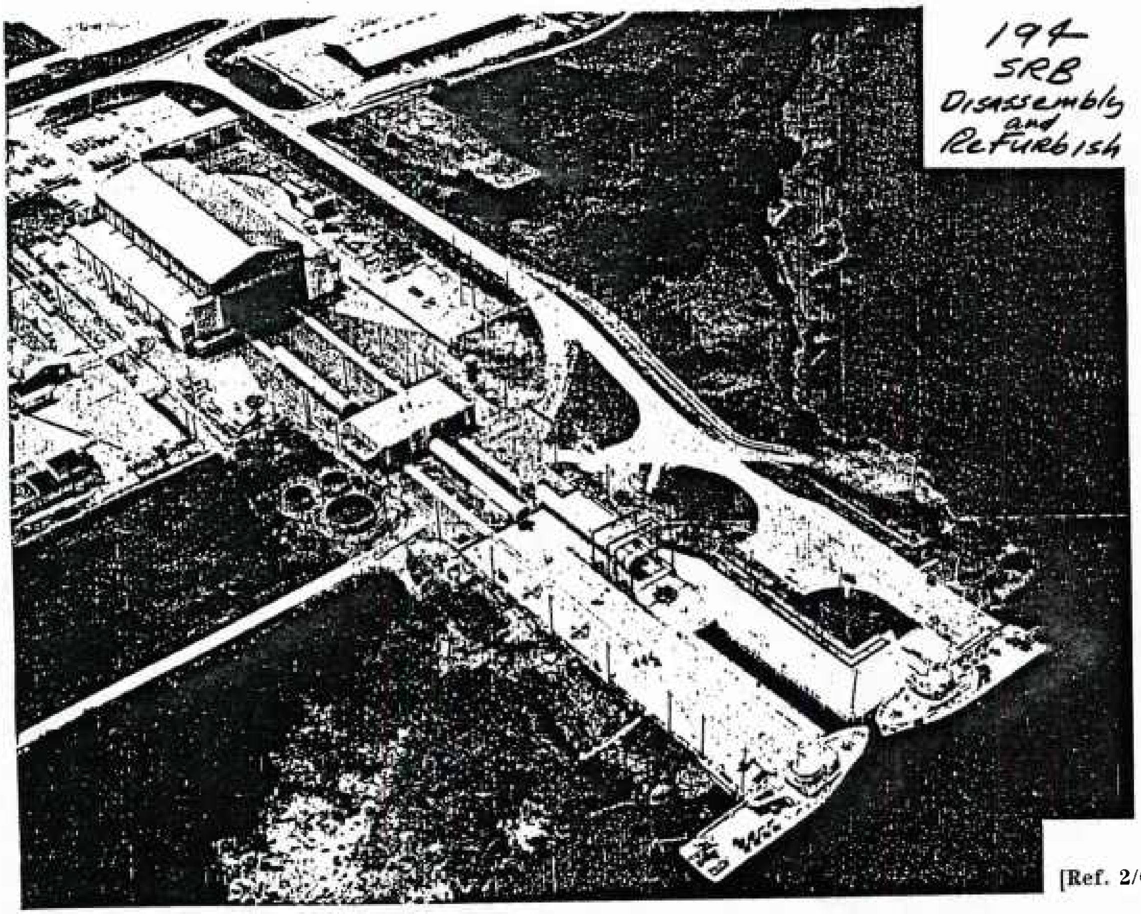


[Ref. 2/6-122]

HANGAR AF - SRB DISASSEMBLY FACILITY

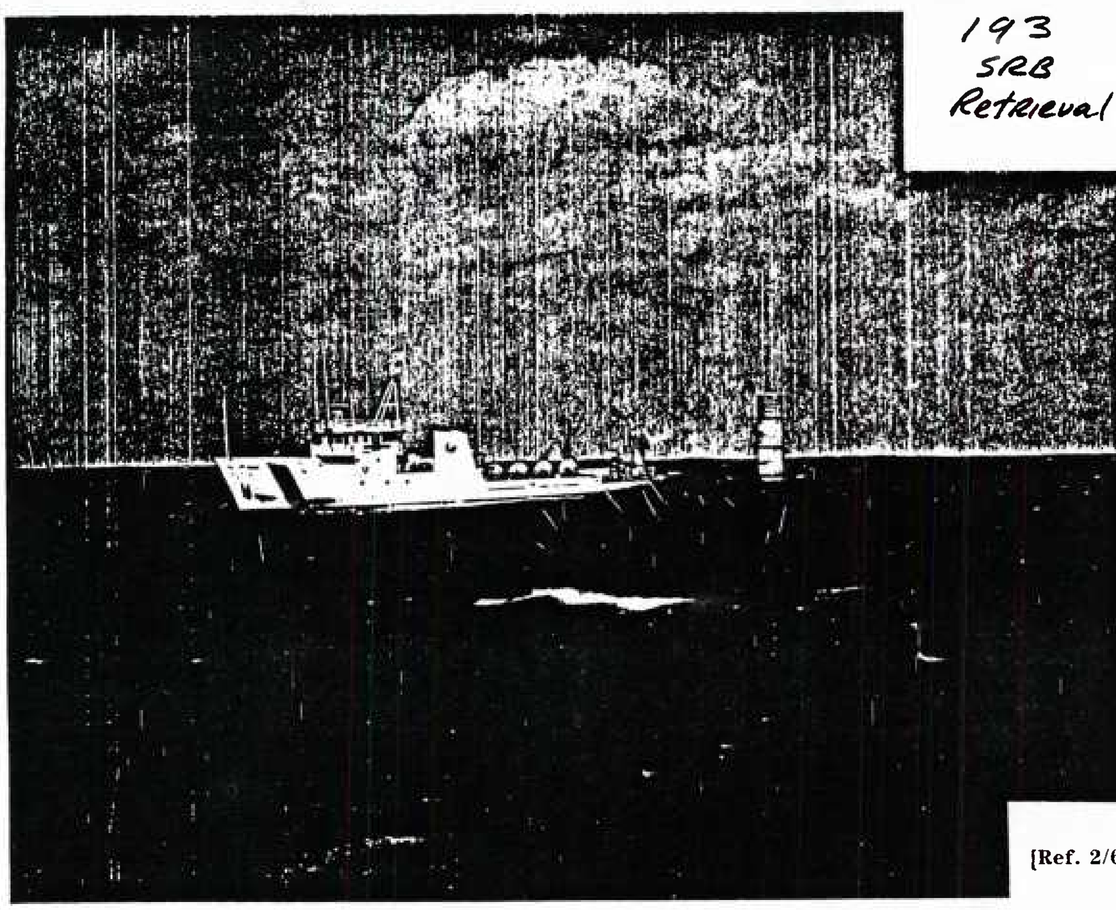
- DESCRIPTION
  - LOCATED AT CAPE CANAVERAL AIR FORCE STATION
  - BERTHING FOR TWO SRB RETRIEVAL SHIPS
- DISASSEMBLY OPERATIONS
  - CRANES STRADDLE SLIP LIFT SRB CASING OUT OF WATER AND PLACE ON HANDLING DOLLY
  - HAZARDOUS SYSTEMS SAFING
  - SRB CASING INITIAL WASH OPERATIONS
  - CASING DISASSEMBLY TO MAJOR ELEMENT LEVEL
  - AFT SKIRTS AND FORWARD SKIRTS UNDERGO HYDROLASER WASHING/DRYING BEFORE TRANSPORTATING TO VAB LOW BAY FOR REFURBISH
  - DISASSEMBLED SRB CASINGS ARE TRUCKED TO SRB PROCESSING AND SURGE FACILITY FOR RAILCAR ONLOADING AND SHIPMENT TO UTAH

[Ref. 2/6-123]



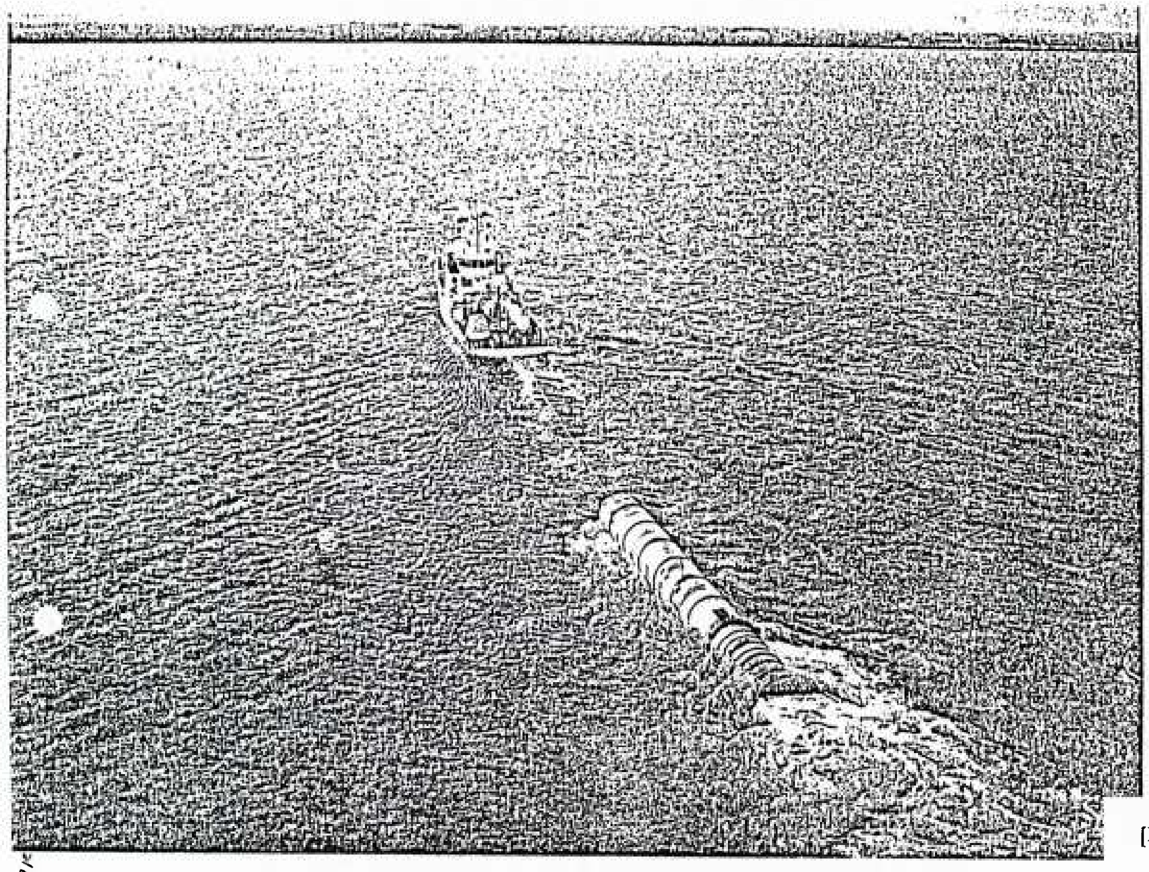
194
SRB
Disassembly
and
Refurbish

[Ref. 2/6-124]



193
SRB
Retrieval

[Ref. 2/6-125]



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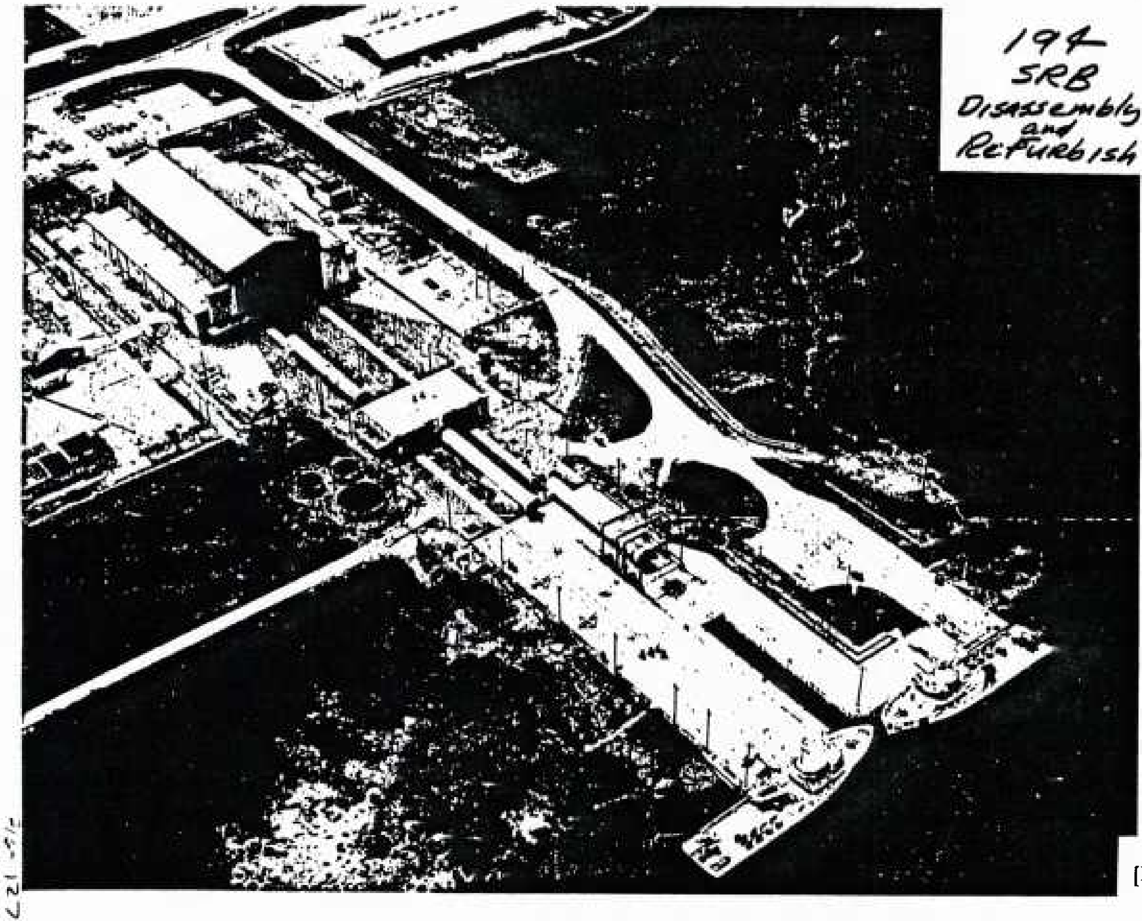

[Ref. 2/6-127]

SUPPORT FACILITIES

LOGISTICS BUILDING (LC–39)

- 300,000 SQ. FT.
- WAREHOUSE, PROCESSING SUPPORT FOR SHUTTLE SPARES

HYPERGOLIC MAINTENANCE FACILITY (INDUSTRIAL AREA)

- OFFLINE PROCESSING OF ORBITER FORWARD REACTION CONTROL SYSTEM MODULE, AFT ORBITER MANEUVERING SYSTEM MODULES.
- CHECKOUT (LPS), DESERVICING, MAINTENANCE

SRB PARACHUTE FACILITY (INDUSTRIAL AREA)

- WASH, DRY, REFURBISH, ASSEMBLE AND STORE RETRIEVED SRB PARACHUTES.

[Ref. 2/6-128]

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TESTIMONY OF THOMAS L. MOSER, DIRECTOR, ENGINEERING, JOHNSON SPACE CENTER

MR. MOSER: Mr. Chairman, members of the Commission, my organization at the Johnson Space Center provides technical support to the Shuttle Program office and to the orbiter systems in particular.

What I have done today is, I have constructed for you and for the Commission an overview of the design, development, and certification process, as Jess said, which is applicable across the board to all flights, and in particular to 51-L.

I hope that this presentation can give you an insight into the process by which the design and development is conducted, and will also give you a feel for the wealth of information that exists in the program, which I think you would want to pursue in more depth. Next chart, please.

(Viewgraph.) [Ref. 2/6-129]

MR. MOSER: I would like to talk to you briefly about the requirements and give you a feel for how they are established, the reviews which are conducted during this requirement and design process, the verification which demonstrates the capability by test and analysis, proves the design. The safety process,

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which I think is very important that you understand, is independent of the program. It is independent of the technical organization that does an independent assessment and audit. And then give you an overview of the external committees which have looked over our shoulders.

The next chart, please.

(Viewgraph.) [Ref. 2/6-130]

MR. MOSER: And the next chart, please.

(Viewgraph.) [Ref. 2/6-131]

MR. MOSER: Now we are on an overview of how the process evolves from the definition phases which essentially establish the Level 1 requirements that Mr. Moore controls, the technology which was developed in parallel to that. For example, this is where the work was done on the thermal protection system, that is, in establishing the advance capability and the enabling technology for the Shuttle Program. There was not a lot of enabling technology developed for this program. It was pretty much on the shelf.

The design and development process is the big phase in the program which established the detailed requirements of the individual elements and the individual systems. I will talk a little bit more about that later on. The ground test program then establishes through ground testing and analysis that the design as

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established meets the requirements that have evolved over the program.

The flight test program then provides a verification that those ground tests are in fact adequate to meet all the requirements, and then the orbital flight tests during the early phases did that very thing. The next chart, please.

(Viewgraph.) [Ref. 2/6-132]

MR. MOSER: All of these requirements, for the Commission's information, are delineated at the very top level, and are traceable all the way down through the various levels through the different elements that we have talked about today, the orbiter, the external tank, the solid rocket booster, the engines, the launch to landing site facilities. It is then—it goes down into the next level of detail, into the subsystems, for instance, the hydraulic system in the orbiter, the electrical power system in the orbiter.

Those requirements are very, very well delineated and documented in a series of documents by each one of the projects. Next chart, please.

(Viewgraph.) [Ref. 2/6-133]

MR. MOSER: In addition to those general requirements, there are specifications which go down through the same level of all of the flight elements and

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including the support equipment for the program. These specifications not only address the interface specification between the various elements, for instance, the interface hardware between the orbiter and external tank, as an example.

In addition to that, the specification for the detailed subsystems are also included in the specifications. Next chart, please.

(Viewgraph.) [Ref. 2/6-134]

MR. MOSER: There is a series of documents which are maintained and controlled at the three different levels of the program which establish the baseline. This is an active system. Any time changes are made in the system for any reason, those documents are kept up to date. The center series of documents which is in your handout are the technical requirements. Complementing that are the NASA management requirements and also the resource requirements which ensure the program meets its requirements.

The next thing in this overview and generic presentation, and I am going to go through this, Mr. Chairman, very quickly in response to your request to try and keep it applicable to 51-L so that you can see what is available, and there are a few things that I

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would recommend that you pursue in more detail.

(Viewgraph.) [Ref. 2/6-135]

MR. MOSER: The next chart, please.

(Viewgraph.) [Ref. 2/6-136]

MR. MOSER: I think it is important to emphasize that, as the time phasing chart indicated to you, that the initial requirements are established and the detailed requirements are confirmed. It is set into place as the design evolves. The chart that is on the monitor at this time shows how the engineering organization, both at NASA and the contractors, are establishing the details, and then they are provided to the program manager at various milestones throughout the program.

These milestones are identified along the lower portion of that chart. Their acronyms—let me just tell you in a few words what they are. The program requirements review are established

early in the program. That evolves all the way down to detailed design reviews which are baselined in the program somewhat time phase lagging as the technical community establishes those, but that is what is documented and established in the documents that I mentioned to you previously. The next chart, please.

(Viewgraph). [Ref. 2/6-137]

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The next three charts delineate exactly what those reviews consist of, who chairs them, how they are approved, how they are modified, and what program elements are involved.

Let me have the next chart, please.

(Viewgraph.) [Ref. 2/6-138]

MR. MOSER: And the next.

(Viewgraph.) [Ref. 2/6-139]

MR. MOSER: These are just definitions of those major review milestones.

The next, please.

(Viewgraph.) [Ref. 2/6-140]

MR. MOSER: Now, once the design is established, the next process is to verify that that design does in fact meet the requirements, and also, Mr. Chairman, to establish what the capability of that system is, and I think on any one of the systems that you have talked about today there is a wealth of information and long presentations which should be made to you establishing how those capabilities have been established based on the things which are delineated on this chart, namely, the ground testing, the analysis, the checkout and the flight demonstration.

The next chart, please.

(Viewgraph.) [Ref. 2/6-141]

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MR. MOSER: Comparable to the requirements in the establishment of all of those that they in fact do meet the various levels of requirements from Level I all the way down to Level III, there is a well-documented path which is traceable for the certification of each one of the elements. Here we have not only taken the elements and the subsystems, but we have cross-correlated, if you will, each one of those systems with the environments to which it must be proven to work in, and that is shown on the integrated system verification.

The next chart, please.

(Viewgraph.) [Ref. 2/6-142]

MR. MOSER: As the verification is established, each one of the elements focuses on those things which affect its design and affects the design of the total system, for instance, the loads, the thermal, the acoustics, the vibration, etc. This is done in a total system sense and provided to each one of the elements. The way in which all of these loads and environments are combined is unique with each one of the elements, and that is probably a half-a-day presentation to this Commission on any one so that you can adequately understand it.

Next chart, please.

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(Viewgraph.) [Ref. 2/6-143]

MR. MOSER: And the next?

(Viewgraph.) [Ref. 2/6-144]

MR. MOSER: It is important to highlight on any one of the specific components how it interfaces with the other systems, how it is traced through a total verification logic from the initial flight requirements, design requirements to the environments in which it must live its

particular mass properties and so forth to establish the design loads, the design conditions, and the tests which verify that capability.

Once those things are done on the ground, the important thing to recognize is that whole process then is verified with flight data from the test program. This was constituted primarily with the first four flights of the orbiter system and correlated back with the analysis. There were a few surprises during that program.

MR. SUTTER: This process was used in the design and development of the basic program?

MR. MOSER: Yes, sir.

MR. SUTTER: When you got to these weight-saving programs and what not, did you use as complete a process when you made those changes also?

MR. MOSER: The answer to that is yes, sir.

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Each one of those elements that had significant changes so that it would affect the conditions or the loads or the environment to which it was designed was re-analyzed and gone through the same process, that is correct, sir.

MR. SUTTER: Thank you.

MR. MOSER: The next chart, please.

(Viewgraph.) [Ref. 2/6-145]

MR. MOSER: And the next. That just shows the verification process from the flight data back to the design.

The next chart, please.

(Viewgraph.) [Ref. 2/6-146]

MR. MOSER: Independent of this total engineering task that I have just walked you through very quickly is another process which goes on independently of that organization, independently of the program office, and that is the safety operations. They do an assessment of the design from the very beginning. They participate in all of the designs, the design reviews, and the certification reviews. They, in concert with the technical organizations, sign off on the adequacy of each one of the systems and subsystems in the program, that it in fact does meet its design requirements.

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In addition to that, this organization does a complementary set of analyses which is highlighted in a box called complementary analysis. There are some acronyms there which are important. They do a failure modes and effect analysis of the system to understand what the impact of a failure of that system is. If there is something that comes out of that, it is identified in a critical items list, of which there are various categories of criticality of functions. A Category 1 means that loss of a component or a function would mean loss of the vehicle or loss of the crew. Category 2 means loss of the mission. And Category 3 means something like loss of data. Those are all documented along with the analysis on the criticality of all of the components in the program.

If I could have the next chart, please.

(Viewgraph.) [Ref. 2/6-147]

MR. MOSER: The next chart entitled "The External Review Committees" gives you a feel for, in addition, gives you a feel for the involvement by committees of technical capability and expertise external to the program which is reviewed in our total process. I have listed for you here a few of those committees. It is not complete, but we could provide you a complete review of all external review

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committees.

I would like to point out that the Aerospace Safety Advisory Panel which reports to the NASA Administrator and to Congress annually in a report, and also to the appropriate NASA managers, has participated in an extensive number of reviews of this program since its inception. Last year, for instance, they conducted 32 reviews of the entire process.

In addition to that, prior to our first flight, we had a certification assessment team which had eleven subteams in it which conducted a review over about an eight-month period in extensive detail which involved a lot of industry and academia personnel, which reviewed the NASA design and certification process.

CHAIRMAN ROGERS: Could I interrupt to ask on these reviews, do you know whether any of them relate directly or indirectly to the Challenger flight?

MR. MOSER: Yes, sir, some of them I believe have related directly, if you are speaking of the 51-L flight or Challenger in the previous flights.

CHAIRMAN ROGERS: I am speaking of 51-L.

MR. MOSER: Mr. Chairman, I do not know specifically that any one of these reviews related directly to that. I think that there were perhaps, and I would defer that to Mr. Moore.

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CHAIRMAN ROGERS: Or indirectly. Rather, if we have to, if our staff looks at these, we want to be able to somehow target the things that become important for our considerations and exclude the ones that obviously are totally unrelated, and I guess what I'm asking is would you be able to find out the ones that could directly or indirectly relate to the Challenger accident?

MR. MOSER: Yes, sir, I will find that out for you and provide that to you.

MR. ACHESON: Are these reviews made of specific anomalies that arise in the program, or are they reviews also of questions that people raise from time to time?

MR. MOSER: They are both of those, sir. Some of them are reviews in response to particular anomalies. Others are part of the normal process, flight readiness reviews, design reviews, certification testing, etc. I would say the majority of them are in response to the normal process, but any time we have an anomaly, many of these committees have participated in the reviews of those anomalous conditions with us.

MR. ACHESON: Have there been reviews of solid rocket boosters on any of the Shuttle flights, or in assembly and testing generally?

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MR. MOSER: Sir, I would like to refer that to that program, to that project office, if I may, and we will get that answer for you.

Listed below in not as much detail because I did not have time to prepare that for you, are other committees which have participated in an oversight or assessment, some of which are ongoing at this time, Mr. Chairman.

CHAIRMAN ROGERS: Are the records maintained here in Washington, or would they be in Kennedy or where? Both places?

MR. MOSER: The answer to that is both places. Some records are maintained here, sir, and some are at the field centers.

Thank you.

CHAIRMAN ROGERS: Thank you very much.

MR. MOORE: Mr. Chairman, for our final presentation this afternoon, I would like to talk about the flight preparation process with some specifics related to 51-L, and again give you a

little bit more feel about some of the specific aspects we go through for the flight and preparing it to get ready for launch, as well as talk in a little bit more detail about anomaly tracking and so forth.

To do that, I would like to introduce Mr.

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Richard Kohrs, Deputy Manager of the National STS Program Office at the Johnson Space Center.

THE CLERK: Do you swear that the testimony you will give before this Commission will be the truth, the whole truth, and nothing but the truth, so help you God?

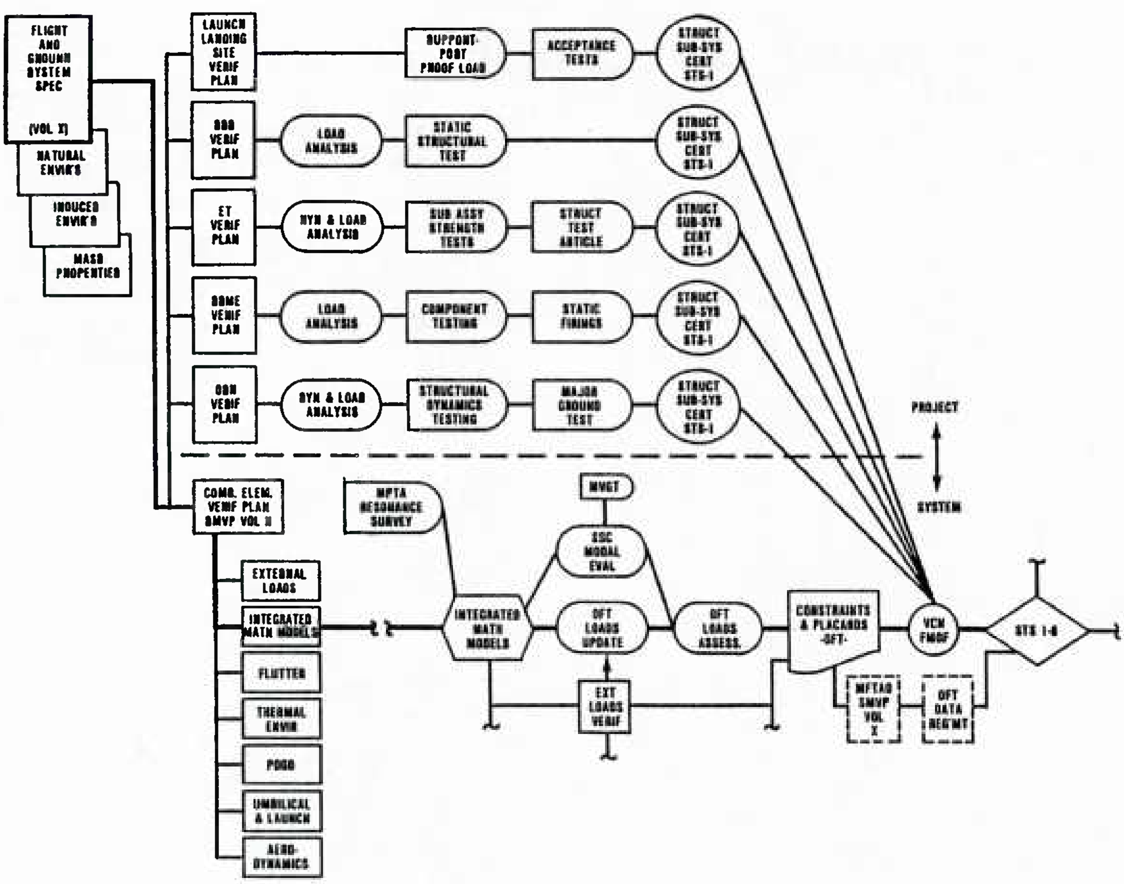
MR. KOHRS: I do.

NASA-S-86-00310

REQUIREMENTS

[Ref. 2/6-129]

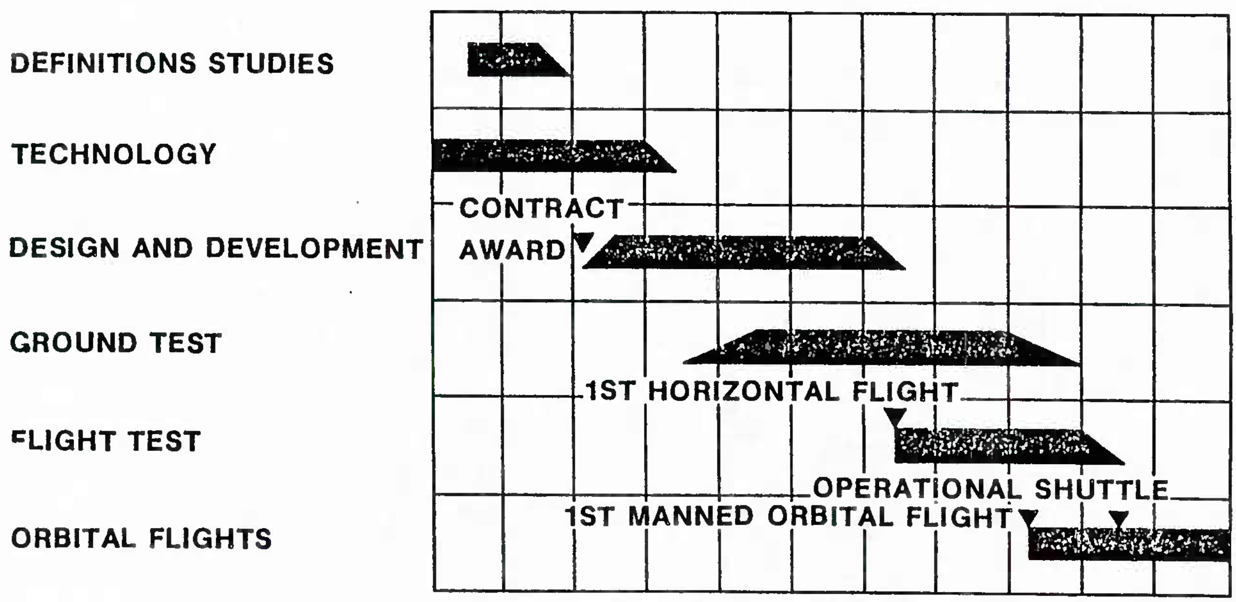
SYSTEMS INTEGRATION VE...FICATION LOGIC NETWORK STRUCTURES



[Ref. 2/6-130]

NASA-S-86-00297

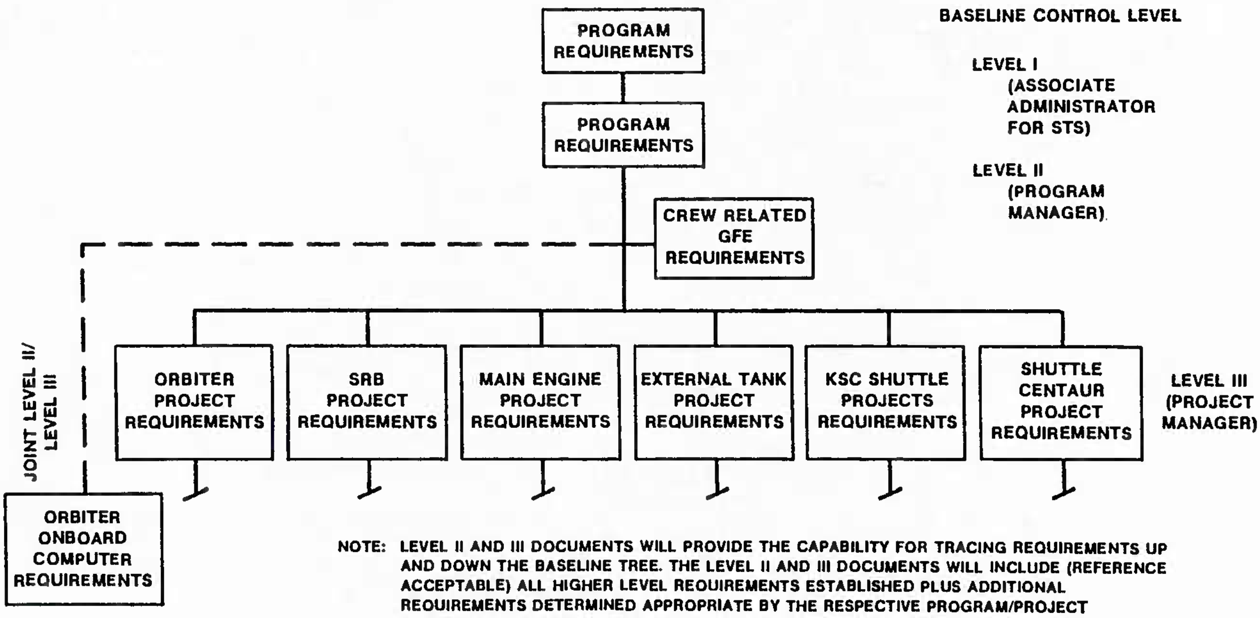
SPACE SHUTTLE PLANNING SCHEDULE



[Ref. 2/6-131]

NASA-S-86-00301

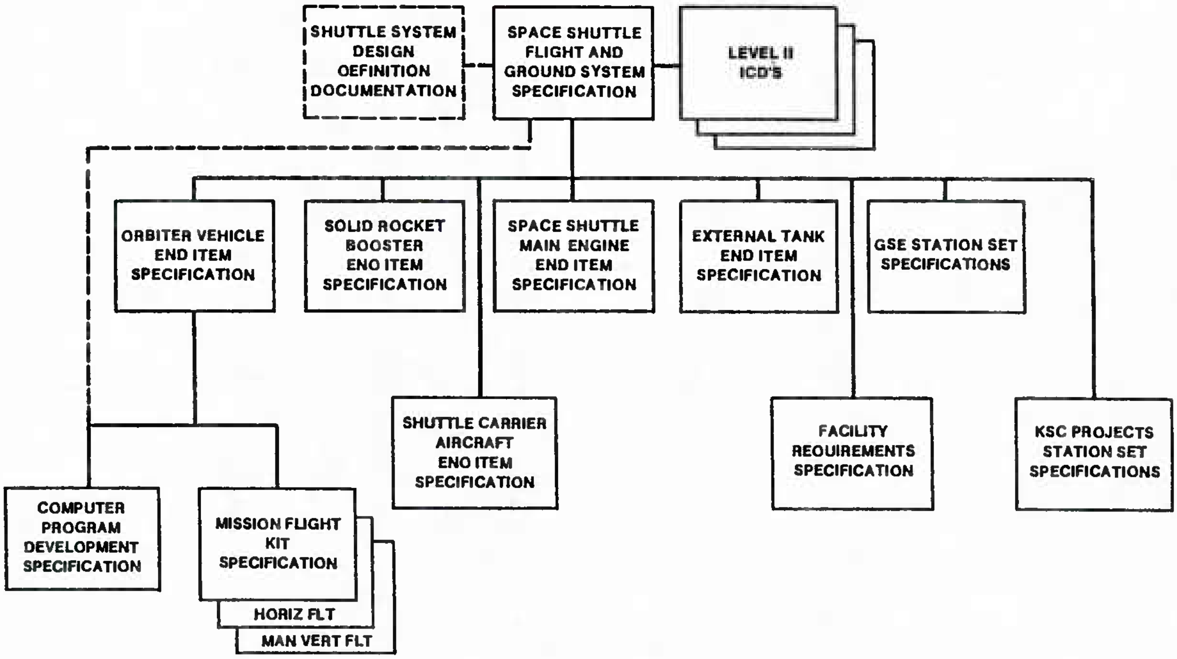
BASELINE DOCUMENTATION RELATIONSHIPS



NOTE: LEVEL II AND III DOCUMENTS WILL PROVIDE THE CAPABILITY FOR TRACING REQUIREMENTS UP AND DOWN THE BASELINE TREE. THE LEVEL II AND III DOCUMENTS WILL INCLUDE (REFERENCE ACCEPTABLE) ALL HIGHER LEVEL REQUIREMENTS ESTABLISHED PLUS ADDITIONAL REQUIREMENTS DETERMINED APPROPRIATE BY THE RESPECTIVE PROGRAM/PROJECT MANAGERS.

[Ref. 2/6-132]

SPECIFICATION TREE



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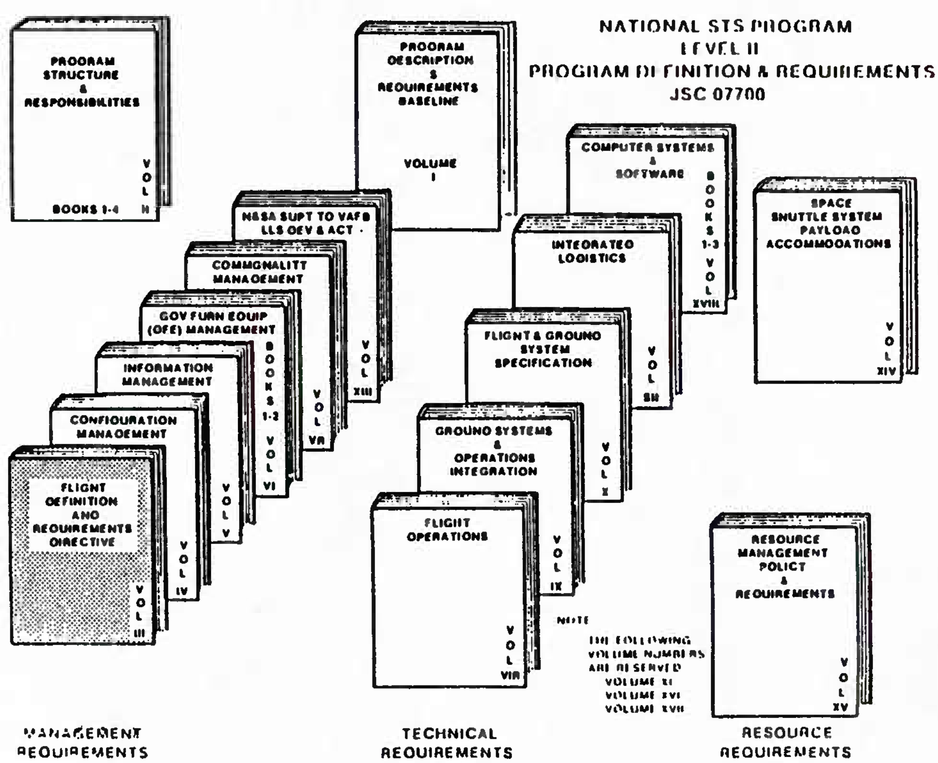


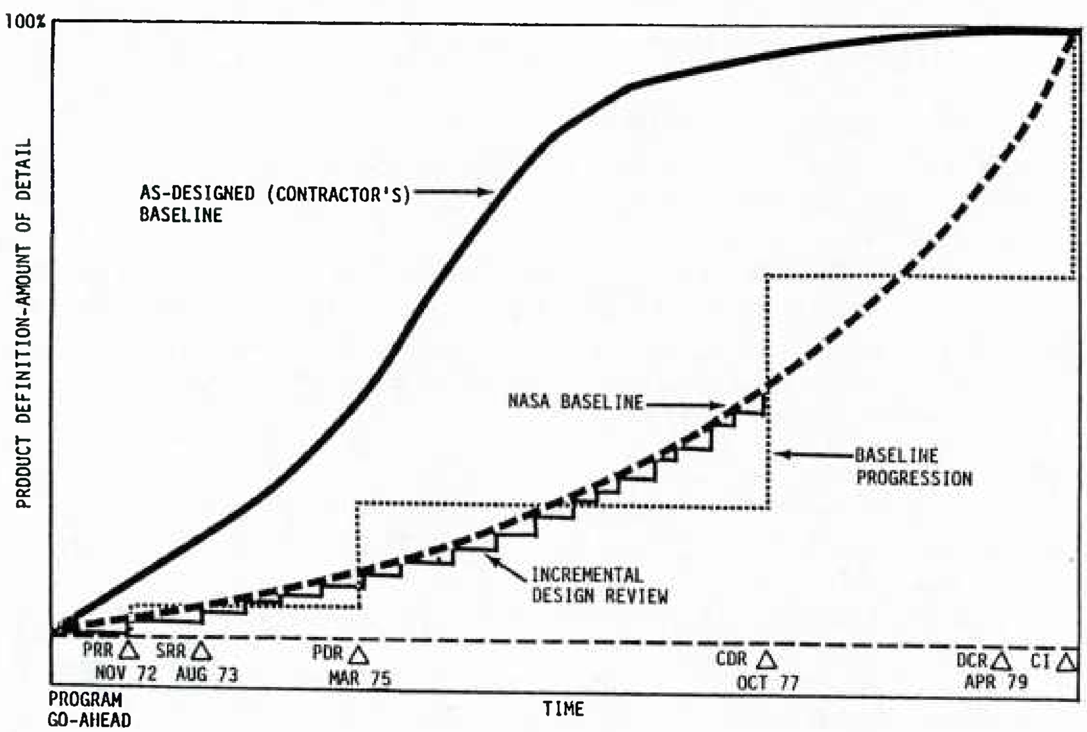
[Ref. 2/6-134]

NASA-S-86-00308

REVIEWS FOR REQUIREMENTS, DESIGN, CERTIFICATION, AND CONFIGURATION

[Ref. 2/6-135]

SPACE SHUTTLE PROGRAM BASELINES



[Ref. 2/6-136]

NASA-S-86-00293

SPACE SHUTTLE BASELINE REVIEWS

- PROGRAM REQUIREMENTS REVIEW
  - ENCOMPASSES ALL SHUTTLE PROGRAM ELEMENTS
  - CHAIRED BY THE SPACE SHUTTLE PROGRAM MANAGER
  - UPDATE PROGRAM REQUIREMENTS
  - EVALUATE MANAGEMENT TECHNIQUES, PROCEDURES, AGREEMENTS, ETC
  - EVALUATE CONFIGURATION MANAGEMENT PROCEDURES AND FORMATS
- SYSTEM REQUIREMENTS REVIEW
  - ENCOMPASSES ALL SHUTTLE PROGRAM ELEMENTS
  - CHAIRED BY THE SPACE SHUTTLE PROGRAM MANAGER
  - UPDATE THE PROGRAM AND SYSTEM REQUIREMENTS
  - DOCUMENT INTO LEVEL II BASELINE AND PLACED UNDER CONFIGURATION CHANGE CONTROL
  - ICD RESPONSIBILITIES DEFINED AND DOCUMENTED

[Ref. 2/6-137]

NASA-S-86-00294

SPACE SHUTTLE BASELINE REVIEWS

- PRELIMINARY/CRITICAL DESIGN REVIEWS
  - CHAIRED BY THE SPACE SHUTTLE PROGRAM MANAGER
  - CONCLUDES THE PROJECT ELEMENTS SERIES OF INDIVIDUAL PDR'S/CDR'S
  - END-TO-END REVIEW OF SUB-SYSTEMS ACROSS ALL PROJECT ELEMENTS
  - PRIMARY ATTENTION TO SYSTEM LEVEL OPERATIONS AND FUNCTIONS
  - EMPHASIS ON THE REVIEW BY FLIGHT CREW, FLIGHT OPERATIONS, DATA SYSTEMS, LAUNCH OPERATIONS, AND RECOVERY OPERATIONS ORGANIZATIONS
- DESIGN CERTIFICATION REVIEW
  - CHAIRED BY THE ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT
  - CERTIFY THE DESIGN AND EVALUATE RESULTS OF THE VERIFICATION PLANNING, TESTING, AND ANALYSIS
  - REVIEW RESULTS OF OPERATION AND PERFORMANCE ANALYSIS FOR THE TOTAL SYSTEM
  - OTHER DCR'S HELD FOR APPROACH AND LANDING TEST, FIRST MANNED ORBITAL FLIGHT, MAIN PROPULSION TEST, AND MAJOR DESIGN CHANGES

[Ref. 2/6-138]

NASA-S-86-00295

SPACE SHUTTLE BASELINE REVIEWS

- CONFIGURATION INSPECTION
  - CONDUCTED BY THE RESPECTIVE PROJECT ELEMENTS
  - FOR FIRST FLIGHT ARTICLE, GSE, AND FLIGHT AND GROUND SYSTEM SOFTWARE
  - COMPARES "AS-BUILT" TO "AS-DESIGNED"
  - CONDUCTED INCREMENTALLY
- TEST READINESS REVIEWS
  - CONDUCTED PRIOR TO INTEGRATED (COMBINED ELEMENT) GROUND TESTS

    IE: 1/4 SCALE GROUND VIBRATION TEST, MPT, MVGVT, INTEGRATED AVIONICS TEST, AND FRF
  - CHAIRED BY THE SPACE SHUTTLE PROGRAM MANAGER
  - VERIFIES TEST ARTICLE, FACILITIES, SUPPORT EQUIPMENT, INSTRUMENTATION, AND TEST PROCEDURES COMPLY WITH REQUIRMENTS

[Ref. 2/6-139]

NASA-S-86-00311

VERIFICATION/CERTIFICATION

[Ref. 2/6-140]

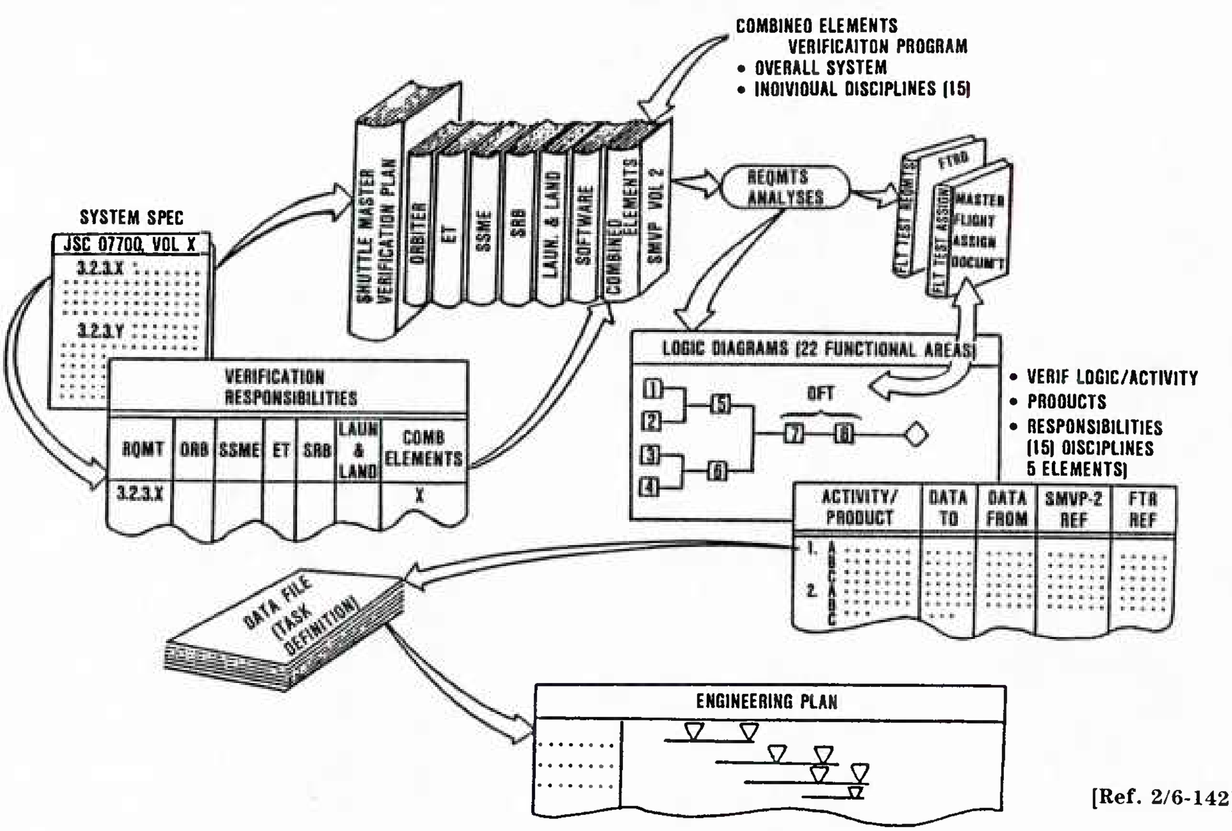
SPACE SHUTTLE SYSTEM VERIFICATION

- SYSTEM VERIFICATION ENCOMPASSES
  - CERTIFICATION/QUALIFICATION
  - DEVELOPMENT TESTING
  - ACCEPTANCE TESTING
  - PREFLIGHT CHECKOUT
  - FLIGHT DEMONSTRATION
  - ANALYSES
- VERIFICATION METHODS
  - ANALYSIS
  - TEST

[Ref. 2/6-141]

NASA-S-86-00291

INTEGRATED SYSTEM VERIFICATION



[Ref. 2/6-142]

NASA-S-86-00302

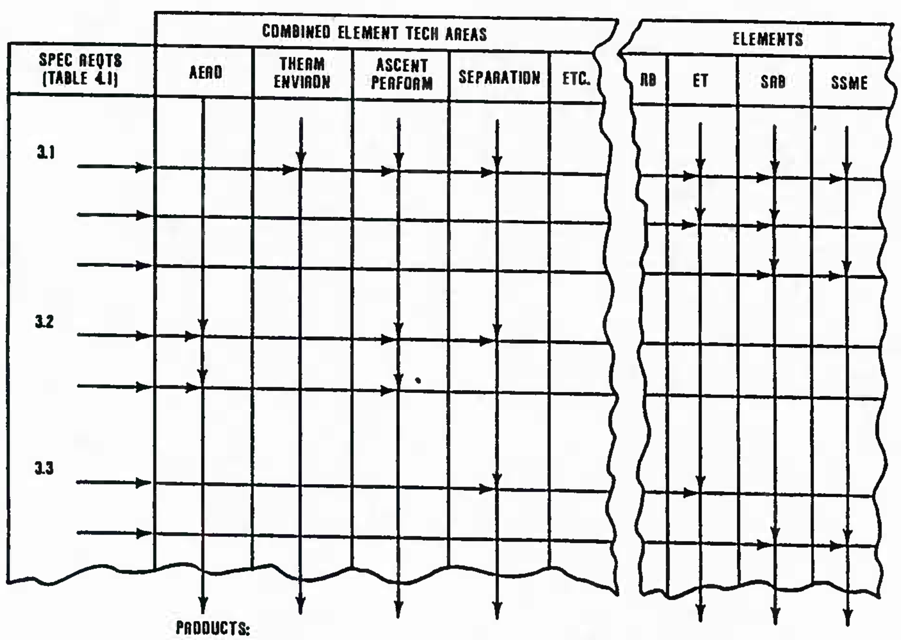
SHUTTLE VERIFICATION SYSTEM DISCIPLINES

- AERODYNAMICS
- ASCENT FLIGHT PERFORMANCE
- ASCENT GUIDANCE, NAVIGATION, AND CONTROL
- INTEGRATED VEHICLE MATH MODEL
- ACOUSTICS ENVIRONMENT
- MAIN PROPULSION
- UMBILICAL AND SEPARATION
- COMMUNICATIONS AND TRACKING
- THERMAL ENVIRONMENT
- SEPARATION
- EXTERNAL LOADS
- POGO DYNAMICS
- FLUTTER
- AVIONICS AND SOFTWARE
- HYDRAULICS

[Ref. 2/6-143]

NASA-S-86-00289

VERIFICATION INTEGRATION
(COMBINED ELEMENT)



PRODUCTS:

SMVP-II DISCIPLINE SUMMARY PLANS

- SYSTEM VERIFICATION NETWORKS
- VERIF RESPONSIBILITY

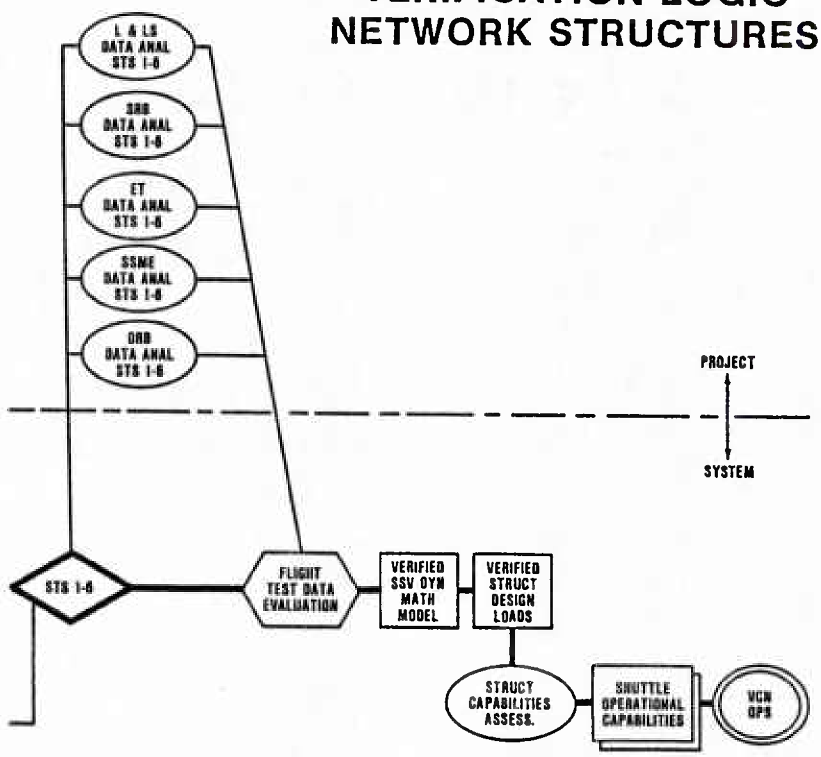
ELEMENT VERIF PLANS

- SMVP-III (ORB)
- SMVP-IV (SRB)
- SMVP-V (ET)
- SMVP-VI (SSME)
- SMVP-VIII (L&LS)

[Ref. 2/6-144]

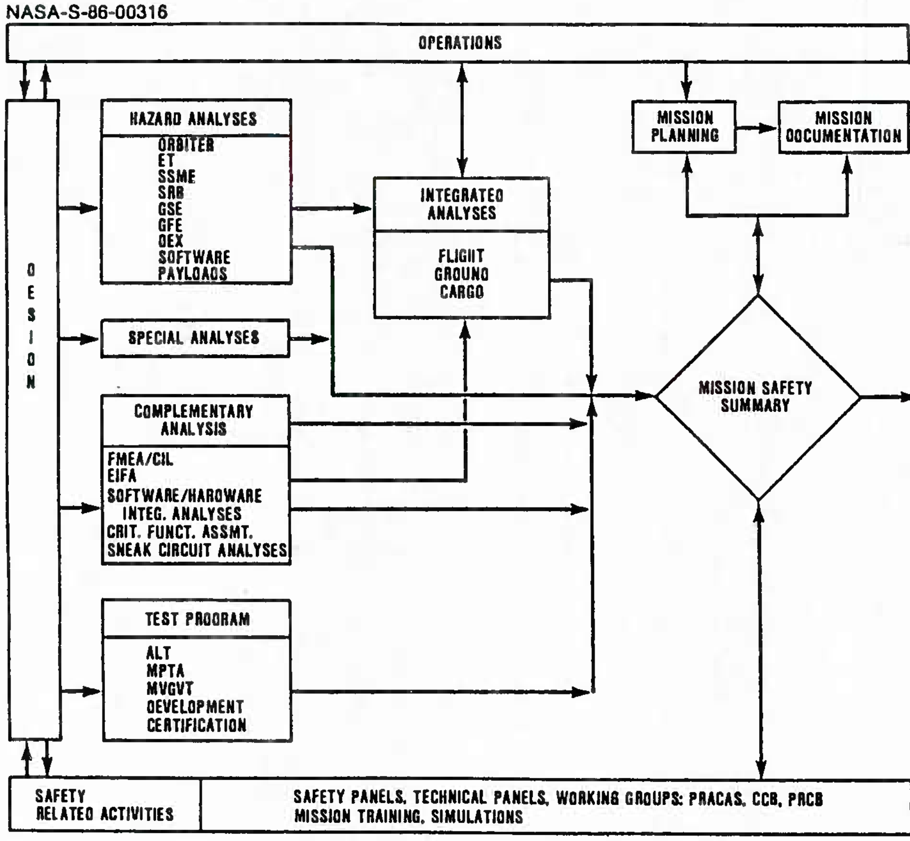
NASA-S-86-00288

SYSTEMS INTEGRATION VERIFICATION LOGIC NETWORK STRUCTURES



[Ref. 2/6-145]

NASA-S-86-00316



[Ref. 2/6-146]

SAFETY REVIEWS OF THE STS BY EXTERNAL COMMITTEES

COMMITTEE	PURPOSE	DATES	PRODUCTS
AEROSPACE SAFETY ADVISORY PANEL (10 MEMBERS)	MULTIDISCIPLINE EXPERTISE TO ASSESS THE STS (SYSTEMS AND SUB-SYSTEMS) FOR SAFETY	1985 - 32 REVIEWS 1984 - 24 REVIEWS 1983 - 35 REVIEWS 1982 - 20 REVIEWS 1981 - TBD 1980 - 14 REVIEWS 1967	• ANNUAL REPORTS TO NASA ADMIN. AND CONGRESS • REPORTS TO APPROPRIATE NASA PROGRAM AND PROJECT MANAGERS
STS VERIFICATION/ CERTIFICATION ASSESSMENT TEAM (11 SUBTEAMS)	ASSESS THE ADEQUACY OF THE VERIFICATION/ CERTIFICATION OF THE STS FOR STS-1	NOV. 1979 TO JUNE 1980	• REPORT TO THE NASA DEPUTY ADMINISTRATOR

EXAMPLES OF OTHER SPECIAL COMMITTEES ON:

- SPACE SHUTTLE MAIN ENGINES
- FLIGHT CONTROL AND HYDRAULIC SYSTEMS
- ORBITER BRAKES
- ORBITER STRUCTURAL INTEGRITY
- ORBITER HYDRAULIC SYSTEM CLEANLINESS
- DESIGN ASSESSMENT TEAM

REPORTS TO PROGRAM AND PROJECT MANAGERS AND/OR THE ADMINISTRATOR

[Ref. 2/6-147]

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TESTIMONY OF RICHARD H. KOHRS, DEPUTY MANAGER, NATIONAL SPACE TRANSPORTATION SYSTEMS PROGRAM, JOHNSON SPACE CENTER

MR. KOHRS: Mr. Chairman and members of the Commission, Tom Moser covered the design, development and verification, and that continues throughout the program. If we have new changes to the vehicle or new changes to the process, then that process continues.

What I am going to describe today as an overlay to that is the flight preparation process, and the flight preparation process is really typical of any mission. It varies a little bit depending on the cargo, but basically it goes over a year and a half time period. And then I am going to conclude with showing you the FRR process, at least on an overview scale of what we used during this 51-L mission, and how that was conducted and handled.

If I could have the next chart.

(Viewgraph.) [Ref. 2/6-148]

MR. KOHRS: It basically shows at the top—and I will only deal with the top bullet—it basically shows that the flight manifest, which is really our mission assignments—is determined by the Level I Board here in Washington, and it is implemented by the Level II system. The way we implement that is through a

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document called the FDRD document, which is a Flight Definition and Requirements Document.

In the next chart we will show you a little bit of detail.

(Viewgraph.) [Ref. 2/6-149]

MR. KOHRS: And I will point out here that that is this document over here, which is Volume 3. It is part of our overall configuration, control and management system of how we track requirements.

The next chart—

(Viewgraph.) [Ref. 2/6-150]

MR. KOHRS:—will show you the details of what this Flight Definition Requirements Document does. It essentially has three phases. The first phase is to document basically the next year's flights in terms of the specific flight requirements for that mission, specific characteristics like throttle setting, the number of crewmen, the payload, the cargo, etc.

The second part of that document looks beyond the first year into the outyears, and it basically sets our schedules and our flight manifests for the downstream activity. It will go as far as probably today, we would go to 1989, 1990, and define the missions we have planned, when they are scheduled, etc. This allows the projects to plan their flight deliveries of

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their tanks, rockets, etc.

In addition to that, on the bottom of the chart, it is also used as a general mission planning document, a general document for logistics scheduling, etc. This document is controlled through our PRCB system, and if you show the next chart—

(Viewgraph.) [Ref. 2/6-151]

MR. KOHRS: I apologize. I think the one in your handout is not too clear. On the top of the chart, it shows a Level I organization which is here in Washington, and in the middle of the chart it shows in the top box in the middle what we call the Level II PRCB organization, which is chaired by the NSTS program manager, who is Arnie Aldrich. And the way we operate on this, mission by mission, is we meet daily every noon in Houston by telecon. At those meetings we deal with the activities that are going on with the flight vehicles at the Cape and eventually with the flight vehicles at Vandenberg. We deal with all changes to the vehicle. We approve both the hardware and software, and to the processing. We approve all waivers, and we approve all changes to things like critical items lists, failure modes and effects analyses, and any other waivers.

(Viewgraph.) [Ref. 2/6-152]

MR. KOHRS: The next chart in this year-and-a-

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half process of a flight to flight, we have developed some other program milestones, and we have adopted the terminology called freeze points. Freeze points are just a term, but we essentially say for the system to flow in a logical order, we have got to set baselines that are the lower tier of what is in our Flight Definition Requirements Document.

This chart is a busy chart, but briefly, it lists in the first column what we are freezing by this timeframe. If you look across the top, we freeze things at 66 weeks, which basically freezes the cargo. At 33 weeks we have something called a cargo integration review, the next step of freezing that mission definition. At L minus 22 weeks, we go into the crew compartment and freeze things that are added to the crew compartment like student experiments, fill up the lockers, etc.

The second one from the right is a major milestone, and that terminology up there stands for the OPF, which is the Operator Processor Facility roll-in, minus four weeks. And what we have learned over the years is to get a logical modification to the flight vehicles, we need to define our engineering and our changes that the Cape needs to accomplish, and we shoot to do that at four weeks before roll-in, which normally

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is on the order of two to three months before launch.

And finally, at the L minus ten week timeframe, we once again have a review of the ascent design. Primarily it is to reflect any late changes into maybe cargo that we have loaded on that might change our performance. We always try to launch, optimize launch probability in terms of our upper winds in our atmosphere. Normally, and I would say in our 24 flights, at that L minus ten review, we probably have only changed the ascent design maybe four or five times. And a very minor change is normally a small update to the ascent trajectory.

The last three lines on the bottom list the OPR, which is the Office of Primary Responsibility for the Level II. Those offices are all within the Level II organization. That is, the code there is just different organizations. The record that we document this is listed in the second column across the bottom, and it shows how we keep track. We update the FDRD. We update the drawings.

This nomenclature of MECSLSI and MESELSI and CCCD are really drawings that reflect the configuration of the cargo below the payload bay, cargo above the payload bay and the cargo that is in the crew cabin. In addition to that, we also baseline for each vehicle, for

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each flow. The drawings for that particular flow, for both the orbiter, the ET, SRB and external tank. Those drawings are maintained and controlled, and that repository is at Kennedy.

DR. RIDE: How closely have you been able to stick to these launch minus weeks in, say, the flights over the past year? If you were to look at one particular flight, take 61-C or 51-L?

MR. KOHRS: As we get closer to launch, of course, we stick closer to them, but I think back in the CIR timeframe, because of some of our remanifesting and some of our launch abort type things we have had here, we have had to go back and readjust and do what we call delta reviews because we had to change our manifest.

DR. RIDE: I know we have been doing a lot of remanifesting lately, and compressing these schedules quite a bit, and getting things like cargo integration reviews actually very close to flight.

MR. KOHRS: That's right, Sally, but in almost all of those cases it is due to some programmatic change that in a lot of cases is beyond the system's control.

For example, on the TDRS flight that we were going to fly last January, we had actually rolled out to the pad and were a week before launch, and the system decided that TDRS needed to roll back for some

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modifications. That, which was two weeks before launch, upset our process, if you will, and created a series of delta reviews that we had to perform in order to get back into our normal how do we do business.

(Viewgraph.) [Ref. 2/6-153]

MR. KOHRS: The next chart—and I have a series of charts—then get into the flight readiness, and this first chart is an overview, and I will just touch on a few points, and then in the subsequent charts I will go into a little bit more detail of what we typically do at one of our major centers, and an example I will use will be the Marshall Space Flight Center. But basically, prior to every flight, and normally it is L minus one week, launch minus one week, the Associate Administrator for Space Flight, in this case, Jess Moore, conducts a detailed flight readiness review with all the Shuttle elements, the flight operations, and the cargo managers and their contractors. Each of these project managers or element managers that I have listed here with the basic content is—I won't read that to you. I will get into a little bit more detail on subsequent charts, and then at that review there is normally a series of open action items. Those open action items are documented by the Board, with the requirement that all open actions be closed out before or at the L minus

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one day review which Jess and Arnie talked about earlier, which is our launch minus one day mission management team.

At that review we formally close those actions, they are signed off. Each project and element manager again states his readiness for flight. That readiness is a matter of record in our documentation, and then the commit to flight again is reaffirmed at the L minus one.

CHAIRMAN ROGERS: And all of this was done, I assume, prior to the Challenger accident?

MR. KOHRS: Yes, sir. This process is typical of our 25 launch attempts. We also do the same type of process, as Tom mentioned, if we have a major test. We call it a test readiness

review. We are going to do a cluster engine firing of three main engines which are scheduled in the next month or so, but that also get into this review process and get the same level of detail and documentation.

The next couple of charts or five charts show typically how Marshall—and this is typical of Marshall and Johnson and Kennedy—

(Viewgraph.) [Ref. 2/6-154]

MR. KOHRS:—establish their flight readiness review process, and the first chart, which is

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up there, shows on the bottom that the prime contractors, element contractors, have their own internal review. The Shuttle element project which is the first project that reports to Dr. Lucas, has their review—I'm sorry. Each Shuttle element project, ET SRB and SSME, has their review. That goes to the Shuttle project office, and then goes to the center director for his review.

It comes to the Level II office for a pre-review and then goes to Jess Moore as an FRR review and culminates at the L minus one day review.

(Viewgraph.) [Ref. 2/6-155]

MR. KOHRS: The next chart shows for each of those who chairs the FRR, and the thing to note is on the first one which is the contractor, it is chaired by a level of management that is at least one higher level than the project manager for that system. The Shuttle element projects are chaired by the project manager for the three elements, and the projects office chairs the FRR, which leads up to the center director review. I have included on the next chart—

(Viewgraph.) [Ref. 2/6-156]

MR. KOHRS: —the typical membership, which is really the main line organization of the Marshall Space Flight Center.

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(Viewgraph.) [Ref. 2/6-157]

MR. KOHRS: The next two charts show in a little bit more detail the purpose and the content of the review, basically, the compatibility of the mission requirements with the hardware and the experience base we have had, the experience base for the engines, tank, orbiter, etc.

The hardware pedigree is determined, and I have listed there. It deals with the changes that have occurred, any waivers and deviations, any—MR there is material review actions on materials, and also limited life items. Anomalies—and Jud talked earlier about, sometimes are called observations—are also reviewed and dispositioned and documented rationale as to why that anomaly close-out is acceptable to allow us to fly on the next flight.

Also, on the next chart—

(Viewgraph.) [Ref. 2/6-158]

MR. KOHRS:—is a safety and RQA review which Tom Moser mentioned is basically independent of the main line program, but it is reported to the center director institutionally, and then review of all unplanned open work that still remains before launch.

And then it goes over the support operations at the Kennedy Center, and will go over the operations

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at the Vandenberg Center when it is operational, and then each element completes what we call a certificate of flightworthiness.

Now, the level of detail of the certificate of flightworthiness goes all the way down to the subsystem managers, as Tom Moser mentioned. It is that we will have in the backup documenta-

tion the statement that the OMS subsystem manager or the APU subsystem manager on the SRB, he and his contractor counterpart have attested that this vehicle is ready for flight.

That culminates up to the project managers, and then on the FRR day, in this next chart—

(Viewgraph.) [Ref. 2/6-159]

MR. KOHRS:—is the end product of that review, and I won't deal with the signatures on there. But basically what it says is that each contractor, on the left hand column, and each NASA project manager signs this endorsement for flight. This particular page is signed on the bottom, signature of Arnie Aldrich, who is a Level II program manager, and after that review, Jess Moore then, who chairs this meeting, and when I show you the next chart—

(Viewgraph.) [Ref. 2/6-160]

MR. KOHRS:—conducts a verbal readiness poll of all of the contractors that are listed on the

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top of the page that are directly involved with the next launch, and this sheet happens to be the sheet from 51-L, where he has polled these contractors listed on the top, he has polled the payloads and their managers listed in the middle, and down below, the Department of Defense, the Aerospace Safety Advisory Panel. In this particular case they did not attend that 51-L. They normally do. Any consultants to the Administrator or to the Associate Administrator, Chief Engineer, data tracking, and finally, the center directors.

Based upon this poll being conducted, the signed certificate of flight readiness, any open actions are documented which are closed at the L minus one.

And the final chart I had is what Jess Moore signs to attest—

(Viewgraph.) [Ref. 2/6-161]

MR. KOHRS:—that this flight configuration, the procedures are ready for flight.

And this is an example. It is typically done for every mission and for every major test that we conduct.

CHAIRMAN ROGERS: Thank you.

Are these reviews that you speak of, do they result in a written report?

MR. KOHRS: Yes, sir, written minutes with

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written action items.

CHAIRMAN ROGERS: And that, of course, would be available?

MR. KOHRS: That is available. For the record, we can go back to any flight that we have had or any major test. That data is available the action items and the closeouts.

CHAIRMAN ROGERS: Have you ever fired any contractor or subcontractor for poor performance?

MR. KOHRS: Not to my knowledge.

Someone else may want to help me on that, but not to my knowledge.

MR. MOORE: We can get you that information, Mr. Chairman. I don't recall any recently.

CHAIRMAN ROGERS: Are you permitted to contractually? Do you have a provision in the contract that permits you to fire a contractor or subcontractor for poor performance?

MR. KOHRS: I believe we do, and then some of our contracts are incentive contracts, some of them are award fee contracts, and you have your mechanisms for dealing with any abnormal performance in that way also.

But to my knowledge, Jess, I don't recall anybody where I would use the word "fire" for poor performance.

CHAIRMAN ROGERS: Well, terminate maybe?

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MR. KOHRS: I will have to look for help on that.

CHAIRMAN ROGERS: I am just wondering, I think, whether you are pretty well locked in to contractors and subcontractors by the mere fact that they are in that position, or whether you have any option to terminate a contract if you find poor performance?

MR. KOHRS: Well, a you know, as I mentioned earlier, the Kennedy contracts were just recompeted into a single processing contractor, and just effective January 1 of this year, I think, Arnie, the eastern contract, major support contract was recompeted and awarded the first of January and actually put into implementation the first of January this year.

MR. ACHESON: But the law allows you termination for the convenience of the government?

MR. KOHRS: I think that is right.

DR. WHEELON: I notice in the example here of the checkoff sheets that you have Xeroxed, most of the signatures are dated 15 January, and a few are dated 23 January.

Do you have a mechanism to go back and make sure that these were still valid certifications on the 28th?

MR. KOHRS: Yes, sir. And the reason you

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don't see all of these filled out is, like Arnie said earlier, we do a lot of our meetings by telecon. The people where you see the signatures were at the meeting that day. Where it says endorsement attached, it was sent in through the mail system, and the record has all of those attached endorsements.

DR. WALKER: I had a question as to what occurs after a flight. You have a lot of data, and I presume there is some procedure for analyzing and evaluating that data?

MR. KOHRS: After each flight the anomalies are tracked. The anomalies, if they affect the next mission, automatically flow into this process for the next mission. Each project writes a flight status report or summary report for the previous mission, and that normally is documented, I would say in the average, within 30 to 40 days after the flight. Any hardware items that are removed from the vehicle go into another tracking system which the orbiter calls CAR, which is customer action request, and tracks the hardware that is removed and tracks its disposition, especially if it was involved with an anomaly closeout.

CHAIRMAN ROGERS: Any other questions?

(No response.)

CHAIRMAN ROGERS: Thank you very much.

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MR. MOORE: Mr. Chairman, that completes our planned briefings for today, to go through the process to give you some feeling of what we go through to get ready for a flight, and to talk to you a little bit about the activities that we are doing at NASA with respect to the Challenger accident. And as we said earlier, we will be happy to provide you any additional information that you need and support your Commission in any way you deem fit.

CHAIRMAN ROGERS: Thank you.

I compliment you and your associates. We appreciate it very much. We know that we gave you very short notice, and I think it has been a very worthwhile and effective presentation.

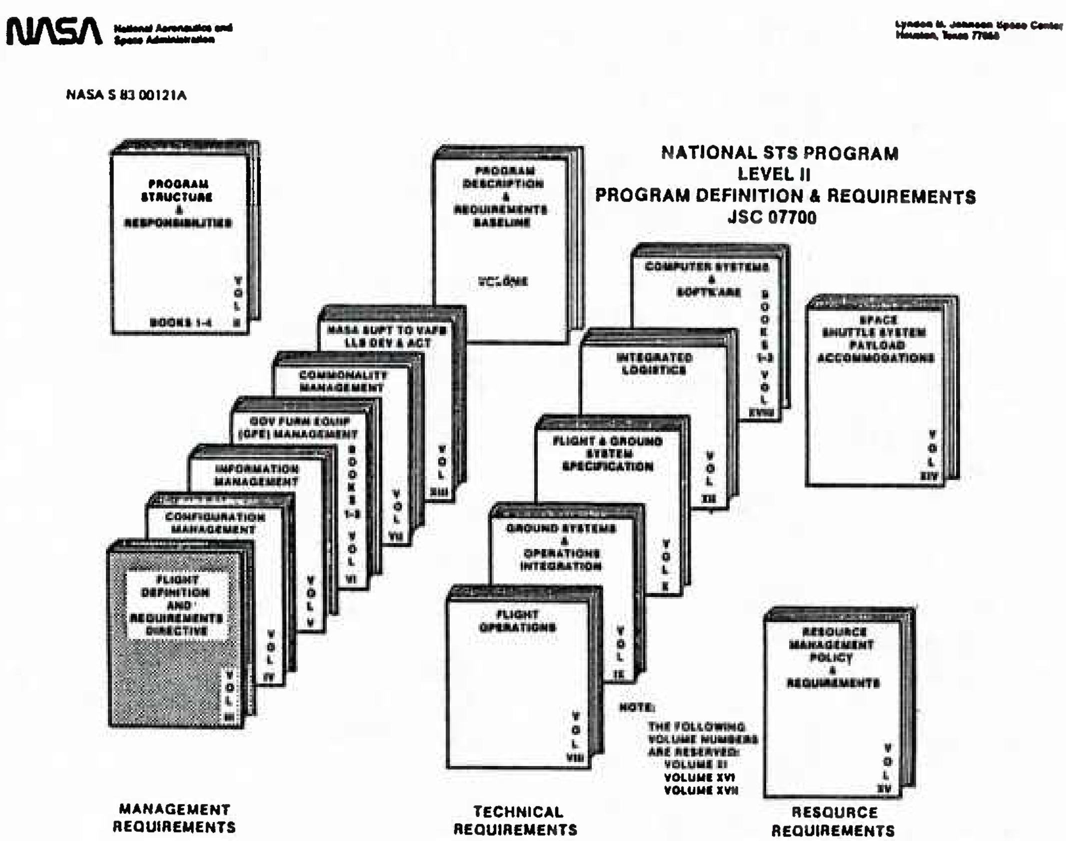
MR. MOORE: Thank you.

CHAIRMAN ROGERS: That's it for the day.

(Whereupon, at 4:25 o'clock p.m., the Commission recessed, to reconvene at 8:30 o'clock a.m, Friday, February 7, 1986.)

- FLIGHT MANIFEST DIRECTED BY LEVEL I AND IMPLEMENTED BY LEVEL II
- LEVEL II
  - CONTROLLING DOCUMENT IS FDRD - VOL III (FLIGHT DEFINITION AND REQUIREMENT DOCUMENT)
  - DAILY LEVEL II PRCB'S (PROGRAM REQUIREMENT CONTROL BOARD)
    - APPROVE CHANGES NECESSARY TO IMPLEMENT FDRD
    - APPROVE ALL CHANGES TO DELIVERED HARDWARE
    - APPROVE ALL ANOMALY CLOSEOUTS
    - APPROVE ALL WAIVERS TO DELIVERED HARDWARE
    - APPROVE ALL CHANGES TO CIL (CRITICAL ITEM LIST)
  - PROCESS CONTROLLED BY DESIGNATED REVIEWS (FREEZE POINTS) WHICH ARE KEYED TO MAJOR PROGRAM MILESTONES

[Ref. 2/6-148]



[Ref. 2/6-149]

NASA National Aeronautics and Space Administration

Lyndon B. Johnson Space Center Houston, Texas 77058

NASA-S-83-00107C

NATIONAL STS PROGRAM
THE FLIGHT DEFINITION AND REQUIREMENTS DIRECTIVE (FDRD)
(JSC 07700, VOLUME III)

- FOR NEAR TERM FLIGHTS, THIS DOCUMENT CONTAINS:
  - CRITICAL FLIGHT PLANNING PARAMETERS
  - HARDWARE AND SOFTWARE CONFIGURATION
  - PLANNED PAYLOADS AND MISSION KITS
  - SPECIAL EQUIPMENT AND INSTRUMENTATION
  - SUPPORTING MILESTONES AND SCHEDULES
- FOR ADVANCED PLANNING, THE FDRD CONTAINS:
  - FLIGHT RATES, ORBITERS AND LAUNCH DATES BASED ON CURRENT MANIFEST PLANNING
  - SIGNIFICANT VLS MILESTONES AND SCHEDULES
  - FACILITIES PLANNING TO ACHIEVE DESIRED FLIGHT RATES
  - SELECTED HARDWARE DELIVERIES AND ENHANCEMENTS
- THE FDRD IS USED BY ALL PROGRAM ELEMENTS AS THE SINGLE AUTHORITATIVE REQUIREMENTS DOCUMENT FOR:
  - MISSION PLANNING
  - FLIGHT DESIGN
  - CREW TRAINING
  - SYSTEM HARDWARE/SOFTWARE DELIVERIES
  - LOGISTICS SUPPORT
  - GROUND OPERATIONS AND TURNAROUND

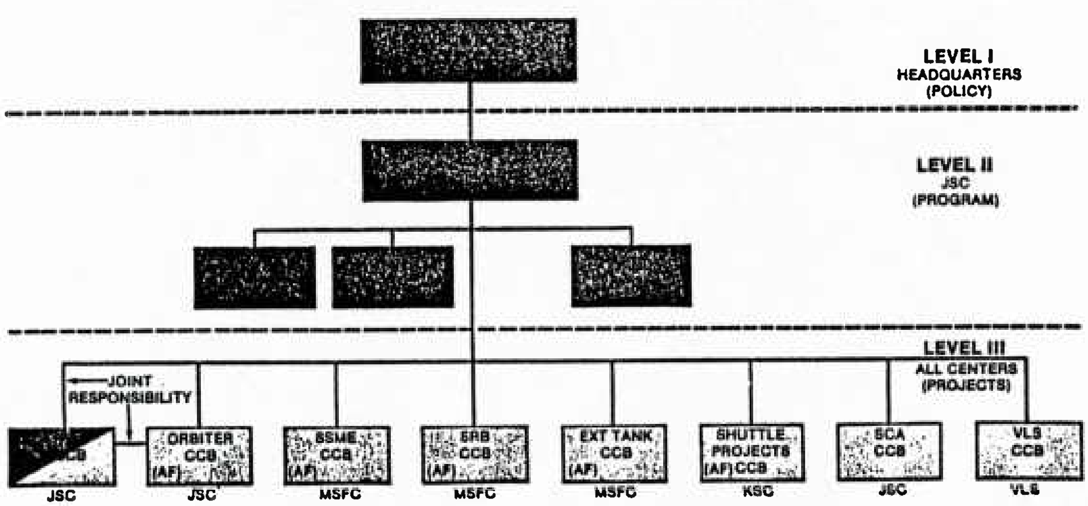
[Ref. 2/6-150]

NASA National Aeronautics and Space Administration

Lyndon B. Johnson Space Center Houston, Texas 77058

NASA-S-83-02125D

NATIONAL STS PROGRAM
CONFIGURATION CONTROL ORGANIZATION



(AF) - DENOTES USAF SPACE DIVISION MEMBERSHIP

[Ref. 2/6-151]

NASA-S-84-04323D SEPTEMBER 1985

FREEZE POINTS

TIME	L-66 WEEKS	L-33 WEEKS	L-22 WEEKS	OPF R/I-4 WKS	L-10 WKS
TITLE	CARGO	CIR	CREW ACTIVITIES/STOWAGE	LAUNCH SITE FLOW	ASCENT DESIGN
REQUIREMENTS FROZEN	• FLIGHT ELEMENT ALLOCATION • CARGO (EXCEPT GAS AND STUDENT EXPERIMENTS) • FLIGHT PREPARATION DATA • USERS COMMIT TO CARGO SCHEDULE MILESTONES	• CARGO ARRANGEMENT • INTEG HDW • CARGO SW RQMTS • FLT DESIGN RQMTS • GROUNDRULES AND CONSTRAINTS • MECSLSI • MESELSI • CCCD (CARGO SUPPORT) • ORBITER MODS	• CCCD REQUIREMENTS - NO. OF LOCKERS/ WEIGHT - STANDARD CREW EQUIPMENT • CREW ACTIVITY PLANNING • STUDENT EXP'S • GAS EXP'S	• FLIGHT ELEMENT MODS • FINAL CHECKOUT REQUIREMENTS • FLOW THROUGH OPF, VAB, PAD	• ASCENT TRAJECTORY CRITERIA & DESIGN • PERFORMANCE MARGINS & PROBABILITY TO SUPPORT I-LOADS UPDATE AT L-9 WEEKS
OPR	MISSION INTEGRATION OFFICE TM	MISSION INTEGRATION OFFICE TM	MISSION INTEGRATION OFFICE TM	MANAGEMENT INTEGRATION OFFICE TO	SYSTEM INTEGRATION OFFICE TE
RECORD	UPDATE FDRD PRCBD	MINUTES OF CIR + DN'S, SUBSEQUENT RELEASE OF MECSLSI, MESELSI AND CCCD	MINUTES OF MICB REVIEW SUBSEQUENT RELEASE OF UPDATED CCCD BY PRCBD CAP RELEASED AT L-4 MONTHS	PRCBD	PRCBD
AUTHORITY	NATIONAL STS MANAGER	MISSION INTEGRATION MANAGER	FLIGHT REQUIREMENTS MANAGER	NATIONAL STS MANAGER	

[Ref. 2/6-152]

FLIGHT READINESS

- O PRIOR TO EVERY FLIGHT (APPROXIMATELY 1 WEEK), THE SHUTTLE ELEMENT, FLIGHT OPERATIONS AND CARGO MANAGERS ARE REQUIRED TO ENDORSE THE COMMITMENT OF FLIGHT READINESS (COFR) TO THE NASA ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT AT THE FLIGHT READINESS REVIEW (FRR)
- o THE RESPONSIBLE PROJECT/ELEMENT MANAGERS CONDUCT PRE-FRR'S WITH THEIR CONTRACTORS, CENTER MANAGEMENT, AND WITH THE LEVEL II MANAGER
- o FLIGHT READINESS REVIEW (FRR) CONTENT
  - O OVERALL STATUS PLUS ESTABLISH THE BASELINE CONFIGURATION IN TERMS OF SIGNIFICANT CHANGES SINCE THE LAST MISSION
  - O REVIEW SIGNIFICANT RESOLVED PROBLEMS SINCE LAST REVIEW, AND SIGNIFICANT FLIGHT ANOMALIES FROM PREVIOUS FLIGHT
  - O REVIEW ALL OPEN ITEMS AND CONSTRAINTS REMAINING TO BE RESOLVED BEFORE THE MISSION
  - O PRESENT ALL NEW WAIVERS SINCE THE LAST FLIGHT
- O OPEN ACTIONS FROM THE FRR ARE ASSIGNED TO RESPECTIVE PROJECT/ELEMENT MANAGER AND CLOSEOUT OF ACTION MUST BE COMPLETED NO LATER THAN THE L-1 DAY REVIEW
- O ACTION CLOSEOUT IS REVIEWED AT L-1 DAY REVIEW WITH FORMAL SIGNOFF BY THE AA-OSF
  - O COMMIT TO FLIGHT IS REAFFIRMED AT L-1 DAY REVIEW [Ref. 2/6-153]

SA–1385

FLIGHT READINESS REVIEW (FRR)

- PURPOSE OF FRR IS TO REVIEW AND ASSESS THE READINESS STATUS OF THE MSFC SHUTTLE ELEMENTS FOR FLIGHT

- REVIEW PROCESS CONSISTS OF SERIES OF FORMAL AND INDEPENDENT PRE–BOARDS LEADING TO THE LEVEL I FRR

  – PRIME CONTRACTOR

  – SHUTTLE ELEMENT PROJECT

  – SHUTTLE PROJECTS OFFICE

  – CENTER

  – PRE–FRR (LEVEL II)

  – FRR (LEVEL I)

  – L–1 DAY REVIEW

[Ref. 2/6-154]

SA–1386

FLIGHT READINESS REVIEW (FRR)

- PRIME CONTRACTOR PRE–FRR

  – CHAIRED BY A LEVEL OF MANAGEMENT AT LEAST ONE LEVEL ABOVE THE CONTRACTOR PROJECT MANAGER

- SHUTTLE ELEMENT PROJECT FRR

  – CHAIRED BY THE MSFC PROJECT MANAGER

  – MEMBERSHIP INCLUDES SENIOR MANAGEMENT FROM SHUTTLE PROJECTS, SCIENCE AND ENGINEERING DIRECTORATE, RELIABILITY AND QUALITY ASSURANCE, SAFETY, AND THE PRIME CONTRACTOR

- SHUTTLE PROJECTS OFFICE FRR

  – CHAIRED BY THE MSFC SHUTTLE PROJECTS MANAGER

  – MEMBERSHIP INCLUDES SENIOR MANAGEMENT FROM SHUTTLE PROJECTS, SCIENCE AND ENGINEERING DIRECTORATE, RELIABILITY AND QUALITY ASSURANCE, AND SAFETY

  – SHUTTLE PROJECTS OFFICE FRR BOARD MEMBERS ARE REQUIRED TO PARTICIPATE IN ALL SUBSEQUENT FRR LEVELS THROUGH LEVEL I

[Ref. 2/6-155]

SA–1387

FLIGHT READINESS REVIEW (FRR)

- MSFC CENTER FRR
  - CHAIREO BY THE MSFC CENTER DIRECTOR
  - MEMBERSHIP INCLUOES:
    - OEPUTY OIRECTOR, MSFC
    - OIRECTOR, SCIENCE AND ENGINEERING
    - OIRECTOR, PROGRAM OEVELOPMENT
    - OIRECTOR, SAFETY
    - ASSOCIATE OIRECTOR FOR SCIENCE
    - OIRECTOR, RELIABILITY AND OUALITY ASSURANCE
    - OIRECTOR, PROGRAM/PROJECT OFFICES

[Ref. 2/6-156]

SA–114

FLIGHT READINESS REVIEW (FRR)
CONTENTS OF REVIEWS

- MISSION COMPATIBILITY
  - COMPATIBILITY OF MISSION REOUIREMENTS WITH HARDWARE CERTIFICATION ANO EXPERIENCE BASE
- HAROWARE PEOIGREE
  - CHANGES – ALL FIRST TIME CHANGES TO HAROWARE, SOFTWARE, PROCESSES, DMRSO, LCC'S/REOLINES ANO CERTIFICATIDN OF SUCH CHANGES
  - WAIVERS & DEVIATIONS – ALL FIRST TIME WAIVERS TD & OEVIATIDNS FRDM CERTIFIEO DESIGN AND PERFORMANCE REOUIREMENTS
  - MR ACTIDNS – ALL SUCH ACTIONS THAT FALL WITHIN THE ESTABLISHEO SPO BOARO CRITERIA
  - LIMITEO LIFE ITEMS – CERTIFICATION OF LIMITEO LIFE ITEMS FOR MISSIDN REOUIREMENTS INCLUOING SSME 50% FLEET LEAOER CRITERIA AND ALL DAR LIMITATIONS
- ANOMALIES AND DBSERVATIONS
  - ALL ANOMALIES ANO ALL OBSERVATIONS THAT INOICATE POTENTIAL TRENO CHANGE IN PERFORMANCE INCLUOING PREVIOUS FLIGHT ANOMALIES

[Ref. 2/6-157]

SA–118

FLIGHT READINESS REVIEW (FRR)
CONTENTS OF REVIEWS (CONTINUED)

- **SAFETY – R&QA**
  - ALL CHANGES TO CIL'S/HAZARDS AND ALL APPLICABLE ALERTS
  - NON–CONFORMANCES
  - LSS ASSEMBLY AND CHECKOUT PROCESS REVIEW
- **UNPLANNED OPEN WORK**
  - UNPLANNED OPEN WORK AND PLANS FOR COMPLETION/CERTIFICATION
- **SUPPORT OPERATIONS**
  - REVIEW OF SUPPORT TEAM AND FACILITIES
- **CERTIFICATION OF ELEMENTS**
  - EACH ELEMENT
  - SUPPORT TEAM AND FACILITIES
  - SPO BOARD

[Ref. 2/6-158]

ENDORSEMENT NO. 2: FLIGHT READINESS (CONTINUED)

This endorsement certifies readiness for STS-51L contingent on closeout of any exceptions noted herein.

RESPONSIBILITY	CONTRACTOR		NASA	
ORBITER (a,b,f,g,h,i,k,l,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	JSC PROJECT MANAGER	DATE 1/15/86
SSME (a,b,f,g,h,i,k,l,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	MSFC PROJECT MANAGER Endorsement Attached	DATE 1/23/86
EXTERNAL TANK (a,b,f,g,h,i,k,l,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	MSFC PROJECT MANAGER Endorsement Attached	DATE 1/23/86
SOLID ROCKET BOOSTER (a,b,f,g,h,i,k,l,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	MSFC PROJECT MANAGER Endorsement Attached	DATE 1/23/86
MSFC SHUTTLE PROJECTS (a,b,f,g,h,i,k,l,m,n)	N/A		MSFC PROJECT MANAGER Endorsement Attached	DATE 1/23/86
CARGO/PAYLOAD INTEGRATION (a,b,f,g,h,i,k,l,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	JSC MISSION INTEG MANAGER	DATE 1/15/86
FLT EQMT/GFE (a,b,f,g,h,i,j,k,l,m,n)	N/A		JSC FLT EQMT PROJECT MANAGER	DATE 1/15/86
SYSTEM INTEGRATION (a,b,f,g,h,i,k,l,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	JSC SYS INTEG MANAGER	DATE 1/16/86
OPERATIONS INTEGRATION (a,b,g,k,n)	N/A		JSC OPS INTEG MANAGER Endorsement Attached	DATE 1/23/86
VEHICLE/LAUNCH PROCESSING (a,b,c,d,e,f,g,j,k,l,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	KSC SHUTTLE PROJECTS MANAGER Endorsement Attached	DATE 1/23/86
CARGO PROCESSING (a,b,f,g,k,m,n)	PROGRAM MANAGER Endorsement Attached	DATE 1/23/86	KSC CARGO PROJECTS MANAGER Endorsement Attached	DATE 1/23/86
MISSION OPERATIONS (b,g,k,n)	N/A		JSC MISSION OPS MANAGER	DATE 1/15/86
SHUTTLE SYSTEM	N/A		NATIONAL STS PROGRAM MANAGER	DATE 1-24-86

NOTE:
1) This endorsement to be completed at the conclusion of Flight Readiness Review (FRR).
2) Open exceptions are to be closed or dispositioned not later than one day prior to flight.
3) See Log of Exceptions for explanation of each exception. Exceptions will be numbered consecutively, beginning with No 1

[Ref. 2/6-159]

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READINESS POLL - 51-L

AREA	CONTRACTOR	Ready
INTEGRATION	Rockwell	✓
ORBITER	Rockwell	✓
SSME	Rocketdyne	✓
ET	Martin Marietta	✓
SRB	USBI / Morton Thiokol	✓
SHUTTLE PROCESSING	Lockheed	✓
CARGO PROCESSING	McDonnell Douglas Astronautics Co.	✓
PAYLOADS		
TDRS-B / IUS		✓
SPARTAN-HALLEY / MPESS		✓
COMET HALLEY ACTIVE MONITORING PROGRAM (CHAMP)		✓
DOD		✓
ASAP		N.A.
CONSULTANT		✓
CHIEF ENGINEER		✓
OSTDS		✓
CENTER DIRECTORS		
-JSC		✓
-MSFC		✓
-KSC		✓
OTHER HEADQUARTERS		✓

Jesse W. Moore,
Associate Administrator
for Space Flight

Jesse W. Moore 1/15/86

[Ref. 2/6-160]

NASA

National Aeronautics and
Space Administration

Washington, D.C.
20546

Reply to Attn of: MOI

JAN 23 1986

TO: Distribution

FROM: M/Associate Administrator for Space Flight

SUBJECT: Space Shuttle Mission 51-L Flight Readiness Review Assessment

The Mission 51-L Flight Readiness Review (FRR) was conducted on January 15, 1986.

Based on the assessments and supporting documentation presented at the FRR, the Space Shuttle mission system is declared to be flight ready contingent upon closeout of action items and open work which were identified and satisfactory completion of vehicle servicing and countdown operations.

Jesse W. Moore

Enclosure
1. CoFR Endorsement Status
2. STS 51-L FRR Action Items

Distribution
(See attached)

[Ref. 2/6-161]

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PRESIDENTIAL COMMISSION ON SPACE SHUTTLE CHALLENGER ACCIDENT—FRIDAY, FEBRUARY 7, 1986

National Academy of Sciences
Auditorium
2100 Constitution Avenue,
N.W.
Washington, D.C.
The Presidential Commission met at 9:30 o'clock a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman
NEIL A. ARMSTRONG
DR. SALLY RIDE
DR. ALBERT WHEELON
ROBERT RUMMEL
DR. ARTHUR WALKER
RICHARD FEYNMAN
EUGENE COVERT
ROBERT HOTZ
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA

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ALSO PRESENT:
JESSE MOORE, NASA
ARNOLD ALDRICH, NASA
JONATHAN THOMPSON, NASA
MARV JONES, NASA
STANLEY KLINE, Federal Bureau of Investigation

Hearings pages 237 through 302 covered a presentation on physical security at Kennedy Space Center and Vandenberg AFB. In the interest of security at Kennedy and Vandenberg, these pages have not been reproduced.

STATEMENT OF MARV JONES, NASA

MR. JONES: Mr. Chairman, members: I'm Marvin Jones, Director of Safety, Reliability, Quality Assurance and Protective Services at Kennedy Space Center. The protective services includes fire and security.

Having gone through a rather lengthy title, I will focus on the very last one of those, specifically security. What I'm going to do is to focus on the security prior to the mishap and what we have done after that, and I will not spend a great deal of time, subject to your questions, as to the routine kinds of security that we maintain around the clock—

(Viewgraph.) [Ref. 2/7-1]

—other than to say that basically we have a very large reservation, around 100,000 acres. We have perimeter gates, then we have gates with guards further in at the critical facilities, and then finally some internal guards.

Now, you will note on the six or seven viewgraphs that I have a large number of acronyms. I'm sure I will be in trouble with Jesse if I use any of them. There is a list, if all else fails, attached to the back of your briefing that describes what each of these are.

DR. WALKER: Could I just ask one question? Are you completely separate from Patrick Air Force

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Base?

MR. JONES: No, we are totally a separate entity, with one exception. The Cape Canaveral Air Force Station and Kennedy Space Center are contiguous, and by agreement between the two organizations the Air Force guards the south side and NASA guards the west and the north side, with the ocean taking care of the east. It's a very cooperative venture, but totally separate.

A few days before the launch we established what is called a Blast Danger Area. This is essentially a circle of about a 4500 foot radius around the launch pad. We also established what we call an Impact Limit Line, which is basically three miles from the launch pad. The purpose of the impact limit line is that, if there is a catastrophic problem, that no major pieces representing danger to property or to life should fall in that particular area.

All of that then becomes a guarded area.

CHAIRMAN ROGERS: How large is that area?

MR. JONES: The Blast Danger Zone, sir, is 4500 feet from the center of the pad. It's a circle. And then the Impact Limit Line is basically three miles to the west of that. So that then the pads and the beach then are the unprotected area from the blast.

CHAIRMAN ROGERS: It is all sides three miles

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around and in the ocean?

MR. JONES: Let me have backup number 12 on the far screen, if I could, please.

What we have done is to effectively determine the maximum yield that the vehicle could generate if we didn't take any destruct action, so that we know essentially what kind of catastrophic situation we would have.

We then go back and draw an Impact Limit Line. In this instance, it is essentially right in front of the vertical assembly building, and then go due north, as you will see.

(Viewgraph.) [Ref. 2/7-1]

Here we have the dotted line, and we make sure then at launch time that we know that there is no one inside or to the east of that dotted line, which, as you see, comes down right by the landing facility, the vertical assembly building, on out through Cape Canaveral Air Force Station.

And then the circles, as you see, are the Blast Danger Areas.

CHAIRMAN ROGERS: What is the closest spot to the launch site?

MR. JONES: Well, this is actually on pad A, but it's exactly the same for pad B. We have 70 people who are inside

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that impact limit line at the time of launch.

CHAIRMAN ROGERS: My question is how far is it from the outside line to the launch pad B?

MR. JONES: From here? This is about three miles, from here to the pad.

CHAIRMAN ROGERS: So you're fairly comfortable that there were no people within that three mile area?

MR. JONES: Yes, sir, I am, with the exception of 60-odd people that I know who are there, and are there for a good reason. So we restrict access, of course, to that area at about the launch minus three day point and, with the exception of people who have to go to the launch pad for actual work, we also restrict access to the Launch Control Center and critical support facilities, such as some of the communications sites and some of the radar antennas.

Now, during this period of time we actually sweep the area twice daily. It is done with a helicopter with a security team on board. We do it at different times. And of course, we also have the guards that are still out in that area, and they are doing roving patrols and making sure no one gets in there.

CHAIRMAN ROGERS: Is it fenced in?

MR. JONES: The only fence we can speak to immediately is approximately 1200 feet out from the

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center of the launch pad, and it is a complete enclosed fenced in area, with guarded access to get in and out. That is the only fence.

There are other fenced areas, but the whole center as such is not fenced in, no, sir.

DR. WALKER: Does the fence go down to the beach?

MR. JONES: No, it does not. There is on the launch pad, 1200 feet out, and then there's a large open area to the east of both pad A and pad B. Then there is a beach road and then the actual beach itself. But no, they do not go that far out, although access to that beach road and that area is controlled through a guard post.

Now, at this point approximately three days prior to launch, anyone who needs to get into the Blast Danger Area has to go through a rather elaborate series of checks, ultimately being approved by the NASA test director, who is on duty 24 hours a day. And when he arrives at a control checkpoint, he checks in with the guard who is there.

And the guard will not accept that individual's word that he needs to go in. He must radio back to the launch control center and to the console, and then the console will discuss why this man wishes to go in with

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the NASA test director. And if he does not know why he needs to get in, he will not approve it.

And for example, if there was a problem on the pad with a fire detection system on the structure, he might send an alarm technician in. If the NASA test director was not aware of that problem, he would go back to the control console in the firing room to confirm that they had a requirement. Then he would approve him going in.

So we think we have good controls for any access to the stack or the entire vehicle on the pad at that particular point in time.

(Viewgraph.) [Ref. 2/7-1]

Let me say that, really up front from our thing, we see absolutely no evidence as of this minute of any sabotage attempt, any willful attempt to damage the hardware, or any terrorist activity. And I might also add, we have no claims of that, which would not be out of character for terrorist activity.

Essentially, as we got closer to the launch, we of course were monitoring all of the security implications, and we simply had no unusual security incidents reported. We also take a pretty

good look—and generally, I've been focusing on the land areas right now, but as we got closer to the launch we

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implemented certain launch area restrictions in the surface out in the water, and we had normal United States Coast Guard support, and also the Range Control Center, which is operated by the Air Force.

And we had various helicopters and boats operating out there. And in fact, on launch day we had three Coast Guard vessels, five helicopters, plus radar, surveiling the launch area.

And all of the logs and all of the tapes that we have been able to come up with so far reveal that there were never any reported penetrations of any boats in that area. On one of the NASA helicopters we had a brand new security officer who was being trained for the first time, was riding backwards in the helicopter and spotted what he thought was a small boat. It was over water and he was looking over water and had no frame of reference.

And we have interviewed everyone else on that helicopter and we believe that he spotted one of the Coast Guard boats. He thought it was 1200 feet away and it turned out it was more like five miles. He thought it was a 14 footer and it was a 41 footer.

But to substantiate their memories, we have also gone back and reviewed all of the films that would have any evidence of that area. On top of the large

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building, the vertical assembly building, there are a couple of cameras and they are essentially looking down at the pad. But, this area, this suspect area behind, and we see no evidence of a ship.

I might add, there were eight to ten foot waves a mile out to sea, and we suggest a 14 footer would have been probably at some degree of risk to have been out there. And so we don't believe that this spotting was accurate.

CHAIRMAN ROGERS: Did this result in an investigation?

MR. JONES: Sir?

CHAIRMAN ROGERS: Did this result in an investigation by you?

MR. JONES: Well, in the course of events post-accident this came up, about three days later, which led us then to go through the interviews and re-examine all of the tapes. And so it was a reaction on our part.

CHAIRMAN ROGERS: Did you file a report or is there a report of that incident?

MR. JONES: No, sir. It was reported verbally to us, and then we have documented that as part of the files that we are building.

DR. WHEELON: Would it be unusual for such a boat to be in the area at the time of the shuttle

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launch?

MR. JONES: No, and I guess if the seas had not been rough it would have been just the opposite. We have experienced—I might add, I've been involved in every single one of the launches. We have had a large number of vessels attempt to get very, very close. We have had a large number of aircraft that have intruded into the area. We have actually had to hold launches for aircraft.

And that is why the Coast Guard is out there, simply because it is very common. It is a heavy area for boating interest, as you know, as well as fishing interest. And in fact, one day we

had the QEII simply pull up and stop with a full load of people to see it. And so it is quite common to have vessels.

But they all know the area is closed. Notices are sent out to them to tell them to stay out, and then of course the Coast Guard enforces it, as well as our helicopters, using loudspeakers or hailers.

DR. WHEELON: So if this should turn out to be a valid report, you wouldn't be surprised?

MR. JONES: No, I would not be surprised, that is correct. But at this point there is no evidence to suggest that it was valid. We believe that he saw something, but what he saw is the question.

MR. RUMMEL: Can you clarify the parachute

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incident that was reported in the press?

MR. JONES: Yes. As you are aware, the solid rocket boosters are parachuted back. Initially when they come down, a drogue chute deploys, and once that's stabilized then at a certain point the other parachutes are deployed.

And what had happened, as best we can tell, that during the sequence of events post-event, post-accident, and post range safety destruct action, the parachute simply deployed. And what you were seeing then was a part of one of the solid rocket boosters as it came down.

MR. RUMMEL: Was it recovered?

MR. JONES: We do have a large number of those parachutes that came down, but the first report of a paramedic going into the scene by parachute was totally false. That was simply an assumption on the part of the author.

CHAIRMAN ROGERS: I do think that these two points illustrate what we were saying earlier. Anything that we do not comment on in the report will be subject to later rumors and criticisms that we didn't even investigate those things.

DR. WALKER: These instances will have to be in our report.

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CHAIRMAN ROGERS: Absolutely.

DR. WHEELON: How close was the nearest Russian vessel?

MR. JONES: We did not have one that was close enough to be a factor, which is fairly common for us.

DR. WHEELON: How close was the nearest Russian vessel? Not that it was a factor, but how close was it?

MR. JONES: I don't have that information.

DR. WHEELON: Can you find it for us?

MR. JONES: It's no problem finding it. I just don't have it.

I do know that from my own point, that he was not within visual sighting of the areas where we normally—where it would be common to see them. He wasn't three of four miles out, but I will find the exact location and provide that to you.

DR. WHEELON: On previous launches, how close do they come in?

MR. JONES: The closest I have seen them on one of these actual launches was I think about three and a half miles out, outside the legal limit. I have seen them probably within 100 yards of the three mile limit, very early in the Apollo program.

In fact, I believe for STS-1—and this is

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strictly from memory now—I believe they were more interested in the solid rocket booster splashdown area, and so we have seen them in various positions, close or a little further out.

DR. WHEELON: And then just in a qualitative way, on this one were they relatively close or relatively far away?

MR. JONES: To the best of my knowledge, the closest one was up north of Charleston, South Carolina. But again, that is from memory and I'm going to have to check it.

DR. WHEELON: So they were nowhere near the area? They were unusually absent?

MR. JONES: No, they have been off and on. We have had several launches where they didn't show up.

DR. WHEELON: On this launch were they unusually absent or in their normal position?

MR. JONES: Their normal position, from my perspective.

DR. WHEELON: Could you clarify that and be a little more precise?

CHAIRMAN ROGERS: Jess, I think on those points you should have a complete file, and I don't care what you call it. We will have to have a section dealing with every one of these things. If we don't exclude

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every possibility with some convincing evidence, we're going to be subject to criticism for a long, long time. And if you remember the Warren Commission, that is exactly what they were criticized for, failing to do this and that and other things.

They did a good job and they did all the things, probably, that any commission should do, but for years they have been subject to that kind of criticism.

So each one of these things, by asking the question we don't mean you haven't done a good job. We just want the material, so that you will have it ready when we need it, to exclude these possibilities.

And we are going to be working on the exclusion theory most of the time, probably. We're not going to discover something, so we're going to have to exclude a lot of these things and say, here is what's left.

MR. MOORE: Yes, sir. We will make sure that gets documented in great detail, and go back and look at the history from the first flight all the way up to this one, and try to give you a relative comparison of it.

MR. HOTZ: I think the point here is, how many—do they—or how do they behave when there is a launch of unusual interest, in contrast to how they behaved here.

MR. JONES: We've seen their interest vary from

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high level of interest, close by, to no interest at all, simply by not being in the immediate vicinity and so you wouldn't be aware of it.

We know through intelligence sources where they are, and routinely prior to the launch the Air Force range commander is briefed on the location of those ships. So it's not uncommon to find them in Charleston or down south and sometimes in transit, going home. And so we get that data routinely.

It is just that I did not get it for this particular flight.

MR. HOTZ: But they didn't appear to be particularly interested in this flight?

MR. JONES: No, not from my perspective.

MR. COVERT: When you say they're highly interested, is that five ships, two ships, one ship?

MR. JONES: In the immediate launch vicinity, I've never seen more than one close by.

MR. MOORE: You also have to realize they put ships in the splashdown area.

CHAIRMAN ROGERS: Most of the intelligence they get is from other sources, anyway.

(Viewgraph.) [Ref. 2/7-1]

MR. JONES: One of the other areas that we concern ourselves with prior to launch is what we call

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mail-package screening. Essentially, what we do is to screen all the mail that is addressed to our center director, to the astronauts, and we also take a look at any other mail which seems to be consistent with the profile that one would anticipate finding if you had a suspicious letter or a letter bomb, something of that nature.

And we use the data or information from the FBI to establish what the profile is. And we had no significant mail of any type whatsoever, announcing any threats or any significant events as far as mail was concerned.

We did have a couple of packages that we thought were a little suspicious, and on actual examination again absolutely no significance related to the mishap.

(Viewgraph.) [Ref. 2/7-1]

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MR. JONES: Turning now to the period of time immediately after the mishap, as you would perhaps expect we've had numerous reports of suspicious persons. We've had numerous letters from a wide variety of people around the country.

Primarily, though, they have been of what we would characterize as the kook type, telling us what went wrong. No one in any of those letters has claimed responsibility, announced any threats or anything.

We feel that just because it appears to be a kook letter to us is not in itself good reason to put it in a round file. As a result of that, we're working very closely with the FBI, the Secret Service as necessary, local law enforcement officers, and we intend to pursue each of those to a logical conclusion to satisfy ourself that in fact it was a kook.

DR. WALKER: You say numerous. About how many?

MR. JONES: Oh, I saw five or six yesterday. We have had reports from FBI field offices around the country that they've received some. I think one day last week I had five letters addressed to Jess Moore that we received at Kennedy. I would think probably a hundred would be a fair representation at this point.

CHAIRMAN ROGERS: It's amazing there are so few.

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MR. JONES: Well, Mr. Chairman, there will probably be a lot more than that ultimately.

We also are concerned, of course, with the water areas, and as noted here on the chart that area is still controlled by the Coast Guard while the search and recovery process is going on.

Late—early in the evening after the mishap occurred, the Vice President came down, as I'm sure many of you know, and during his visit there or shortly after he departed we had a small boat that was reported about half a mile off the coast adjacent to Pad B which is shown on the map over there.

He seemed to just pull up and just stop in the water, and he was spotted by our security forces on the beach road. We tried to shine lights out and get some identification off the boat. It was not a large one, and we were never able to identify him.

We tried to raise him with a radio and a PA system, public address hailers, and he did not respond at all, and so we called the Coast Guard.

As you can imagine, this was about nine hours after the accident. The Coast Guard was simply too busy to come and investigate him. He did not declare himself in distress, and after about thirty minutes he drove away, and unfortunately we were not able to get any

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identification on him.

Our assessment is at this point it was probably a curiosity seeker, but we're trying to make every attempt to see if we can identify him. I am not encouraged that we will be successful at this point.

DR. WHEELON: From the way you describe this incident, you seemed to identify it with the presence of the Vice President. Did you mean to?

MR. JONES: Yes.

DR. WHEELON: Why?

MR. JONES: I will in fact come back to that in a couple of minutes.

Well, what we have taken a look at was what was not normal, and was it not normal because of some malfunction? Was it not normal because it doesn't happen? The Vice President being there is not normal. He doesn't routinely come down there. We had that boat incident that I just referred to. We had the Vice President on hand.

CHAIRMAN ROGERS: Could you give us the facts again on the boat matter? I wasn't clear on the facts.

MR. JONES: It seemed to be a small boat in the 15-18 foot range.

CHAIRMAN ROGERS: What happened to the man in the boat?

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MR. JONES: The boat stopped about a half a mile out to sea, approximately adjacent to the launch pad that we have used. We signalled him with flashlights, car lights, tried to speak to him on loudspeaker systems in the police car. He never responded to us at all. He never identified himself. We had no radio transmissions from him at all.

We asked the Coast Guard to come investigate, and since the ship had not declared himself in distress the Coast Guard opted to not show up.

CHAIRMAN ROGERS: Then what happened?

MR. JONES: Then he——

CHAIRMAN ROGERS: He didn't land or anything?

MR. JONES: No, sir. There was no evidence. We were standing there with armed guards in case he tried it. He just cranked up his engine and left.

DR. WALKER: It's not illegal for that person to be there because it's not a launch.

MR. JONES: That's right.

DR. WHEELON: But why do you tie that to the Vice President's presence?

MR. JONES: Because of the presence of the Secret Service there and his presence, we were being very sensitive to any event that was out of the ordinary, and

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that was out of the ordinary.

We were also concerned because of the mishap with anything that was out of the ordinary.

At approximately the same time, and I don't have the precise times with me. Over on Cape Canaveral Air Force Station there is a balloon that is called—well, it's tethered aerostat radar system, or nicknamed Fat Albert. Fat Albert crashed into the ocean at approximately the same

time. All three of these mishaps occurred, of course, shortly before noon. The Vice President arrived at 5:00 and was there until around 7:30. This event occurred about thirty minutes later, and within an hour of that time the balloon fell into the ocean.

The initial report on the balloon falling in was that there was a report of small arms ground fire, obviously a non-normal event, and that gave us a great deal of concern. It now turns out that the investigation revealed the balloon was being hoisted back up. It is taken down routinely before Shuttle launches and was going back up to aid in air traffic control over the search area because we had a large number of airplanes.

The balloon was probably up over 10,000 feet. The tether broke, and as each of the different strands

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began to break under a lot of stress and high strain, then the Accident Investigation Board believes at this time, and their findings are not final, that what was heard was in fact the strands breaking.

So these were the not-normal events that I would actually include in there, and of course ground fire being heard within approximately ten miles of the Vice President gave us some concern, but again, no evidence whatsoever that there was ground fire.

MR. HOTZ: Fat Albert doesn't normally fly that high, does it?

MR. JONES: Normally it's about 12,000 feet.

DR. WHEELON: If the Vice President hadn't been there, would you have been surprised that there was a boat in the place you described?

MR. JONES: I think because of the mishap, yes, because we wanted to keep that whole area sterile at this point. It was obviously just a few hours after the accident, and we simply did not know what had occurred. We knew that we had floating debris out there. We had objects being washed up on the shore by this time, and I would not have wanted him there.

Now, I do not know as of right now what time the Coast Guard declared it a closed area. That is part of our trying to tidy this particular event up, because the Coast Guard did close that area to all surface

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shipping.

DR. WHEELON: But you see the point of my question. You were tying it into the Vice President's being there. Had the Vice President not been there and he had still come, perhaps he was unwitting of the Vice President's presence and he was in an area where he shouldn't be, and all that sort of thing.

But it seems pretty normal to me that there would be a lot of curiosity seekers. Why are you so concerned with this?

MR. JONES: I'm not concerned with it at all, sir. I just simply am reporting the facts to you as I see them.

CHAIRMAN ROGERS: Jess, consider if you will, and you don't have to decide now, whether at some time in a public session this kind of report would be useful.

MR. MOORE: Okay.

CHAIRMAN ROGERS: In other words, this is the kind of thing that would show care, and it would show that you have done a lot of work ahead of time and you have excluded some possibilities in public, so that would give us a basis for the report that we will make.

MR. MOORE: Yes, sir. I think that if we continue to go in and get most of the details put in to place and kind of get a big picture story, then I think

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we can talk to you about some version that is going to the public.

CHAIRMAN ROGERS: I think that you might keep in mind what kind of public sessions we can have without damaging your investigation and still reassuring the public that a lot of things are being done.

MR. MOORE: Yes, sir.

CHAIRMAN ROGERS: Go ahead.

(Viewgraph.) [Ref. 2/7-1]

MR. JONES: The next day after the launch we decided that one of the most important things that we could do was to search the area immediately adjacent to the pads, and we were perhaps somewhat influenced by what Dr. Kutyna suggested in view of the latter part of this in a sabotage event.

What we did was take thirty of our very well specially trained investigators and formed up five-man teams, and we spent the next three days closely examining the entire area within 2,000 meters of the launch pad; some pretty formidable terrain, some wild animals, alligators, virtually impassable areas out there.

We were simply searching for any evidence that someone had been there; food, paper, cigarette butts, scuffed areas, broken branches, flattened weeds. We

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simply found nothing.

Now, related especially to the sabotage issue, we did find a few items of debris that could be orbiter related. We very carefully plotted where all of those were. We photographed them in place. If they could, we felt, stay secure, we would leave them there and get a trained engineer who would understand the thing better than my security people would, to come take a look at it.

To date we have found—well, I think we found about 25 items, many of which we have discounted as being orbiter related, a couple that were related to the vehicle. It is not uncommon to find a little bit of foam, for example, and an occasional piece of a tile, those kinds of things.

But no one who has examined the debris that we have found to date reads any special significance into it. We do have, of course, exactly where it was located.

As a separate action while we were doing the area outside of that perimeter fence which I referred to a few moments ago, we had a special facilities team, who understood the launch pad from an engineering point of view, do a complete walkdown of the launch pad, and that data will of course be available to you in a later forum.

After they finished, then we went back with some of our security people to take another complete

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look at the entire area, essentially from the very top of the fixed service structure all the way to the ground. Yes, we started yesterday morning and should have finished late last night after I left for here, with a complete security investigation of the entire inside of the area.

Again, as of now, we have not found anything of any concern to us, and we have continued on using the mail, as I referred to a little bit earlier, and we are working with all of the other agencies who have perhaps received mail or those who have not. We're using their talents to help us in our use of it.

(Viewgraph.) [Ref. 2/7-1]

MR. JONES: As we get official mail for the Board, it goes to the investigative board that we have. Unofficial mail goes to our public affairs office, and any suggestions or information relating to the mishap goes to the security office as well.

Additionally what we're doing, we are continuing to review all of the films that we have. The 60-odd people that I referred to who were inside the Impact Limit Lines are all being interviewed. We are satisfying ourselves that there were always two or more people together, that no one person was in there alone.

We are in direct liaison with the Air Force

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Office of Special Investigations and the Naval Investigative Service, the local law enforcement agencies, the state as well as the FBI, as I was saying, and the real bottom line is, sir, as of this point there have been no claims of responsibility, and we have no evidence at this point that there was any attempt of sabotage or terrorism related to the orbiter. Just because we haven't found anything doesn't mean that we will stop looking.

DR. WALKER: The 60 people who were there, would they be doing things like operating cameras?

MR. JONES: Yes, there were camera operators, security personnel at various roadblocks, and fire, crash and rescue in the event that we had a problem on the pad to help the flight crew get out of the pad and get into a safe area.

DR. WALKER: So, they were all officially there?

MR. JONES: They were all there. We know exactly who they were and what their job is. It is a very limited number, and that is why we know precisely who the individuals are.

DR. WHEELON: Let me address a question to you and to Jess, if I may, meaning both the security and the technical side.

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By the inspections that you, visual and otherwise, that you performed prior to the launch, can you preclude the possibility that an explosive was attached to the vehicle?

MR. JONES: I would say from my perspective, no, I cannot exclude that until I have examined all of the film and satisfied myself and looked at it [or]

MR. MOORE: I was just going to say that what we have tried to do is to look at the outside aspects of this thing to make sure that we could not find any, and we did not find anything suspicious.

We still plan to continue the investigative process, but we can't exclude that as an absolute possibility until we look at all of the photos from all aspects, and it's not clear that we're going to get coverage of all aspects of the areas we might be interested in. We can't exclude that possibility.

DR. WHEELON: Was the vehicle thoroughly photographed prior to launch?

MR. MOORE: Yes, we have photographs of the vehicle very, very close up prior to the liftoff.

DR. RIDE: Did you get pictures of the right SRB?

MR. MOORE: We got pictures of the right SRB; not total, however. They are in stacks. There are parts of it that are

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excluded and so forth that we do not have pictures of, Sally, so there may be some area there that you're just not going to see anything.

GENERAL KUTYNA: Jess, on our guidance systems the accelerometers are sensitive enough if I had an explosion aboard my vehicle it would be picked up by the accelerometer. Does the Shuttle have the same kind of sensors?

MR. MOORE: The Shuttle has some pretty sensitive things on it to pick up G-loads and the movement of the bird, like that and so forth, and that data is under work right now.

GENERAL KUTYNA: So as you analyze it, possibly if you had anything you would see it?

MR. MOORE: The loads analysis is a very critical question we're addressing right now in addition to the events time line which, as I said earlier, needs great correlation from everybody that was taking flight data in real time, and that seems to me is a systems thread one has to go through to get the people to agree on what the events are.

Then the loads analysis is another major effort that is going on right now to try to understand the dynamics of the loads that were on that vehicle as a function of its flight.

DR. WHEELON: Did you take—not have you analyzed, but do you have in your possession a good

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photographic coverage of the area of the SRB which is presumed from the in-flight photography to have been a problem?

MR. MOORE: We have good photography of that SRB, but we do not have photography competely 360 degrees around the SRB, so there are some limited zones that we do not have photography of.

DR. RIDE: But I think there is photography of the area where the plume comes out.

MR. MOORE: There is photography but, Sally, there is not photography of exactly where the origin is at this point in time. At least the guys have told us that we do not see that area exactly where it comes out.

DR. WHEELON: But is there prelaunch of that area?

MR. MOORE: There are prelaunch photos of the entire stack that our photography team is going into and putting together right now, including as I might say prelaunch closeout photographs of all the flight segments and the various phases. We are trying to pull that whole stream of photographs together.

DR. RIDE: Do you do closeout photos of the vehicle on the pad just a day or two before, or are most of those closeout photos in VAB?

MR. MOORE: Most of those closeout photos are

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on the VAB. There are some pictures taken at prelaunch but not in great detail from the time it goes to the pad until the time of launch.

DR. WHEELON: What kind of closeout photography do you have on the day of the launch?

MR. MOORE: The photography we have on the day of launch is the still cameras that were sitting there taking pictures during the actual liftoff. How many pictures have we seen of that still photography, Arnie, ten or so?

MR. ALDRICH: We personally have not looked at but a small percentage of the total number of films and locations. They are available.

MR. MOORE: But we have a real time camera that looks at the launch pad that is transmitted back into our console area that looks at video and so forth. There are also cameras that are sitting out at the pad.

DR. RIDE: I was going to ask you about those. There are a million cameras on the pad. Are they running the day of launch?

MR. MOORE: They are running at some time before launch, Sally. I don't know exactly that time. Marv, do you happen to know that time?

MR. JONES: Well, there are a series of them out there, sir, activated one of two ways. One are

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sound activated, and the others are light activated. So essentially, Sally, they would be at ignition.

DR. RIDE: I'm talking about the ones that are basically—gosh, there must be almost a hundred percent coverage of the pad. You can't stand any place there without being in view of a camera. Just on, if you go out there a month before launch or something, there are cameras running all the time everywhere, and I was just wondering when those are turned off.

MR. MOORE: I don't know the answer to your question, and our photography team down at the Cape is pulling all of that data together and we have not looked at that data yet. We haven't had a chance to look at it, and I don't know the answer to your question. I don't know specifically when they are turned off. There are a lot of cameras out there.

CHAIRMAN ROGERS: Would the Commission be able to look at those pictures whenever we want?

MR. MOORE: Yes, sir, we would be more than happy to provide this Commission with any of the photographic data that we are looking at.

CHAIRMAN ROGERS: It probably would be better to do it down there.

MR. MOORE: Yes. There's a photography lab that we had set up down there, and we have got a major

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team of people that are just doing nothing but looking at photography and trying to enhance the photography and looking at it from different aspects.

So, yes, at the appropriate time it would be quite good.

CHAIRMAN ROGERS: We would not want to do it at a time that would interfere with whatever they're doing, but from the time standpoint I think that would be useful, just so we don't interfere with the analysis.

MR. MOORE: Yes, sir, and I would say sometime in the next several days or week or week and a half we would probably begin to have a good photographic story together on the sequences that we see.

We may not have all of the fine enhancement done yet on the photography, but I think we will have a good knowledge base of the data in terms of the complete set of events sometime within the next several days or week.

CHAIRMAN ROGERS: Is this classified?

MR. JONES: No, sir.

MR. MOORE: Only sensitive from the standpoint of the public and exciting the public again.

CHAIRMAN ROGERS: Why don't we give it back to you? We don't need it.

MR. ACHESON: Has there ever been a sniper

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incident at the Center?

MR. JONES: No, sir. One of the things that we thought, that Dr. Graham suggested that we offer to the Commission, is the subject of the generic terrorism threat at Kennedy Space Center.

We have asked the FBI to share that information with us, and I think they will have a few minutes on that.

DR. WALKER: Can I just ask one question on that? Would you say that your security is sufficiently tight so that in your mind no one unauthorized could have gotten to the vehicle at any time when it was undergoing any of its operations at the Cape? It sounds like that is the case.

MR. JONES: We believe that we have a very positive control system. I would be very hesitant to say absolutely no one can get in. I think I would be foolish to make that commitment to you, but I think we have a pretty positive control system.

DR. WALKER: The intent is good.

MR. JONES: The intent is good. I think we have reasonably good control.

DR. RIDE: As a result of what you've been doing just the last week or so, have you come up with any suggested improvements for the security system, or

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have you got any recommendations?

MR. JONES: Oh, I could come up with a million dollars worth of improvements probably overnight, Sally; infrared detectors, closed circuit TV cameras, intruder alarm detection systems. But yes, I could improve on the system.

DR. RIDE: I guess that is kind of the same as Art's question, which was do you think it is possible for somebody to have gotten on there and sabotaged it, not on this particular launch but just generically? Is that something you're concerned about?

MR. JONES: One, I think, it would have to be an orchestrated effort by more than one person because I think we try to——

CHAIRMAN ROGERS: Could you just say you can assume that what is happening in the world is that there are all kinds of orchestrated efforts and they always have more than one person, so that's an assumption.

MR. JONES: We try to keep two-man control anytime anyone is out there. There are obviously times when you can't do that, when you're getting back into the aft end of the engine department, and so on.

But we do try to make that effort.

CHAIRMAN ROGERS: I think this is really a

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vitally important issue because of terrorism in the world. I mean, this is a natural place, particularly with this accident.

MR. MOORE: Yes, sir. As visible as the Shuttle program is worldwide, I totally agree with you.

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CHAIRMAN ROGERS: It seems to me we should continue to draw on this one as long as necessary to make sure it is absolutely secure.

DR. RIDE: I think this is a good area of investigation to be sure what the possibilities for sabotage are.

MR. JONES: We have done some fairly lengthy studies and tried to come up with some scenarios, and then we have characterized those in terms of attractiveness to an outsider, and then in terms of likelihood, and then we have those kind of numbers.

CHARMAN ROGERS: When you talk about attractiveness, do you have any scenario to handle a small plane with a suicide pilot?

MR. JONES: No, sir. Well, yes, we do, as a matter of fact, and I think that I would have to say that we are—and I would like to treat this portion of it, sir, if I could, as classified.

It is simply a vulnerability that we cannot address. It is rather ironic that one week ago today I was supposed to be in this very building meeting with the Presidental Protective Detail to discuss their procedures of how they protect the air

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space over the White House, and that was an arranged meeting which, of course, was cancelled, but to address that very subject.

DR. WALKER: But this is an area of concern for you?

MR. JONES: Well, I think it is, and of particular concern, and I happen to have had a military background as General Kutyna does. The rules of engagement are horrifying. An airplane comes toward the launchpad and you get terribly concerned. At what point do you shoot him down? Only to find that it was a poor young man from Memphis on his way home from the Bahamas, and was curious. I mean, he shouldn't have been in that air space.

DR. COVERT: Or lost, even.

MR. JONES: But it could occur. And that is kind of a real problem to come to grips with. The rules of engagement and how do you protect a 40-story building which is the launchpad with a vehicle on it?

CHAIRMAN ROGERS: All you have to imagine is the type of terrorist who drove the truck into our embassy, and I guess I accept the fact that probably there isn't anything you could do that is completely safe. But I do think the record should reflect that you are giving a lot of consideration and you are trying

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to do something, and that there are some steps that can be taken along that line. I think that would be very helpful.

MR. MOORE: Yes, sir. We have not given up looking into the internal prospects as well. I mean, I think Marv Jones and company have done a good job of sweeping some of the areas. He told me that they walked over some areas on the land around the launchpad down there.

That is probably the first people since the Indians left that area down there, and so they have covered a lot of acreage around the pad, but I do think that we have to continue to look internal as well at our own system to see if there are some suspected areas that we would want to proceed on. So I think this is by far not closed at this point in time, and we need to continue to work this area.

CHAIRMAN ROGERS: Thank you very much.

MR. MOORE: Sir, the next thing is that we asked for a very short presentation by the FBI on the general threat in the area, just as some background information for the Commission.

Viewgraphs introduced by Marv Jones on February 7, 1986 were not published for security reasons.

[Ref. 2/7-1]

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STATEMENT OF STANLEY KLEIN, FBI

MR. KLEIN: Thank you. My name is Stanley Klein. And I am an FBI special agent, and as such, I direct the bureau's counterterrorism efforts in the United States, both domestic and international. I am assigned to FBI Headquarters.

I would like to begin by saying that Director Webster expresses his greetings and offers the full resources of the FBI to this Commission and to NASA. We are now working with NASA in Florida and throughout the country in trying to dispel, if that is what we should do, any hint that a terrorist has committed sabotage and caused the explosion of the Space Shuttle.

What I would like to do is touch on terrorism briefly, what we see in this country and what we view as the major threats, what we have seen so far, and some of the inquiries we have looked at based upon information supplied to us by private citizens and the news media, et cetera, and where we stand and where we hope to go in the future.

We have approximately 18 domestic terrorist organizations currently under investigation and 40 some odd international terrorist organizations under investigation operating within this country with many hundreds and hundreds of supporters and infrastructures

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and groups, et cetera, and it is a tangle of motives and ideals and religious fanaticism, et cetera.

I would like to concentrate on Florida more than anything else to show you what we see there now. Between 1981 and 1983, there were nine bombings and seven attempted bombings and one kidnapping carried out by terrorist groups or alleged terrorist groups in the Florida area. All 17 of these incidents were in Miami, Florida.

There has been no indication of terrorist activity up around the Cape, and there don't seem to be any groups operating in that area. A group called Omega 7, which was an anti-Castro Cuban group some of you might have heard of, were the perpetrators of these bombings. The leader, Eduardo Arencino, was arrested not too long ago, and is currently serving a life plus 55 year sentence, I believe, and with his prosecution and conviction, there have been no terrorist acts in Florida since that time, in the past year or two.

The major threats in the United States right now, we believe, are posed by Libya, by Iran, and by domestic terrorist groups operating out of Puerto Rico, which is not that far from Florida. As far as Iran is concerned, we have seen no terrorist acts committed by that country in this country.

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Most of their activity centers around intelligence-gathering and attempts to purchase weapons and spare parts to ship back to Iran to support their war with Iraq. They have a large organized infrastructure in the United States based mostly on college campuses, and they collect their information from meetings and taskings that they get directly from Iran, and we believe

we are about as on top of their operations as we can be at the present time. We don't believe that any Iranian terrorists were involved in the action on the disaster of the Shuttle.

Libya. Although Mr. Qaddafi makes a lot of pronouncements and he makes it seem as though he can reach into our streets, I believe, and our investigations have shown that he has not as yet reached into our streets. When he has attempted to, we have been able to stop him. The attempts of intelligence officers that he sends into this country are almost naive. They seem to be the gang that couldn't shoot straight. They go through intermediaries to get things done, and because they do, we are able to insert often undercover agents and thwart their plans before they occur and arrest their operatives.

DR. WHEELON: Stop right there. You would agree they reach into the streets of Lebanon, wouldn't

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you?

MR. KLEIN: Absolutely. Could they reach into the streets here? Of course.

DR. WHEELON: And haven't they drilled some of their own people here on our own streets?

MR. KLEIN: What Qaddafi has done is offered training to individuals in the states that he believes would form a support base for his view, his Green Book view of what the Arab world should be in the states and support his causes, and because of that people have been flown or have flown themselves to Libya and participated in training in that country.

Most of that training is what you and I would refer to as basic training, some basic military skills, political indoctrination, and through the intelligence community and through our sources we have not seen Americans being trained there for what we would define as terrorist activity.

DR. WHEELON: I appreciate all of that, but isn't it true that they have in fact killed their own people here in this country?

MR. KLEIN: They have made attempts to assassinate dissidents in this country. And the last instance was about a year ago, when they did send an intelligence officer into the United States, and he was

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forming up an organization to plan the assassination of anti-Qaddafi dissidents. We were fortunate enough to thwart that plan, and he left the country, and we have the people that are part of his group currently under very close scrutiny.

DR. WHEELON: But there have been no killings?

MR. KLEIN: No, there have been no terrorist acts in this country involving assassinations, murders, or bombings that were supported by Qaddafi unless you go back to the assassination that occurred in Colorado by a surrogate, whose name escapes me at the moment, who did shoot, attempted assassination, I should say, a Libyan dissident in Colorado.

This guy was part of the Wilsin-Terpil network, and of course Wilson is in jail now, so he did make attempts, but he has not really succeeded.

DR. WHEELON: I don't think it is relevant. I was just trying to focus in on that.

MR. KLEIN: I just wanted to touch on Iran and Libya briefly to say that we have no information based upon our best intelligence, which includes human sources and technical sources, that either Iran or Libya, our two greatest threats in this country, were involved in any actions against the Shuttle.

That is not to say that information such as

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that might not come up in the future as we continue probing our sources and things such as that. The only viable domestic group would be an organization that calls itself the Macheteros out of San Juan, Puerto Rico. They have been involved in numerous terrorist acts on the island, one of which is interesting because it does involve explosions that damaged aircraft on the island. This occurred on January 12th, 1981, where a series of 18 explosions occurred which totally destroyed a Corsair, two A-7B subsonic attack planes, damage to one Starfire F-104 aircraft, and also some equipment that was in the National Guard air corridors alongside the planes. They went in, cut through a chain link fence, went in at night, planted their explosives, one in the front of the plane and one in the back.

Let's see. There were, I think, eleven devices planted, and it caused $45 million in damages. The Macheteros have also been active in this country. They were responsible for a multimillion dollar armored car robbery in Hartford, Connecticut, in which we have staged a series of raids and arrests in Puerto Rico in the latter part of last year and arrested eleven individuals, the leadership of the Macheteros.

And they are currently incarcerated and about to go on trial, hopefully by the summer or fall in

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Hartford for that armed car operation. We do not have any indication that this group, which does have the skills, and could have the desire to make a statement, was involved in any action that was taken.

CHAIRMAN ROGERS: Are they supported by any foreign governments?

MR. KLEIN: Cuba. The Puerto Rican independence terrorist groups that operate in Puerto Rico we have definite very interesting information that is going to probably come out during the trial process about the Cuban involvement in support of Puerto Rican terrorism going on.

DR. WHEELON: Okay. Now, you fellows rolled up a Puerto Rican gang in Chicago which was preparing for activity about two years ago.

MR. KLEIN: That group was the FALN, which is the Puerto Rican liberation movement in this country. We did conduct a series of arrests. We did arrest a number of people up there. There also have been a number of people arrested in New York and in other parts of the country. Most of the leadership of the FALN is in jail right now. And I think with the FALN in jail and the Macheteros in jail, we are going in the right direction, and that is how we look at terrorism, as criminal acts. I mean, we don't look at it as political acts. And we try to

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identify those people who are involved and put them in jail, and all of a sudden the terrorist statistics go down.

DR. WALKER: What about the neo-Nazi group that was just sentenced?

MR. KLEIN: The Aryan Nation is a rightwing organization. Again, its leadership, eleven of them, ten or eleven of them were just convicted of racketeering charges in the state of Washington and are currently awaiting trial. There is no indication, although they certainly have the wherewithal as far as weapons, homemade weapons and explosive expertise to do something like that, there is no indication either that this group was active or is active or has been active in Florida.

DR. WALKER: Going back to Libya, what about Farakhan, who is making some noises about going to Libya?

MR. KLEIN: Yes, we are very interested in Mr. Farakhan.

DR. WALKER: Do you think it is mostly noises?

MR. KLEIN: Well, he certainly received a lot of money from Mr. Qaddafi, and we are looking at that very closely to see if that money will be, could be, might be used to support terrorism in this country. At

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this point in time we can't say one way or another.

DR. WHEELON: What about the parties responsible for the bomb in the Capitol?

MR. KLEIN: We believe there were two cells. I think I will take you back in time a little bit to the old Weather Underground, going back to the SDS days on the campuses, and as I say often, I think most of them when they turned 40 became—their motives changed, yet there was a small, dedicated group of people that were involved in criminal acts and supported revolution in this country.

One of those groups was, one of those cells which moved up and down the east coast from Boston to Washington to Baltimore consisted of individuals that were involved in the robbery of the armored car in Nyack, New York, in 1981. Marilyn Jean Buck's name comes to mind. I don't know if you have heard her name mentioned before. And in her house during the searches that followed her arrest we did find detailed plans in a folder calling for action on various facilities around the country, which included this building, very detailed sketches of where to place a bomb, and also other targets such as some facilities at Annapolis, et cetera.

Most of their bombings occurred in the evening hours. They were preceded by a telephone call. They

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did not want to cause any grave, I don't believe, any grave physical damage. Our terrorists as opposed to the ones you see in Europe seem to want to make a statement, and they believe, I think, that the taking of human life detracts from their cause, which is to gather the American people behind them in a socialist communist revolution at some point in time, and that turns people off.

So, most of those individuals also currently have been arrested during the past few years and are currently in jail, and we don't believe that those two cells function or exist any more. I would be surprised to see another bombing like that occur in D.C., but then again there are people out there who could take up the flag.

CHAIRMAN ROGERS: I think the FBI does a fine job. I am sure you do all you can do prevent it. This is a little bit beyond our jurisdiction. Go ahead. We are having a bit of a time problem here.

MR. KLEIN: I have been working with NASA since the explosion by offering FBI laboratory services to NASA, and we were in receipt of some hairs and fibers on February 2nd from NASA that we have examined in the FBI laboratories, and the exams have been completed, and we do have human hair, Negro hair, Oriental hair, and

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hair from two different brown-haired Caucasians, and what is interesting, according to the laboratory, is that there were no signs of heat damage to any of the hair, which was surprising. The hair came from face seals, fragments of helmets, and helmet liners, and headrests.

There have been a number of threats and investigations that we have become involved in since the Shuttle went down. An engineer in a consulting firm for Rockwell Engineering out in California advised the FBI that he believed the Shuttle was hit by a laser, that his examination of the frame by frame stills—I don't know how he recorded it—showed that brown smoke was

emitted as opposed to white smoke, which would mean that—the white smoke would indicate a fuel problem, and brown smoke would indicate possibly a laser.

And we have interviewed the chief engineer at Hughes Aircraft, and he believes the theory is plausible but not probable. The FBI agent that interviewed this person believes he is sincere, and not a flake, or not demonstrating any emotional instability, but it is just a theory which we have passed on to NASA.

CHAIRMAN ROGERS: Is that being taken seriously at NASA?

MR. MOORE: All possibilities are being looked

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at. Yes, sir. No possibilities are being excluded.

GENERAL KUTYNA: I tell you, if that guy has got a weapon like that, I would like to put him on my project.

(General laughter.)

MR. KLEIN: There have also been a number of other instances where people have come forward and said they believed that this happened or that happened, and we are following every one of those leads out. At this point in time we pass it on, because we don't have the scientific expertise to say whether their theories are correct or not correct. We just pass it on to NASA and let them be the judge. And so I guess what I wanted to say is, there is no terrorist threat, no information that there is, or was or is a group that planned, organized, or executed any action at Cape Canaveral.

CHAIRMAN ROGERS: And I am sure you are watching very carefully to see that it doesn't happen in the future.

MR. KLEIN: We are watching very carefully.

CHAIRMAN ROGERS: Thank you very much.

Any questions?

DR. WALKER: Should we handle these theories with our own discretion? That is, if an individual who

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is someone who we know, and if the person has a theory, we handle that in a way that we think best, but if there is someone that you don't know, do you have any advice in that regard?

MR. KLEIN: I am sorry?

DR. WALKER: If someone contacts us personally and has a theory about the explosion, do you have any advice to offer us as to how we should handle that?

MR. KLEIN: Yes. If you could pass it on to either us or NASA, we are conducting these kinds of interviews jointly, so we will take it for possible sabotage violation, and somebody from NASA.

MR. MOORE: It is kind of my repository right now for all of these kinds of things, and we are going to run every one of them down. It is our intention right now.

MR. JONES: If you get some information of that type, either refer it to Jess's office or Stan's, and then while you are down at the Cape area, again, my office, and we will tie this thing together, because I see the highest degree of cooperation between all of the government agencies on this particular operation. No one is worried about turf. Everybody is working together. The Bureau and NASA are working together. And we will handle any of those kinds of things that you

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receive.

CHAIRMAN ROGERS: Okay. Thank you very much.

MR. MOORE: Let me just add one little more comment to what Mr. Walker said. Don't send it to me by mail. Hand it to me personally or to Marv personally, because I don't think we want to distribute that information in the mail to my office.

MR. THOMPSON: Mr. Chairman, if you want to give it to me, I can take care of getting it to Jess.

CHAIRMAN ROGERS: Okay. That's fine.

MR. MOORE: We have completed all of our discussions we had planned this morning with you, Mr. Chairman.

CHAIRMAN ROGERS: Why don't you come up here, Jess? Can you tell us now anything beyond what we have read in the newspapers?

MR. MOORE: I can tell you that about 670 or 80 milliseconds after launch we saw a short puff of black smoke come out of the righthand solid. We cannot see the exact origin of it. I have had reports that it is all the way down to where the attached unit from the external tank attaches the solid all the way to some two feet or so above that, which could or could not include the joint, which some people are focusing in on right now.

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I can also tell you that we did see some——

DR. WHEELON: Wait a minute. Excuse me. Within a second of launch?

MR. MOORE: It was less than a second. What was it, Arnie, 680 milliseconds?

MR. ALDRICH: Six-tenths of a second from ignition of the solid, and that is the precise time that the solid comes to full internal pressure.

DR. WALKER: That was before liftoff?

MR. MOORE: No. Let me go through the sequence again. The sequence is, the main engine starts at about 6.3 seconds before actual liftoff from the launchpad. You bring the engines up to near full throttle. Then you send a mission signal to the solids from the GPC, the General Purpose Computer on board. The solids ignite, and that is what we call liftoff. At about 670 or 80——

DR. WALKER: It doesn't actually lift off then?

MR. MOORE: Well, it takes some pressure to lift it off of the launch support platform.

DR. WALKER: How long does it take the solids to build up?

MR. MOORE: We don't know precisely. That is what we are looking at on

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these films, to try to get the precise time that the system unlatched itself from the pad. There will be some variation in there. It is not exactly precise.

This happened about 680 or so milliseconds. It finished at about two seconds or so. And it settled down. We don't see it any more. And the orientation of it on the one set of films that we have looked at is kind of behind the solid. It is kind of obscure. We see the smoke come out, but we do not see, we cannot see on at least the initial films we have looked at exactly the origin of it.

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DR. FEYNMAN: Could you remind me whether the oxygen line that comes down along the ET tank and those trays of electrical cables, is it in that area, or is it somewhere else?

MR. MOORE: They are on the right side of the Shuttle. The whole cable tray that you saw yesterday on the bottom and so forth are on the right side of the Shuttle.

DR. FEYNMAN: In a place that could possibly be related?

MR. MOORE: Possibly. The other question that was asked, I guess, Bud asked me earlier about the range destruct and so forth, the linear shape charge that goes up and down the solid, all the way up to approximately the area below the thrustum where the parachutes are stored up, down the segments, up until some 18 or 24 inches above the attach going onto the ET, the linear charge that goes up the solids and each side of the solid.

DR. FEYNMAN: How close is that to the electrical tray?

MR. MOORE: That in terms of distance, I would say a few feet would be my guess to the electrical tray that carries that cable.

Arnie, do you have a better idea?

MR. ALDRICH: You are talking about the shaped charge

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on the solid?

MR. MOORE: Yes, where it is relative to the tray on the external tank.

MR. ALDRICH: I don't think we could comment in terms of the clocking radially around the solid with respect to that. I am not sure that I know.

MR. MOORE: It is feet. I mean, it is not very close. I mean, there's the big solids themselves and then there is the distance.

DR. FEYNMAN: I am confused. I thought there was also a destruct tray along the ET.

MR. MOORE: There is.

DR. FEYNMAN: That one, is that close to the electrical line?

MR. MOORE: That is close to the cable tray. There is a cable tray that goes up through the thing, and there is an destruct package down on the top of the tank and one down on the bottom.

MR. ALDRICH: There is a destruct tank in the middle under the orbiter. However, you are asking for information that we know. We have found pieces of both of those cord, and neither has been fired on the external tank.

DR. FEYNMAN: You have found that?

MR. MOORE: Yes, we found that floating.

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CHAIRMAN ROGERS: Why don't we let him go ahead and finish, and then we will ask questions.

MR. MOORE: Then that finished at about two seconds, and we did not observe or we have not observed in any of the photography we have looked at up until that period of time, and then there was an unusual event, a forced event that occurred around, what, 40 seconds or so is what the time line chart indicated. And you have a better analysis of that than I have. Why don't you come up here and discuss that?

MR. ALDRICH: This would be a correlation.

MR. MOORE: Is that the time line? We haven't seen that?

DR. RIDE: Yes.

MR. ALDRICH: Without trying to read this, the telemetry events do show indications of happenings as the flame occurs on the solid rocket boosters in flight. And Jess, I can't recall the discussion we had at Marshall about the possibility of some dynamic change at 40 seconds. There was such a discussion.

MR. MOORE: The gimbal angles on the solids moved at about that period of time, as I recall. And they moved about two degrees, and maybe Sally has got it listed in there.

DR. RIDE: I think it is in there. I think

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there may be something at around 40 seconds.

CHAIRMAN ROGERS: Sally, why don't you come up here, too?

DR. COVERT: Sally, what was the altitude, then, 35,000?

DR. RIDE: Probably. I am not sure.

MR. MOORE: The events that we are looking at could be associated with some winds. It could be associated with some loadings and so forth. We send up weather balloons at various times before launch, and the last balloon we did was about two and a half hours.

That balloon data is then sent down to Houston for computing the wing loadings and system loadings on the orbiter at that point in time, and early on, I guess, about 20 hours or 15 hours before launch we had seen some wind changes, but they were within the spec of the envelope and so forth.

DR. WHEELON: At 40 seconds you are at Mach .85 and 16,000 feet going 950 feet per second at 65 percent throttle thrust with a Q of 605.

MR. MOORE: Go ahead, Arnie.

MR. ALDRICH: Looking at this closely, the first thing that I recall started about 60 seconds, and that is when you first began to see the flame on the exterior photos as well, and at that time the thrust

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within the solid rocket boosters is building gradually, normally, and they see the lefthand booster, which is the one we suspect performed normally, build along the normal thrust profile, which is a slight increase.

At that time they do not see the righthand rocket. It attempts to stay down, and is affected in some way and does not build.

VICE CHAIRMAN ARMSTRONG: And what is the sensor, Arnie?

MR. ALDRICH: I believe the thrust level from the solids is a derived calculation based upon the performance of the stack. Coupled with that, we do have thrust chamber pressure from the solids, and the chamber pressure on the righthand solid does not build, and it does on the right. So both the calculated thrust level and the chamber pressure correlates as not increasing on the righthand side as this bright spot appears on the left, it does appear.

As I say, we haven't been down to Florida in two days, and I haven't seen what Sally has, but further up the cycle, close to the event, as you approach the 70-second time frame, you do see a kick of order of magnitude of two degrees in the gimbals on both solids, both pitch and yaw, or what do you call it, rock and tilt on both solids, and you see a similar minor

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adjustment on the main engines as they account for some dynamics, and that is what Jess was describing.

My recollection is, that is higher up, closer to the event.

MR. MOORE: You could be right, because I haven't physically sat down and seen that data in several days, so the timing that you probably have, Sally, is probably the most accurate timing that we have at this point in time.

MR. ALDRICH: And then I recall at about the time that the visual analysis predicts the hydrogen tank first beginning to rupture, where you see hydrogen fuel come out of the hydrogen tank, and on the order of a second later you can see the rupture occur and fuel come out of the oxygen tank. You can also see the main engines which are being fed by these liquids. The main

engine chamber pressure decreases slightly because of the lack of feed from the propellant system.

I think those are the only indications that I can remember.

CHAIRMAN ROGERS: Would you mind going over what you just said about the six-tenths of a second, and then the remainder of it, because I am not quite clear.

MR. ALDRICH: Yes, sir. At ignition T zero on the Shuttle launch is defined as the time we light the

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solids as the main engines are lighted at minus 6 seconds, so T zero is essentially the solid rocket ignition. The time of release is slightly different than that, almost precisely the same time. At six-tenths of a second the thrust within the solid rockets builds to its maximum pressure, its normal flight pressure, and in the photos that we have——

CHAIRMAN ROGERS: Now, is that when the rocket takes off and leaves the ground at that point, six-tenths of a second?

DR. RIDE: That is sort of hard to define, because as soon as it ignites, it starts trying to get off, and the main engines are already running, and so they are trying to get it off, so the whole process, it is hard to say when it actually is no longer in contact.

CHAIRMAN ROGERS: But obviously at about that time.

MR. MOORE: Yes, at about that time. I think that is correct.

DR. WALKER: It is not restrained, it is just sitting there?

MR. ALDRICH: No, it has bolts that hold it up.

CHAIRMAN ROGERS: It rips them out?

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MR. MOORE: They are blown. You want to hold down the launch to make sure that you do a validation check on the main engines. In other words, we want to bring the main engines up to make sure all three are running and you have full redundancy in each engine, and that is a launch commit criteria.

DR. WALKER: Once you start the solids, then you release any constraints?

MR. MOORE: That is correct.

DR. WHEELON: And isn't it true you probably start to lift off before you have the maximum pressure?

MR. ALDRICH: I believe so.

DR. WHEELON: So probably at about .4 you start to move.

MR. MOORE: I think we will be able to tell that when we go back and look at some of the high-speed photography.

MR. ALDRICH: That is my point. I didn't want to imply that that fact can in fact be determined. I just don't know precisely.

CHAIRMAN ROGERS: But at about that time?

MR. ALDRICH: At about that time, that is what happened, and we can tell you precisely.

CHAIRMAN ROGERS: But then you see something?

MR. ALDRICH: Let me say one more thing about

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blowing the bolts, because that is an important factor. We send the signal to ignite the rockets. At the time it is called T zero, or launch. We also send the signal to fire the bolts, either at precisely that time or within milliseconds of it. But before that, the rockets are bolted solidly,

and they see a loading, because the Space Shuttle main engines are cantilevered off the solid rockets.

We light them at minus 6 seconds. The whole vehicle stack actually swings forward and back, and the timing of when you will commit to light—to release and light the solids is timed such that you load that stack this way and back from the main engine ignition, and about the time it is vertical again, that is the 6-second period, and the ignition of the solid, so these boosters give a strong bending load during that 6 seconds up to the time of ignition.

Now, your question, what did we see? At six-tenths of a second after ignition the solid rocket pressure is essentially up to flight level. On the righthand solid in the same area that 60 seconds later you see the flame you see a puff of black smoke, a big puff.

DR. WHEELON: How big?

MR. MOORE: I would say it was a couple of

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feet in diameter.

MR. ALDRICH: I would say it was about the size of a main engine bell.

MR. RUMMEL: Would a burning seal produce that black smoke?

MR. ALDRICH: That is under discussion and investigation. The comment I have heard, and I am not an expert, and we don't know the answer yet, is that more likely the grease that is used around the seals if it were burned could cause a smoke of that characteristic. It is a very dark smoke. Most of the flame and smoke you see is light-colored, and this is a very black puff.

DR. FEYNMAN: Possibly from graphite in the liners or something?

CHAIRMAN ROGERS: Why don't we go ahead and let him tell the rest of it? Then we can come back with questions.

DR. WHEELON: Have you ever seen such a puff before?

MR. ALDRICH: We are researching all flight films, as we do on every flight. No one that has done that viewing the films in the past recalls seeing it before. We are going back to be sure that we haven't missed anything.

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DR. WHEELON: So it could have been there?

MR. ALDRICH: It could have been there before. No one recalls seeing it.

CHAIRMAN ROGERS: Anyway, now you have got the puff of smoke, roughly two feet.

DR. WHEELON: Two to ten.

MR. ALDRICH: I would say two by ten.

CHAIRMAN ROGERS: Then what happens?

MR. ALDRICH: That seems to persist for 2.9 seconds and stop, because as the stack rises you can see the booster rising through the puff, and it disappears, and there apparently is no evidence of further black smoke or any other condition from that area until the time 60 seconds later that you see this little finger of flame approximately, and by approximately it could be a number of feet apart along the solid either radially or upwards or downwards. We haven't correlated the blownup photos well enough to say how close the two things are together or whether they coincide in their location. They are from that place on the solid rocket booster in a general sense, however.

CHAIRMAN ROGERS: If you get to that point where those things are pretty accurately determined visually, will you then be able to isolate the area or could that condition be related to a lot of other

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things? In other words, does that help you isolate the cause?

MR. MOORE: I think it helps us isolate the cause, but contributing factors, it could be a while to understand what the contributing factors were. The other thing, let me point out, as the cases are put together, I don't know if they were shown yesterday, I don't recall, there are metal pins about that big in diameter and about that long that are inserted around the case. There is a cork lining put around those pins. There is a band and a cork lining around it. And then there is grease put along the outside of that entire segment out there, and so I have heard some comments that the smoke goes up, and others have said the smoke goes out, and so forth, so we don't have that exact origin, but we have really got an area to go and concentrate on.

MR. ALDRICH: I would also like to recall Don's presentation this morning. We of course do discuss scenarios as we go along our path, and there is every possibility that this black smoke is from something completely different from the thing that caused the flame or the thing that caused the accident.

DR. FEYNMAN: This is what we would have called an anomaly? Is that right?

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MR. ALDRICH: It is an anomaly unless we find a film where we have seen one just like it.

VICE CHAIRMAN ARMSTRONG: These incidents all fit very well within Don's anomaly tree.

MR. MOORE: There is no question, and we have got the people putting all these anomaly trees together. The way you eliminate the anomalies is, you validate them, and in most cases you need to go through testing or to have previous test data to cross off this as an anomaly, and that is what we are building now, is one giant system anomaly tree, to go through and try to cross off all the things on that tree that we can validate as we test or through other procedures.

MR. ACHESON: How many seconds apart did you say the black smoke and the first visible plume of flame were?

MR. ALDRICH: Sixty. This is fairly close to the ignition and liftoff. Now, when we do the refinement of the photography, it may be that a much smaller flame is visible a good bit earlier than that, but what we have been able to see to date, the flame starts about 60 seconds.

GENERAL KUTYNA: Arnie, I didn't press Judd yesterday, but he talked about erosion, post-flight inspection to find out about erosion of these joints.

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How much of a problem has that been? Have you got it solved yet, do you think, or do you still think it is sort of an open item?

MR. ALDRICH: It has been in discussion in the program at least during the last year. There are two O-rings around the seal, and on about five, perhaps half a dozen STS flights, on each flight there are six seal areas, three segments, three breaks in each of two solids. There are six seam sets that see the flight experience each time we fly, and on five flights one or at least on one flight two of those seams saw some amount of erosion on the inner O-ring.

That is believed to have occurred at ignition. When you first pressurize, you get some blow-by of the first O-ring, but in every case they have seen it stop by the second O-ring. However, that blow-by has caused what they consider to be within an allowable tolerance the amount of degradation called erosion of that O-ring, and then sooting from that degradation occurred between the two O-ring seals.

DR. WALKER: But not beyond the second O-ring?

MR. ALDRICH: Not beyond the second one.

MR. SUTTER: Are these O-rings replaced?

MR. ALDRICH: They are brand new every

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flight. There is an interesting factor related here that you should understand. Of all of those instances that occurred, the worst one that we have seen from torndown rockets is the teardown after the launch we did a year ago, also in cold weather. There was more erosion and more blown-by material between the O-rings on that one than on any of the other four or five.

DR. WALKER: Did that one burn two rings?

MR. MOORE: Yes, that was one of the flights—that was the flight where the temperatures were low.

MR. ALDRICH: Everything that I know about the certification of this seal, and this is being worked in much more depth at the Marshall Center than anything that has been reported to our board, is that the certification tests run on that joint show that the seal would be somewhat more stiff, but completely adequate for sealing at all temperatures in the ranges. There was never any intention that the system couldn't be launched in freezing conditions, particularly at 32 degrees. And it is my belief that we expect this Viton O-ring to perform essentially at much lower than that.

MR. SUTTER: They said when these pieces are brought in they are somewhat warped, but they may not be as good as a new piece.

MR. ALDRICH: What they said yesterday was,

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they ship these segments on rail cars after they pour them. They pour them vertically, and they are tested for roundness, and then they ship them horizontally all the way from Utah to Florida. They store them there, and sometimes when they go mate them, they found that they are not completely circular any more, and they have a variety of hang points so that if it is not circular you can hang it up some other way for a little while and it will reform into a full circle, so that they do this check for roundness before they do the mate.

MR. SUTTER: Then maybe that builds some stresses, so again when the load changes it pops back to where it wanted to go.

MR. ALDRICH: Perhaps. We will find as we investigate this that the contractor and the Marshall Center have been very thorough in analyzing the characteristics like that in their buildup of this design and the way it is handled and the procedures. I agree with you. It seems like there is some flaw here we should be looking for, but I think many of the obvious ones will be shown to be completely researched.

CHAIRMAN ROGERS: One of the things that—when we ask questions, when we continue to ask questions, we are not really trying to point a finger. Really, this is an area of what can be done to make it

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safer. I mean, I don't think anybody, if there were any errors in judgment, God knows, nobody is going to expect everything to be perfect. I think that is one of the reasons that you will see that there is bound to be a lot of discussion on this point, and it may well be that we will decide to recommend that you not retrieve them.

I mean, I know—some people have told me that I thought knew what they were talking about that it might be just as cheap now to buy new ones when you consider all of the problems of recovery and rehabilitation and all of that. Anyway, that is certainly one problem, and there will be continued interest by the public.

The other thing I wanted to mention to you, I thought it was a little unfortunate in the paper this morning that they said that, and I don't think you really said that, that you had excluded the possibility that weather had any effect. I mean, I think weather is also going to be considered very actively by a whole lot of people, and if at the end of the road you decide or we decide to exclude it, fine, but if it appears you have excluded that to begin with, particularly because apparently Rockwell did call and gave you a warning which you considered and decided that it was okay to go ahead, suppose that judgment was wrong. Nobody is going

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to blame anybody. I mean, somebody has to make those decisions, and you were all there and made the decision.

MR. MOORE: We made the decision on the basis of the data that was available. Let me say one other thing about weather which didn't come out. Weather is by no means being excluded. My opinion is that that might be a very major factor. During the month of January, I believe, before this launch there was also something like seven inches of rain down at Kennedy. And so that is another element that really has to be looked at, that the associated moisture may have had something to do with it.

CHAIRMAN ROGERS: And again, the testimony yesterday was that one pad had a rain cover and the other didn't. I mean, those things are of interest.

MR. MOORE: Absolutely, and the differences between Pad A and Pad B, those are being looked at as well. They were looked at before this particular launch. But whether we have put everything in place, we have got to go back and scour that from top to bottom to see, although we did use the same mobile launch platform, I guess, as the previous launches have gone off, but there could be some kind of different loading effects at that particular pad.

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MR. WALKER: Is it possible to get a detailed drawing of those seams showing where the O-rings are?

MR. MOORE: Yes, sir.

MR. WALKER: If we could just have a sketch.

MR. MOORE: Yes, sir.

At some point in time, if that continues to be a very high probability area, as it is right now, we think it would be appropriate to bring our experts from the field to talk about this whole thing and give you a very detailed presentation on it. I think that is the best thing to do for the Commission, and we would certainly offer that, to get some more information about the seal area.

The other thing that we are looking at is the checking of those seals at the segment of the stack. There is a pressure port in the seal on the side of the case, that a person goes in and does a pressure check. And that pressure check is done to ensure the integrity of the O-rings, and the pressure check is up to 200 psi, and then it is backed down from 200 psi and brought back up and held at 50 psi.

VICE CHAIRMAN ARMSTRONG: Is that a plug on the nozzle?

MR. MOORE: It's a plug on the side.

MR. ALDRICH: You blow one O-ring up and you

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check it?

MR. MOORE: You check the integrity of the O-rings prior to launch, and that is done several days or a week or so before.

VICE CHAIRMAN ARMSTRONG: But the pressure would be outside-in in the case of the inside O-ring, then, right?

MR. ALDRICH: That's right. And that is presumably what sets up the position for initial blow-by of the first O-ring. You do the pressure checks and it moves the upstream, the inboard O-ring, upward. And then when you ignite the engine many days later, the first pressure surge hits the O-ring out of the sealed-down position.

And that is what has been alleged to be the most probably cause of the blow-by, in addition to the dynamics of the joint.

MR. MOORE: It should seat the other one.

VICE CHAIRMAN ARMSTRONG: It should seat the other one, but it could act like a flapper valve, where you would be pressurizing.

MR. MOORE: That's right.

MR. RUMMEL: In the main joint, I think near the attached fitting, in addition to an O-ring you have a seal that was somehow inserted, I think in the seam.

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Is that inserted after assembly?

MR. MOORE: There is a putty that is laid up on the segments there. I think it's a zinc chromate putty that is laid up on the segments as they fit together prior to the assembly, and then the segment is put down with a male section of the segment down into the female section of the segment.

And then that fits around, the case segment fits around, and you've got putty around this area, and then there are holes, about 175 or 180 holes, about an inch in diameter, that—circumferentially around this joint. And each of those holes is then plugged with this piece of steel, round steel about an inch in diameter and about two inches long. It is plugged in each of these holes and there is a little clip put over each one of those pins from the outside to hold it, a retaining clip.

And they are all completed and then there is a band that is put on the outside of that, and that band is tightened around the segment, and then there is a cork layer put over the outside of that, and then you finally put some coating on the outside of it, some of the white coating. I forget the kind of material.

DR. FEYNMAN: I think it's an epoxy.

MR. MOORE: That's probably what it is. And

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then there's a layer of grease put around the outside segments, each of these segments, to form a water repellent seal. And that is hand laid up, to keep any moisture from getting in that seal.

So there is—the O-rings are laid in there and this thing is put together. So when it comes together, those two O-rings kind of are laying around this way.

MR. RUMMEL: How is the putty inspected afterwards for voids?

MR. MOORE: There are closeout photographs made and an inspector goes off and looks at those things.

MR. RUMMEL: This is purely visually or X-ray?

MR. MOORE: Most of it is visually looking at how the putty is laid up. And we do have closeout photos that we saw before we left the Cape of some of the segments.

Now, we don't know whether we've got complete closeout photos. We're supposed to have complete closeout photos of all the segments. But Arnie and I only saw a few of the photographs before we left Kennedy the other day, of some of the segments.

MR. RUMMEL: Has there been any evidence that

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that particular sealant, from past launches, was a problem?

MR. MOORE: We haven't observed much as far as the putty was concerned. There was a putty that was used, I think a different kind of putty that was used for the first seven or eight launches, I think, and the manufacturer of that putty went out of business. And I think the supplier put another putty in, the same kind, supposedly the same kind of putty, the zinc chromate putty.

But the characteristics, what is supposed to be the same, we don't really have any evidence of anything from a putty standpoint.

MR. RUMMEL: Excuse me. If I recall correctly, yesterday it was said that this was not the first launch with the new putty.

MR. MOORE: No, it has been since STS-10, which was in '83, I think. And I'm having a hard time remembering. It was several years ago and there have been a lot of flights with this particular putty in it, this kind of putty.

DR. FEYNMAN: Would it be fair to say that when you were discussing it, the blowby by the O-ring, you said we don't expect it on the other O-ring. On the other hand, you didn't expect it on the first O-ring, that

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if the second O-ring gives just a little bit when the first one is giving, that that is a very much more serious circumstance, because now the flow has begun. That is, if the second O-ring holds, if there is a little bit of blowby, the pressure builds up and the flow stops, and then it settles back and seals. But if the second one opens just a little bit, there is a flow of very hot burning fuel product.

MR. MOORE: That's very true.

DR. FEYNMAN: And that can eat away the O-rings and so forth, and so the second one is very much more critical if it doesn't hold when the first one isn't holding; is that true?

MR. MOORE: I think that is a true statement. There has been no evidence to my knowledge of erosion of the secondary. There have been some burnishing, some discoloration, and some soot that was deposited between the first one and the second one, was what I think our flight experience had said.

And the testing of these O-rings and so forth, there was a lot of lab tests that were done down at Marshall in the laboratories on these O-rings, and I think some out at Thiokol of the O-rings, and their analysis indicated that as long as you don't, as you say, have a complete gas path through this whole cycle

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of O-rings, that you are okay.

I think these O-rings are Viton rubber and I think they are probably the same kind of O-ring that is used in the Titan segment, where you guys have only one O-ring, I think, in Titan, Don, as I recall.

GENERAL KUTYNA: Let me add to your comment, however. That is how we lost the inertial upper stage, was there was leakage and once it got a path, then it burns like an acetylene torch, once it gets that continuous path. If it leaks a little bit, you just get soot. As soon as you get that flow, it just goes right through.

DR. FEYNMAN: I have a picture of that seal in cross-section here, if anybody wants to see it.

VICE CHAIRMAN ARMSTRONG: Is it determined yet that the two degree nozzle switch would be consistent with—would the direction of the two degree nozzle change be consistent with the moment of correction that would be expected if the kind of thing that Professor Feynman is talking about actually made a nozzle——

MR. ALDRICH: That is being calculated and it is conceivably reasonable, based upon the first look.

DR. RIDE: Do you have data from the RGA's, the gyros, that would show you this?

MR. MOORE: Yes, what the RGA's were doing and

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so forth.

VICE CHAIRMAN ARMSTRONG: In addition to that question, have you ever seen nozzle excursions of this magnitude as a result of a breach?

MR. MOORE: My understanding, based upon the question—Arnie, you probably have more detail. My understanding is the upper regime——

MR. ALDRICH: An excursion like this we would consider highly unusual. We don't predict, from what we considered the winds to be that day and the wind speeds, that there was such a wind event. If the flight had been completely normal and all you had seen was these booster nozzles kick, it would probably not have raised many considerations or concerns that something peculiar happened.

MR. MOORE: They have what, a three degree limit of the motion of the nozzles?

DR. WALKER: I thought I saw eight degrees.

MR. MOORE: I didn't realize it was that much, but we have seen up to a couple of degrees. The question we are addressing is the rate. You've got to look at the rate of how fast this thing is responding, and so forth, and I tried to get all of that correlated and we've just not looked at that.

But the RGA's, as Sally mentioned, and any

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other kind of acceleration—one of the people reported to us last week when we were down in Florida—I guess it was the week before last—that there was some noticing of some kind of fish-tailing of the system going up. We saw some cargo bay, maybe the cargo bay area, accelerations that were anomalous, and we are running that down.

But I don't have any more data on that.

DR. RIDE: Do you have any data from any accelerometers? Is there anything like that in the payload area?

MR. ALDRICH: I think we've got some data from TDRS.

MR. MOORE: We have that data impounded, and we're going to try to look at that acceleration data to correlate the events. That is the hardest job, is to try to get the time line in sequence that everybody's data will sign up to, because you could be off a few milliseconds and miss events, just like you pointed out very clearly. And that is what we're so concerned about.

GENERAL KUTYNA: Was the IUS guidance on at this time?

MR. MOORE: I think IUS guidance was on, I believe, although I'm not sure. We have a lot of IUS

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parameters we monitor.

GENERAL KUTYNA: We initialize on the ground, but I don't know whether the record came back or not.

DR. RIDE: It should. It comes down on the down link.

CHAIRMAN ROGERS: I feel awfully sorry for this gentleman who's trying to record what we're saying.

(Laughter.)

MR. MOORE: Don, to answer your question, we are looking at all that data. It was impounded out at Sunnyvale. All of that cargo information is very important to us.

MR. ACHESON: Could I ask a question? Have there been any interesting discoveries and material collected from the ocean that we have not seen in the press yet?

MR. MOORE: I would say there are some observations that have been made on some preliminary pieces. We have recovered the orbiter speed brakes, which are on the rear section, and the left speed brake I guess is just like it came out of the factory and the right speed brake has got a lot of particles, it looks like, were embedded in it, and so forth.

But if you look at the flow and everything, it

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looks like the wind would have shielded it. So we're not sure what all that means. But it is just kind of a—those are some of the black and white differences that we're beginning to see.

And we're got metallurgists and others going back and looking at those samples to tell us the kinds of materials we see. We have a very—that we did not show to the public yesterday, we have a very elaborate layout that the National Transportation Safety Board has provided and has assisted us in doing.

We have got a facility down at Kennedy that has been laid out to assemble a lot of the pieces. And I guess the Navy brought in, with the assistance of the Air Force, a tremendous tent or a portable building that they are putting up, of some 250 feet long by about 75 or 80 feet wide. And we're going to use that as kind of a layout area.

And we would invite members of the Commission——

MR. HOTZ: Is this the jigsaw grid?

MR. MOORE: Yes.

MR. ALDRICH: We're actually building a frame for the orbiter to be constructed on. There's a very interesting characteristic on the orbiter. Every tile on the orbiter has a serial number, so when they find a

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piece of the orbiter it is not one of these, let's just see where it goes. We can look in the tile records and they know exactly where it goes.

MR. WALKER: Each tile is different, basically?

MR. MOORE: Yes, sir.

MR. RUMMEL: I have a somewhat different question. According to what I read in the press and partly from what I heard today, there was generally a lack of heat evidence. If that's true, I just don't understand. Can you explain that?

MR. MOORE: We can't explain that at this point in time.

MR. ALDRICH: One of the places that we think the largest amount of the explosion was focused—I don't say we think, but what Marshall reported in their early analysis showed, the largest part of the explosion occurred where the oxygen comes out of the oxygen tank in the inner tank area.

They found pieces of the inner tank, which is the cavity between the two tanks and the external tank. Some parts of it are melted and burned dramatically and very blackened, and other parts are as pristine as new metal.

MR. RUMMEL: From the same canopy?

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MR. ALDRICH: Yes.

DR. WHEELON: I have a request. The time lines that Sally has there, I wonder if we could get copies of that.

DR. RIDE: Actually, I am not sure that this should be considered an official time line.

MR. MOORE: We can take an action to provide it. I mean, Sally has it and she can distribute it.

CHAIRMAN ROGERS: My only question about that is whether we should have this until we're a little more sure of it.

DR. RIDE: I guess I would feel better about having somebody, one of the working groups down there, not put out an official one, but just because there really is the problem of time correlating all of the data, and this has their first guesses at the time correlation down to milliseconds. But those things could change a little bit and a couple of things could get added.

MR. MOORE: I would say that we could pull together a preliminary events sequence by very early next week and provide it to the Commission, if that would be acceptable.

CHAIRMAN ROGERS: I guess maybe I'm sort of conscious of possible leaks. Not that I think anybody

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would leak it, but if it hits the press and we have it, then everybody's going to say, well, somebody in the Commission must have leaked it.

And I'm not sure that at the moment having any of these documents is particularly valuable to us. We've got them all anyway in due time, and certainly, if you're going to correct it later on—I mean, there's no reason why we can't all look at them now, and Sally could make it available to look at it.

But I just think that the fewer documents we have in our own possession until we get ready to do something with it, the better off we are.

Dick?

DR. FEYNMAN: I think we've just learned from the General's report that the proper way to do it is not to go flying at various possibilities when the data is only preliminary. You think you're going to gain something by that.

That is what I learned. I thought I was gaining something. That is what I thought I was going to do. But when I saw what he suggested, I see that the proper way, I think, is to wait until that. Do it as fast as we can, we might come down and look and suggest something that you could calculate on the time, or a suggestion of something else or something like that, but

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not to try to make the decisions and analyses as we're going along.

We have the same thing in physics experiments. The theorists sit on top of the experiment and as the data starts to come out, when it still has errors in it that haven't been checked out, they are already making theories to explain bumps in the curve which turn out to be nothing, and it's just a complete waste of effort and confusion and everything else.

DR. RIDE: I'm a little bit sensitive to this because I have good sources and over the last week I've heard a lot of things from excellent sources that are wrong, like for example the balloon, the balloon being shot down, and that's not right.

MR. MOORE: Let me say also, Sally, that we made a very special effort to involve as many members of the astronaut corps down at Houston as we can. But your point about data and the

sources, we've heard all kinds of things, too, that sounded very promising a week ago, and now they totally don't have any relevance.

DR. RIDE: I'm very sensitive to the right approaches: Collect the data, look at all the data, and then once you've looked at it all, come to your conclusions.

MR. MOORE: And have this devil's advocate

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team set off and make sure we haven't forgotten something over here, a bunch of wild and crazy ideas.

CHAIRMAN ROGERS: I think you're right. I think we are entitled to that. Let's not think that because we would—let's not take it because we would dry up Sally's sources.

(Laughter.)

DR. WHEELON: But we will have an official time line?

MR. MOORE: Sure.

CHAIRMAN ROGERS: Unless there are any other immediate questions, I have a couple of questions on procedure. Any other questions?

VICE CHAIRMAN ARMSTRONG: I guess, was that that—did they complete their list of things that we ought to hear about and we haven't heard about?

MR. ALDRICH: There's at least one other thing that shows on the telemetry. The telemetry shows, from the gyros that Sally was talking about—there are gyros in each solid, and it shows that in the last second before the loss of data the right solid rocket deviates from the stack, and the left one and the orbiter stay together in a conformed trajectory.

VICE CHAIRMAN ARMSTRONG: When was that?

MR. ALDRICH: Within the last half second before loss of data.

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MR. RUMMEL: Would you repeat that?

MR. ALDRICH: The data on the telemetry has gyroscopic measurements from the orbiter and from each solid rocket, and in the last second of data before loss of data and the explosion, the right-hand rocket is shown to deviate away from the orbiter and the left-hand rocket, which means that it broke loose.

DR. FEYNMAN: Can I ask a dumb question? Do we know on which side which rocket is afterwards? Did they go like this and cross or do they look like they went that way?

MR. ALDRICH: The photo team will be able to pinpoint that precisely, and you've asked quite a few questions about what we see in the photography and, believe me, there's a lot of photography. On a normal mission, there's over 80 rolls of film and television tapes to go through, and this one even more has been brought in.

And to do it justice, the photo team will be able to spend hours with you showing things that are of interest.

DR. COVERT: Arnie, can I ask, in this rotation of this right-hand solid booster, does it look like it is pinned at the bottom or pinned at the top?

MR. ALDRICH: I did it wrong in my talk. It

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looks like the bottom comes free and it toes in at the top.

MR. MOORE: Let me make one more comment in response to Neal's question, and that is we think we have sonar data that has located the right-hand booster. And we're putting a lot of priority on getting that booster, and we think it is in about 12 or 1400 feet of water.

CHAIRMAN ROGERS: This is sort of interesting, Dick.

MR. MOORE: I say, I think we have data that pinpoints an area, that has located what we think is the right-hand solid rocket booster. We are spending a lot of effort trying to look at that on television to make sure that that is what we're looking at.

However, we believe it is going to take a fair amount of time to get that hardware back, because we don't want to do anything to that hardware during the retrieval process. That is absolutely fundamental to our investigation.

So Navy salvage is working with us. We've got experts down there kind of directing the whole salvage thing, and it's absolutely critical that we keep that hardware in as good a shape as we can in terms of the retrieval process.

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MR. ACHESON: How far offshore is that discovery?

MR. MOORE: It's 40 to 50 miles is the range, and it's about 12 to 1400 feet, is what was reported to us before we left the Cape.

MR. WALKER: Can you tell how many pieces it's in?

MR. MOORE: No, I have not seen any of that.

MR. ALDRICH: I had a report on that last night. It is in the Gulf Stream and the currents are high, and the first submersible they wanted to photograph or video observe it with was too lightweight to be able to operate in that condition, and they're bringing in heavier equipment.

And I was told that perhaps by Sunday or Monday we would have our first views of it.

MR. SUTTER: Do you have access to the submersibles that were used up in the Irish area, in that accident?

MR. MOORE: I don't know if they're the same vehicles. Mark, you probably know.

MR. JONES: We have access to the very same vehicles, and actually we have gone a step further. We also have the same people who were involved in the recovery of the Air India that went down off the coast

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of Ireland, as well as KAL-007. So we have the same people and equipment involved.

MR. MOORE: We've had offers from all kinds of people. AT&T doing underwater cabling offered their boats, and everybody has just offered all kinds of services to us.

MR. SUTTER: That job they did in the Irish Sea was tremendous. They got like to 6600 feet in two days. And with the salt water, the quicker you get it up the better.

CHAIRMAN ROGERS: Have you made an announcement about this?

MR. MOORE: No, sir, I have not announced it to the public.

CHAIRMAN ROGERS: I guess it's known.

MR. MOORE: No, I will tell you, I have a very strong feeling that I'm very nervous about releasing information to the public, because that immediately focuses the public on an area, and that may turn out to be the wrong area.

CHAIRMAN ROGERS: How do you correlate with NASA here in Washington, because they are giving out some information.

MR. MOORE: I have a public affairs person that is on my investigating—on my task force that is

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down at Kennedy. And when we're down there, there's a tremendous amount of pressure to release something. And what I have been trying to do is only release kind of status information of my activities, our group's activities, that we are continuing to meet here and we're continuing to meet there.

And I've got Bob Crippen and Bob Overmyer down at the Cape looking at all of the photographic data that comes in, and selectively they're releasing pieces of it. Chuck Hollins, my person, talks to the NASA public affairs person up here, Shirley Green, and supposedly maintains contact that way.

One of the things we need to do is to make sure that we have got a liaison here. That Jonathan and I have talked about, so that you aren't surprised about things that are coming out, and you will have your own announcements.

CHAIRMAN ROGERS: The only announcements we will make will be when we decide on meetings, where the meetings are going to be held. And if we have public meetings, we will announce the witnesses. But except for that, we won't make any announcements or comments at all.

I think we have all agreed to that, and if there is any thought of deviation, why, I will talk to

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all of the Commission members.

MR. MOORE: Sir, if it's okay with you, I thought I would set up someone with my task force to maintain a constant liaison with Jonathan down here, so the members of this Commission are not surprised by anything that our group deems important to release. We are totally trying to focus on any factual information, and not let the public speculate.

CHAIRMAN ROGERS: There was one other thing I noticed in the press about a little accident you had. Tell us a little about that.

MR. MOORE: That was a handling of one of the segment accidents. It was called an SRM incident report, and it was down at the Cape. And it was, Lockheed handles the unloading of segments off of rail cars into the proper facilities to begin the stacking, and they've got either two or four places that they pick all of these things up.

And there were load cells on the device that picked it up, and I guess there were a couple of failures of a load cell, and the segment came down and impacted too hard on the ground, and that segment was discarded from use.

It was scheduled, back when it occurred, for this particular stack. I think it was pulled out of the

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whole flow.

MR. JONES: This particular segment has a large handling ring on the top of it, and it was part of the processing procedures and we had to get this handling ring off of the top of it. And I say "we"; as Jess pointed out, it was Lockheed.

This particular device is held in by the very same pins that we referred to earlier, like the flight pins, and the handling ring that is on top of it weighs on the order of 11,000 pounds. So it is not something that you can come up and gently pull the pin out and lift it up.

And so, with the handling special device to pull the pins out, some of them were just simply wedged in by the brute force of this 11,000 pound device. The technician in charge invented a procedure which was a deviation from our published procedure, in that he attached the crane to the handling ring, asked for 11,000 pounds up, effectively zeroing it out, so he could pull the pins out.

He didn't clear the area around it. He got this lift up and they continued to pull pins. And that should have been, people should have backed away from it. At that particular time, the load cell indicator on the crane failed.

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The crane operator did not look over to take a look and see what kind of current he was using at that point, and we suspect that he probably put perhaps even 100,000 pounds up-lift on it. Then at that point it created some damage.

There was a loud bang, a noise, and it immediately shut down, and of course the investigation ensued and revealed all of these things I've just shared with you. A decision has not been made whether that segment was then flightworthy or not. One discussion said, no, when exposed to that kind of environment it probably ought to be discarded. Another one said, we think we can X-ray it, examine it, shim it out properly, and fly it.

But whatever the case was, we couldn't do it for this particular flight, 51-L. That segment was pulled out and set aside and its corresponding segment on the other side, because you have to have matched pairs for burning rates from the same amount of propellant.

So in fact, two new segments were put into this particular stack and flown as a result of that handling incident in I believe November.

MR. MOORE: It was in November. And the center director at Kennedy signed and approved the

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report on the 13th of December. That is when it was approved, and it was worked in concert with the headquarters safety office and the guidelines that come out of the headquarters and the headquarters office, not just bureaucratically signed off the paperwork on it.

CHAIRMAN ROGERS: Well, as you know——

MR. HOTZ: The incident was reported publicly?

MR. MOORE: Yes.

MR. HOTZ: It was made public at the time?

MR. MOORE: It was made public.

MR. MOORE: But you know, Lockheed is heavily involved in all the processing. They are our shuttle contractor, and we have criticized them for some of their operations. Their operations have improved some overall, but we have criticized them from time to time.

MR. SUTTER: It makes me a little nervous to

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hear that this was a procedure outside of the operating handbook, and if this incident hadn't have happened, it might have continued. I'm not criticizing. All I'm saying is——

MR. MOORE: I understand what you're saying. Let me say as far as that is concerned, we've got some people from Dryden Research Center, our flight research center out in California, that I asked several months ago to go down and spend some time at Kennedy and look at the overall process and flow down there and to take a look at it.

And I haven't had a report back from them, but we have had that concern for some time.

DR. WHEELON: I would ask, Mark, you've just described an incident having to do with the handling or the mishandling of the solid rocket segment. Is that unique or does that sort of thing happen regularly or now and again or often? And if so, what are the other incidents that have happened?

MR. JONES: We maintain an informal log, if you will, of any kind of incident, for whatever reason, whether something broke, whether a platform fell down, those kinds of things. Given that there are 16,000 people employed there, I would suggest that there are some number of incidents related to flight hardware.

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They are very minimal.

That is why we chose to do a very thorough investigation in this particular procedure, and that is why we were critical of the management from the immediate technician, who was the lead tech on the floor, through the higher levels of management that perhaps had not reviewed that procedure.

I cannot compare our incident rate with any other industry. I know that we have a certain rate of incidents, which I would figure out over a per hours worked, but I can't compare it to say it is more frequent or less frequent than an assembly plant.

DR. WHEELON: Excuse me. That wasn't the question. The question was, was this event that you described, was that unique? Have things like that happened before? And if so, do they happen every ten flights or every one flight, just in your place?

MR. JONES: This incident was unique in itself. This is the first time we have seen that kind of an incident in handling an SRB segment.

DR. WHEELON: Have you seen other incidents in handling SRB's? And I ask you because you're the director for RQ&A.

MR. JONES: I'm not aware of any other incident, and I can research the record, but I'm not

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aware of any other incident in which we have damaged flight hardware to the point where it wasn't usable. This was the only incident of that type.

DR. WHEELON: Would you review the records and give us an answer to that question?

MR. MOORE: Yes, sir.

MR. JONES: Yes, sir.

MR. MOORE: We track incident rates down at KSC and I know overall incident rates on flight hardware, as well as other kind of activities, and we have got that data and we will be happy to provide it to you.

DR. RIDE: Do you have any idea, just a feeling of whether the incident rates have gone up since Lockheed took over or whether they stayed about the same?

MR. MOORE: Sally, my general feeling on that is they went up initially and stayed up for some period of time, and they have been coming down pretty well over the past year. As a matter of fact, Dick Smith was telling that they have been giving a few special certificates to some people in certain areas of Lockheed for safety consciousness and improvements and so forth. And so I believe the incident rate is coming down, is what I have been told. And we have had that data

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tracked and we can provide that data to you.

Now, one of the other things that we have had a long-term concern about is safety at Thiokol, and we have had a couple of safety incidents out at Thiokol, where we've had pit fires and so forth.

DR. HOTZ: This is in Utah?

MR. MOORE: This is in Utah where the units are poured. And we have had some major concerns about that. We have had several boards look at that and that is one of the reasons that NASA initiated about a year ago a look at second sources for the solid rocket launcher—solid rocket boosters.

MR. WALKER: When they're poured, are they then shaped?

DR. WHEELON: Most solid rocket motors are poured and then machined.

MR. MOORE: That is the way these are done.

MR. COVERT: I wanted to follow up on something Bud said, and that is, every so often problems arise where some guy gets a good idea in the middle of a process and, with all the best intentions, implements it without adequate clearance.

And how often has this occurred down there?

MR. JONES: I think that, first of all, I can't give you a specific answer.

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MR. COVERT: Is it a recurring problem?

MR. JONES: It's not that much of a problem, because the intent—we have tried over the years to instill into what we call the test scheme the discipline to follow that procedure.

And all of the procedures, especially the hazardous procedures, are routinely reviewed periodically. In fact, the procedure this man invented was probably a pretty good procedure, except what he did was he didn't document it. It wasn't reviewed and he didn't use things, like giving a 64th of an inch up, which capability we have. Instead he said, give me 11,000 pounds and a measuring device.

So I think the procedure in and of itself was probably a good one, but very few instances of deviations from published procedure.

MR. COVERT: That is a partial answer. People are going to get good ideas. The kinds of people you want working for you are going to get good ideas, and they're going to want to see them implemented.

But there should be a discipline around to make sure that the procedures are reviewed by other people who are knowledgeable, and so they can say to them, don't use a load cell because we know they drift, that you ought to give dimensions, and so forth.

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So I guess, with a bright, inventive group like you have, there must be from time to time unauthorized improvements put into the processes, and then they either work well or they don't work well. And my question is, does that happen a half a dozen times a year or three times?

I have only heard of it twice since I have been down there; that is the kind of response I'm looking for.

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MR. JONES: I'm not sure how I could answer that. I simply can't give you a definitive answer.

MR. MOORE: I understand what you're driving at. Let me mention one other thing, Gene, that about a year and a half ago or a year or so ago the top management at Lockheed down there was replaced, and it was because of our criticism of the way they were paying a lot of attention to the overall management of this job, and I think during that period of time there were some of the kind of things you're talking about. I don't have the specific numbers.

Lockheed then brought in some aircraft experienced people who were experienced in running this overall thing, and I personally believe that has been tightened up pretty good. But again, I can't give you a precise set of numbers. We certainly have the statistics on that. We go through a very comprehensive award fee process with each of the contractors down at the Cape, as many of the other things, and we can go in and give you a much better quantitative feel than I can give you off the top of my head.

DR. COVERT: Well, it may not be relevant to this particular investigation that we are talking about, but I have seen other cases where, in fact, a good idea turned out to be the source of a problem.

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MR. MOORE: One point that ought to be kept in mind here is a majority of the people that Marv talked about are the same people that have been down there for a long time. When Rockwell did not win the Shuttle processing contract. Eighty-five percent, approximately, of those same people stayed on and simply worked for the new company. There were critical areas and certain areas you had a big void. In others you had a peak, and there was a general management concern down there for some period of time, and a lot of techs and so forth were there to do the procedures.

CHAIRMAN ROGERS: Jess, if I could ask a question about termination, whether you terminated and so forth? It seems to me very difficult if you find Lockheed, for example, has been deficient and you are disappointed with the performance, to terminate because they are right in the middle of something. Do you have any alternative? Are there any severe penalties you can impose?

MR. MOORE: It depends upon the specific contract, but in the case of Lockheed, they are under an incentive fee contract, and they are supposed to process so many missions, and process them and make sure that—if they have a problem with their processes, they are penalized. There is not only a dollar penalty, but

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there are also written-up penalties that go to the corporation, and each contract that we have has various segments to it.

I think the Lockheed contract is basically a three year contract with three three-year extensions to it, and so you can terminate at the end of that particular three years.

CHAIRMAN ROGERS: But in the meantime you can fine them or impose a heavy penalty?

MR. MOORE: You can impose a heavy penalty, and also you can get the corporate management involved in this thing, and that is where the action came about in terms of replacing the managers that originally took over.

CHAIRMAN ROGERS: Have you imposed any penalties?

MR. MOORE: Yes, sir, there are award fees, and they've lost a lot of fees over the past few years.

DR. WHEELON: You can withhold the awarding of grants. All of their costs are covered, so they are not forfeiting costs. They are just not earning profits. But there are opportunity costs lost.

MR. MOORE: That is true.

CHAIRMAN ROGERS: That is money they don't get, in other words?

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DR. WHEELON: In other words, they might have done a little better under the award fees.

CHAIRMAN ROGERS: Maybe we ought to think of some more severe penalty. You know probably as well as I do, and probably better, that there is dissatisfaction about performance, and particularly in Congress. I mean, they have spoken to me about the fact that they feel that the performance has been sloppy, the contractors have been taking it easy and so forth, and even if we don't relate it, relate that poor performance to this accident we are talking about this morning, there's no reason why we can't comment on it, there's no reason we can't comment on the very severe terms, and propose a penalty or some other provision that you can use so that if there is a deficiency of that kind, even if management couldn't have detected it——

MR. SUTTER: I would like to make a comment.

CHAIRMAN ROGERS: Is that too tough?

MR. SUTTER: No. I think that as soon as you take that up, then we should ask the question why, and the answer may not be sloppy work, per se. It may be pressure. For instance, this pad did not have a rain cover, an item that was scheduled, and these people, they work for one company once, and then they work for another company and change management; it's the same

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workers with experience but different managers, and he is under pressure.

And I think that is one of the things that we should ask, is the schedule realistic, especially now.

CHAIRMAN ROGERS: What was the reason for the lack of rain cover?

MR. ALDRICH: Can I give you a bit of a lengthy answer?

CHAIRMAN ROGERS: Sure.

MR. MOORE: Why don't you give some background on the whole waterproofing situation?

MR. ALDRICH: The orbiters come from the factory with brand new tiles and they are waterproofed internally. That is, they can stay up indefinitely. When you fly the orbiter, it enters under a high heat environment, and that heat causes the waterproofing to be driven out of the tile, so you have to rework it before the next flight.

The arrangement was to internally replace the waterproofing in each tile in Florida between flights, and it was a system developed with squirt guns where we actually penetrate each tile and squirt a certain amount in. And on Challenger, several years ago now, after about its eighth flight, after about its fifth flight, I believe, of Challenger, we noticed a condition where the

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rubber that was holding the tiles to the vehicle, the rubber cement, was softening, and in effect, a tile came off during re-entry, which led to the investigation. We found large amounts of softening rubber, and the investigation showed a reaction between the constituency of the rubber and the rewaterproofing material.

Since that time we have not used the rewaterproofing technique again, and we have run the threat of the orbiter picking up some amount of water. We have been investigating. We have had an extensive analytical program to find a new material that will work as well. That is still under work.

In the interim, we have been spraying the vehicle with Scotchguard, which is a deterrent but doesn't completely preclude water pickup, particularly in heavy rain, and we have added additional rain protection to the launch pads. We put up some metallic protection on Pad A, the old pad, and also some canvas type material that protects it from the driving rain, and it has been very effective.

During this period of time, however, Pad B has been coming along to be the second launch pad, and a more elaborate all-metal waterproofing system was conceived and was being implemented on Pad B. Sometime this past fall some of the materials for that were

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delivered, and they were defective, the metal was not up to the standard expected of the metal, and so it had to be scrapped and returned and wait for additional metal to arrive.

So in that period leading up to the first use of the pad, we had to make the choice of going to the pad without the complete mechanization of the waterproofing intended for Pad B, and Pad A would be retrofitted to the same all-metallic consideration later.

The original schedule was that in January of this year for this launch, all the rain protection on Pad B would be finished, along with the other facilities on Pad B, and materials prob-

lems caused this issue which we believed we could well work around, and I think we did work around it successfully as far as the orbiter, which is the concern for the rain protection. We don't have rain protection for the solid rocket boosters. It has never been considered that that would be required.

CHAIRMAN ROGERS: Well, yesterday didn't I hear testimony to the effect that rain also had an effect, you were concerned about it on the outside of the boosters and the launch pad itself?

MR. ALDRICH: Two questions with separate answers.

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The concern on the outside of the external tank comes from either rain—in fact, rain is good. What is really bad is humidity in the air because the tank is so cold, ice will form on the tank, and that is a concern only because when you launch, the ice will fall off and strike the orbiter. So again, we have different loading conditions and conditions leading to launch that preclude moisture from forming ice on the tank.

DR. RIDE: The tank is only cold after you put the cold propellant in it.

MR. MOORE: That is exactly right, about seven or eight hours before launch, when you fuel the tank, then you get it extremely cold, and if there is a lot of humidity, you will get ice on the tank, as Sally was saying. We are concerned about ice then breaking up.

MR. ALDRICH: But it is only of concern again for the orbiter.

MR. HOTZ: You did get some damage in early flights from the ice breaking up?

MR. MOORE: We did see on some flights some tiles that were damaged and so forth on the orbiter.

MR. HOTZ: They were pretty big chunks that were coming off.

MR. MOORE: Yes, and that is why we send an

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ice team out to take a look and see how bad. There is a spec in the program about what kind of ice is acceptable and what is not acceptable.

MR. ALDRICH: There have been modifications to the insulation on the external tank to correct the areas where we had large pieces of ice. And we have seen a very significant reduction in damage to the orbiter from the ice during flights.

MR. HOTZ: You only got it on the early flights?

MR. ALDRICH: Yes, sir.

Now, the facility, we were concerned for ice there largely, not so much from the rain but from the facility systems themselves, broken lines, fluid comes out on the facility, freezes, creates ice that can interfere with the operations of the equipment at the launch facility. There's not a concern about safety of the flight hardware but for the ability of the facility to continue to do its job successfully during that time period.

CHAIRMAN ROGERS: I mentioned yesterday, I think, and maybe before the report I saw in the paper, about Rockwell, and I found it in Time Magazine, and you made a comment about it. The report said that a Rockwell official said that he had called 20 minutes before the launch to advise against the launch because

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of ice, and I wondered, did Rockwell change their mind? Were they in that conference?

MR. ALDRICH: Let me tell you about the conference. In the conference that I held outside the firing room—I was in the firing room with Jess, and I went outside to hold this conference.

The concern was for the orbiter. The concern was for ice that we knew was in the vicinity of the facility, on the facility, at different locations, falling at ignition and hitting the orbiter.

In the conference I had, the orbiter project manager from Johnson Space Center, the chief of engineering from Johnson Space Center on the telephone, live, myself, the Shuttle Program had worked these issues previously in the meeting personally, the head of the orbiter project for the Rockwell contractor in Downey, on the line to him, the chief engineer for the orbiter project at Downey, and we had a discussion where all parties participated.

CHAIRMAN ROGERS: And this call was reported to that group, and you all considered it, and decided to go ahead?

MR. ALDRICH: I think this call was that discussion.

MR. MOORE: There is no recollection of any

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other manager from Rockwell calling outside of this group. I certainly didn't get any phone calls. Nobody that I talked to knew about that.

CHAIRMAN ROGERS: Well, I press that a little bit because as long as there is not a dispute about the facts, then it doesn't seem to me that anybody can fault anybody. After all, we are going to have a lot of different points of view on something like this, and somebody has to make the decision.

DR. RIDE: Well, I guess the question is whether at the end of this meeting Rockwell was saying we don't want to launch.

CHAIRMAN ROGERS: That is exactly it. If Rockwell comes up in a public session and says we advised them not to do it and they went ahead anyway, and were concerned about the weather, and so forth, then you've got a problem.

On the other hand, if there is no dispute about the facts, that this was conveyed to everybody, and everybody, after consideration, everybody agreed to it——

DR. WHEELON: What position did Rockwell take?

MR. ALDRICH: Everyone in that meeting—and I just told you who was there. There were many others,

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Kennedy facility people at that meeting, everyone in that meeting voted strongly to proceed and said they had no concern, except for Rockwell. The comment to me from Rockwell, which was not written specifically to the exact word, and either recorded or logged, was that they had some concern about the possibility of ice damage to the orbiter. Although it was a minor concern, they felt that we had no experience base launching in this exact configuration before, and therefore they thought we had some additional risk of orbiter damage from ice than we had on previous meetings, or from previous missions.

CHAIRMAN ROGERS: Did they sign off on it or not?

MR. ALDRICH: We don't have a sign-off at that point. It was not—it was not maybe 20 minutes, but it was close to that. It was within the last hour of launch.

CHAIRMAN ROGERS: But they still objected?

MR. ALDRICH: They issued what I would call a concern, a less than 100 percent concurrence in the launch. They did not say we do not want to launch, and the rest of the team overruled them. They issued a more conservative concern. They did not say don't launch.

GENERAL KUTYNA: I can't recall a launch that I have had where there was 100 percent certainty that

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everything was perfect, and everyone around the table would agree to that. It is the job of the launch director to listen to everyone, and it's our job around the table to listen and say there is this element of risk, and you characterize this as 90 percent, or 95, and then you get a consensus that that risk is an acceptable risk, and then you launch.

So I think this gentleman is characterizing the degree of risk, and he's honest, and he had to say something.

DR. RIDE: But one point is that their concern is a specific concern, and they weren't concerned about the overall temperature or damage to the solid rockets or damage to the external tank. They were worried about pieces of ice coming off and denting the tile.

MR. MOORE: I think that is an important

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point.

MR. HOTZ: This is completely different from Thiokol's alleged concerns.

VICE CHAIRMAN ARMSTRONG: The concerns that were expressed there are not obviously related to the accident at this point although they may be.

MR. ALDRICH: I think that is not true, Neil. The concern was for falling ice to damage, and therefore remove a tile from the orbiter. The tiles play no role in the ascent of the Shuttle system, and if I had made the wrong decision in proceeding for the launch, we would not have known any issue from that until the orbiter came to the re-entry phase and we saw it, and we saw a hole burned in it.

CHAIRMAN ROGERS: I guess what we want to be sure about, though, if we can, is whether there was a telephone call, and who got it. In other words, if you have an official of Rockwell saying we called and told them not to do it; however, they say—and you say, well, we never got such a phone call, then you've got a discrepancy. If you got the phone call and you considered it, and you decided to make the decision based upon the reasons you have just described, then I don't see any problem with that.

MR. ALDRICH: My only question was I think the

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phone call went the other way. I think my Rockwell project manager was in the room and called the chief engineer at Downey and asked for their position, and he reported that position to me which was his, and, as I stated, we made the decision to go.

I don't think there was any input from Rockwell.

CHAIRMAN ROGERS: The Time story said, and I don't have much confidence in their accuracy, but this one sounded accurate, said that a Rockwell official had been watching the launch preparation and noticed on the television how much ice there was and made a call 20 minutes before the launch saying he didn't think they should do it, and then they quoted Jess as saying we fully considered that matter at length and decided to go ahead.

MR. MOORE: I don't know why the 20 minutes, where the 20 minutes comes in. I don't think any of us were involved in any conversations, because you came down from your meeting. Your meeting was at 8:00 o'clock in the morning. You came down about 9:00 and sat in the firing room, and Phil Culbertson was there, and Dick Kohrs, and you guys came down and said we have considered the launch, considered the ice, and we believe it is an acceptable risk based upon the ice

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concern for the orbiter. It is principally a descent problem, and we do believe we ought to send out one final ice team out at about 20 minutes before launch, and that is exactly what was done. An ice team went out and walked on the mobile launch platform and reported back into the loop, to Arnie and so forth, and came back and said they don't see anything out there that is anomalous.

And so that is the way it happened. I don't know where the 20 minute call came in.

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CHAIRMAN ROGERS: I just wanted to clear up the phone call and let's get our facts straight as we can before there's any further discussion about it. I don't want to press any more on this. It is just the kind of thing I know that gets a lot of attention in the press when you have this kind of thing, that they focus on a few little things, and this would be one, if there's a disagreement as to facts.

If I said I made a call and I told them and they said we never got any such call, then you have to go get the record of the phone call. And so I just wanted to straighten that up.

Unless there's something else, I would like to ask some questions about our plans. I think the consensus is among the Commission members that we at some point would like to visit the facilities, all of them, I guess. And the question comes up, when would we be the least nuisance.

MR. MOORE: Don't call yourself a nuisance.

CHAIRMAN ROGERS: Well, I think we are when we go to those places. There's going to be a lot of concern about what we're going to ask and so forth.

First, I want to tell you, as far as I'm concerned we will try to do it in a way that is not disruptive. But secondly, if you could tell us when it

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might be best to do it.

I was thinking certainly not next week, but maybe the week after that.

MR. MOORE: That is what—you kind of suggest, we, Arnie and I and a whole group are going back to Kennedy Monday morning, and we're going to spend Monday afternoon and probably Monday evening getting not only the reports from the groups of activities, but also putting in a more permanent organizational structure and trying to look at these teams and see if we have got some things that have been left out and if we want to consolidate some teams and so forth.

Tuesday we will have detailed reports, status reports, back from the people we have had working. So we will spend Tuesday in a data gathering mode.

Wednesday we will probaly come back to the Washington area and Arnie will probably go back to Houston. And I would say the first part of the following week would look like a good time that we could set up, I think, out at the Cape, an excellent meeting for you.

CHAIRMAN ROGERS: That would be a week from next Tuesday?

MR. MOORE: That would be a good time frame to do that.

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CHAIRMAN ROGERS: How does that sound to the members of the Commission? We don't all have to be there.

MR. HOTZ: What is the date?

MR. MOORE: That would be the 18th, February 18. You could see the wreckage, the layout, the photography that's being pulled together there, and the analysis that we have done.

DR. RIDE: Is that something that we could do in a closed meeting, in a closed way?

VICE CHAIRMAN ARMSTRONG: That's going to be tough if they find out we're going.

DR. RIDE: That is not what I was wondering about.

MR. HOTZ: Well, they will find out we're going there. There's no way you can keep that out. But you could certainly keep it a closed meeting, and maybe the chairman or somebody could make a statement after the meeting that, we have inspected this, and so on.

CHAIRMAN ROGERS: Let's assume we could do it in a closed way. Let's first just decide on the time.

GENERAL KUTYNA: Mr. Chairman, Mr. Fuqua's staff members came up to me and said they wanted a report from Jess and our side on what the alternatives were, and I thought that was the 18th.

MR. THOMPSON: Mr. Chairman, there is—and I

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just throw this out for your consideration. The Senate Committee on Space will be holding hearings that day on this issue.

CHAIRMAN ROGERS: What about the end of next week, like Thursday?

MR. MOORE: As far as I know, that would be okay. I think some time next week, next Thursday, would be well. We could support you next Thursday.

DR. WHEELON: Isn't there a certain charm to that? After all, the press would be focused on the Senate, who seeks press coverage, in contrast to us.

CHAIRMAN ROGERS: No. There are some of them that would follow us. They are assigned to us and they would go with us anywhere.

MR. SUTTER: Well, how about the 19th or 20th? I don't think these guys will have a lot more to tell us in four or five days.

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MR. MOORE: We will show you the status of our analyses and the status of our photographs of the investigation.

DR. RIDE: I would say I think there is probably at least a full day of things for us to see, and just seeing the debris and seeing the photos——

CHAIRMAN ROGERS: How about Thursday?

MR. COVERT: I would prefer the 13th or 14th. That's next week.

MR. ACHESON: Could I ask a question? What should our timing be in relation to the recovery of the booster?

CHAIRMAN ROGERS: I think we're going to have to take a trip down there anyway. I think this is a good time to do it, if it is a convenient time. I mean, we can't wait for the recovery.

MR. MOORE: We will status you where we are in all of the areas and any prognosis that we might have. But my guess is it's going to be a long time before we have got all the procedures in place to make sure that we can handle that hardware without doing it additional damage.

DR. WHEELON: The 14th is better for me, and I probably can provide transportation from the West Coast.

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MR. HOTZ: What kind of a trip are you talking about, down in the morning, back at night?

CHAIRMAN ROGERS: That's what I'm wondering about.

MR. WALKER: I think we're going to need at least a day down there and maybe two.

CHAIRMAN ROGERS: Why don't we do it Thursday, and if you can't make it then, Bud, you come down Friday.

How about the others? Is that okay with you?

MR. HOTZ: What was that again?

CHAIRMAN ROGERS: Thursday.

MR. HOTZ: That would be Thursday and Friday of next week?

CHAIRMAN ROGERS: Yes.

MR. MOORE: I would say we've got enough data to keep you very, very busy for as long as you want to stay down there. I mean, every one of our meetings that we've held down there has gone seven or eight hours, and we have had to hurry them through.

CHAIRMAN ROGERS: Gene, is that okay with you?

MR. COVERT: Yes, sir.

CHAIRMAN ROGERS: Dick, in your absence we arranged a meeting for all of next week. Is that okay?

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[Laughter.]

CHAIRMAN ROGERS: We're planning to go to Kennedy on Thursday and possibly Friday of next week, okay?

DR. FEYNMAN: Yes.

CHAIRMAN ROGERS: Bob?

MR. RUMMEL: I think so.

CHAIRMAN ROGERS: And Arthur?

MR. WALKER: It's as bad as any other time.

[Laughter.]

CHAIRMAN ROGERS: So let's plan on that. We will go down Thursday.

MR. MOORE: We will be ready to talk to you next Thursday, then.

MR. RUMMEL: A housekeeping question. Will someone arrange hotel accommodations?

CHAIRMAN ROGERS: Yes, that is our hope. You will make the arrangements, Jonathan?

MR. THOMPSON: I will take care of it.

CHAIRMAN ROGERS: What about transportation? Do you want to provide an airplane?

MR. THOMPSON: I will have to look into that.

CHAIRMAN ROGERS: Well, we will decide how many people are going from here and then decide how to get there.

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MR. THOMPSON: One other thing. I think, to avoid confusion with Jess' people and between you, if you have got a request for information from anybody that testified yesterday or that you think of something over the weekend, between now and next week, I think what might be easiest, if you could give me a call or detail exactly what it is you want.

And I will provide that information to Jess and his people, and in turn I can submit it back to you.

GENERAL KUTYNA: Mr. Chairman, thank you.

Jess, at that meeting would you have an overview of where you are in the planning for your investigation that would be better than we have today, so we can see the major milestones and possibly plan future actions?

MR. MOORE: Yes, you will see a more definitized charter of the things I'm looking into and an organizational structure of how we're going to operate, and probably some forecasts of milestones and some consolidation of the teams that we have formed to make sure that we are cross-

cutting all of the areas that we need to and that we are consolidating teams, that now we have maybe several teams working on various things.

And so I will have, I think, a significant

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amount of progress from where we are now with respect to organization. As far as long term prognosis about what is it going to take to get this fixed, it is totally too early.

CHAIRMAN ROGERS: May I suggest that we break up for the moment and reconvene at ten after 2:00. What about food? Is there any place to eat here?

Why don't we do that. We will try to finish—let's try to meet here at 2:00 o'clock, so we can get out by 3:00.

(Whereupon, at 1:20 p.m., the Commission was recessed.)

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PRESIDENTIAL COMMISSION ON SPACE SHUTTLE CHALLENGER ACCIDENT—EXECUTIVE SESSION—MONDAY, FEBRUARY 10, 1986

Room 476
Old Executive Office Building
Washington, D. C.
The Commission met, pursuant to recess, at 2:10 o'clock p.m.
PRESENT:
WILLIAM P. ROGERS, Chairman
NEIL A. ARMSTRONG, Vice Chairman
BRIGADIER GENERAL CHARLES YEAGER
DR. SALLY RIDE
DR. ARTHUR WALKER
RICHARD FEYNMAN
DR. EUGENE COVERT
ROBERT HOTZ
DAVID C. ACHESON
MAJOR GENERAL DONALD KUTYNA

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ALSO PRESENT:
JONATHAN THOMPSON, National Aeronautics and Space Administration
AL KEEL, Executive Director, Presidential Commission
WILLIAM G. GRAHAM, Administrator, National Aeronautics and Space Administration
PHILLIP CULBERTSON, National Aeronautics and Space Administration

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PROCEEDINGS

CHAIRMAN ROGERS: If I may, I would like to call the meeting to order, please.

The first order of business is to welcome our new member, Chuck Yeager, who was not with us at our previous meetings because he was breaking another record, and I just wanted to say

how happy we are that he is here, and how pleased we are that he is a member of the Commission, and we would like to give him a hand.

(Applause.)

CHAIRMAN ROGERS: The second thing, Chuck tells me that he has to, because of previous commitments, he has to leave tonight and is going to be gone until when?

GENERAL YEAGER: When is the Commission ending?

(Laughter.)

GENERAL YEAGER: The 7th of March.

CHAIRMAN ROGERS: Anyway, we will look forward to your safe return and the opportunity to get together with you when you come back, and take part in the most crucial aspects of this investigation.

By way of preliminary comment, let me say that I have not had a chance to meet with all the members of

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the Commission since the weekend developments. I talked to some of you on the phone, but we will have the opportunity to talk a little further in executive session about some of those developments and why we felt it was desirable to have this meeting, and why we felt it was desirable to have a public meeting.

And in that connection, I want to say that the public meeting was something I think that most of us felt we had to have, and I talked to Dr. Graham, who strongly supported that position. Otherwise I would have polled everybody on the Commission before we made the decision to hold a public meeting. But in view of the time pressures and in view of the fact that that is what we wanted, I felt it was quite appropriate to go ahead with this meeting.

So what we have asked for was the production of all the documents and records that relate to the matters that involve the seals, and we realize that it is not possible on such short notice to produce all of the documents, but I am sure that NASA has attempted to give us the key documents now, and Dr. Graham has agreed that he would give the Commission not only the documents that we have available, which will be supplemented by later documents, but also that he has here today and he will provide any further witnesses that

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you would like to discuss the matters that we are here to discuss.

I think it goes without saying that the article in the New York Times and other articles have created an unpleasant, unfortunate situation. There is no point in dwelling on the past. The important thing is to be sure that the Commission has all the appropriate documents and all the appropriate information. It may well be that we have all learned a lesson from this, that as much as possible we would hope that NASA and NASA's officials will volunteer any information in a frank and forthright manner. We don't want to be in a position that we have to ask for everything in advance.

This is not an adversarial procedure. This Commission is not in any way adversarial, and we hope that in the future, as much as it is humanly possible, when you think information has been developed that we should know about, that you will volunteer to give us that information.

And with that short statement, I would like to call on Dr. Graham to proceed and present whatever he would like to present on behalf of NASA.

And I might also say while he is taking the podium that he has cooperated fully with us, and we have

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no reason to think that we will not get full cooperation from NASA.

DR. GRAHAM: Thank you, Mr. Chairman and Commission members.

As I said in the remarks opening NASA's presentation to the Commission last Thursday, that while NASA continues to analyze the system design and the data, you can be certain that NASA will provide you with full cooperation. That is NASA's policy and my personal position as well, and that continues to be NASA's policy, and will remain that way throughout the course of this investigation.

To help that process, I have put out the following internal memorandum today that I want you to be aware of. This is to the Associate Administrator for Space Flight, Jesse Moore, who is also the head of our Internal Design Review and Data Analysis Task Force, who has the overall responsibility within NASA and to me for the conduct of our work related to the Challenger. And it says all NASA testimony should be reviewed on a word-by-word basis by a knowledgeable NASA technical review team, and this refers to testimony which has already been presented to the Commission. Should any error, partial or incomplete statement or potentially misleading statement be found, an amendment to the

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testimony should be filed in order to clarify the issue of concern.

And so I want you to know that in addition to trying to give you as timely and complete a volume of information as we can during our testimony, we realize that it is possible for NASA to occasionally misspeak or to delete something inadvertently, and should that occur, we will in any case be going back over the testimony and looking at it and checking it. As soon as we find something that appears to be—to warrant an amendment to the testimony or a clarification or an addition to the testimony, we will provide that to you.

CHAIRMAN ROGERS: We are going to have a little atmospheric problem here, and so I don't want to interrupt you.

Let's see if we can get it a little cooler.

(Pause.)

DR. GRAHAM: Mr. Chairman, NASA hereby formally submits to you all of the reports, memoranda, briefing charts and other material that we have been able to locate to date concerning issues associated with the integrity of the SRB segment seals and other things related to that part of the solid rocket performance, and assembly and operation.

And I would now transfer to you, the Commission, the

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material. We will continue to search for such material. As we find it, on an incremental basis we will transmit it to you through our channels of liaison and make sure that it is called to your attention.

From this point forward, I intend to turn the conduct of the NASA presentation over to Mr. Moore. But before I do, I thought I would be willing to entertain any questions you have.

CHAIRMAN ROGERS: When did you first hear of the possibility of the story by the New York Times?

DR. GRAHAM: Mr. Chairman, I first heard of that at approximately 1:40 p.m. yesterday—Saturday.

CHAIRMAN ROGERS: And what did you do at that time?

DR. GRAHAM: At that time I called you to make you aware of the general subject that was being addressed and the possibility of a story from the New York Times, and that certain docu-

ments appeared to be in their hands, and I also informed others within the administration that this material apparently had come into the possession of the New York Times and had the possibility of the story going forward.

CHAIRMAN ROGERS: Have you since determined whether any previous work had been done by NASA in

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connection with the preparation of the story or preparing a reply to the story?

DR. GRAHAM: I believe there was an awareness, at least earlier that same day within NASA, of the story, because it was brought to me by NASA employees. There was some thought given as to how to respond to it, but no response was transmitted outside NASA at that time because it seemed appropriate that the Commission be aware of the subject before a public response was put forward.

CHAIRMAN ROGERS: So that my question is not misunderstood, I want to make it clear that obviously you can't report to the Commission every time that some newspaper is going to write a story. We wouldn't expect that. On the other hand, there are certain types of investigations which you may be aware of that seem to have particular significance, and in such event, we would hope that you would, and members of NASA would immediately let us know about it so that it didn't appear that we were taken by surprise.

Do you agree with that?

DR. GRAHAM: Very much so, Mr. Chairman. I am in complete agreement with that policy, and I have transmitted that policy to the NASA staff both before last Saturday and since last Saturday.

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CHAIRMAN ROGERS: Furthermore, I want to make it clear, speaking for myself, that we don't want to react to every newspaper story because it is inevitable there will be a lot of them, and a lot of them will be unfair and unfriendly.

I guess what concerns me a little bit about it, and I hope we don't have any further discussion publicly about it, is that this seemed to go right to the heart of the matter, and it seemed to be related to the plume that was started and shown to the public, and it occurred to us that there must have been a good deal of thought in NASA about how serious a story it would be if it appeared, and therefore I would have thought that there would have been an eagerness to present it to the Commission on Thursday, and particularly on Friday, in the private session.

But with no further ado, go ahead.

DR. GRAHAM: Yes, sir. I share your view of that, and I think that further discussion addressing that issue will be given to you today.

DR. WALKER: Is any of this material specifically classified?

DR. GRAHAM: Let me ask the people who compiled it all.

Is any of this package, have you identified

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any material that is classified related to this subject that isn't in the package?

MR. MOORE: No.

CHAIRMAN ROGERS: In that connection, it would be helpful to be sure that any time we are given classified information, that it is made clear to us that it is classified because otherwise we will treat it as if it is not.

MR. ACHESON: Could I ask a question?

In The Washington Post story on Sunday, a number of theories of the accident were expounded and illustrated in that story, and whether or not any of these are probable, I assume one or more may have associated internal correspondence, and I would like to suggest that that be submitted to this Commission in advance of any similar publicity occurrence as that of last Sunday.

DR. GRAHAM: I have also asked that NASA staff pull together all of the information that may exist in our files related to any failure modes of the SRBs and make sure that is available first to me, but very quickly then transmitted to the Commission. That subject can be enlarged even further to the extent of the tank and the orbiter itself and the ground support equipment, and if you wish, we will try to do that.

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I think that we will need to realize that there will be a large volume of engineering information, concerns and issues and so on, that it may take a little bit of time to pull that together in any complete way, and there probably will be a substantial volume of information as there has to be in any complex program such as this, a constant ebb and flow of engineering design checks, information, background and so on, such as this, and I strongly agree with the Chairman that this is a particularly pertinent one and very much to the issue now.

We will continue to pull that information together across the entire system and continue to provide it to the Commission. And with your concurrence, I would like to do that in the order, SRB first, expendable tanks second, and the orbiter third, and then ground support equipment and related matters fourth, subject only to particular issues that seem to arise that might put us off the track.

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(Material deleted.)

We will certainly do that, and you will have that within the day.

In fact, this information is information we need to collect internally in the course of our own analyses and review, and so this operation does not constitute a major interference. In fact, it is complementary with what we are doing, and we would certainly do it in any case, and we are pleased to provide it to you.

CHAIRMAN ROGERS: Very well.

DR. GRAHAM: Thank you very much.

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Mr. Moore?

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TESTIMONY OF JESSE W. MOORE, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT, NATIONAL AERONAUTIC AND SPACE ADMINISTRATION

MR. MOORE: Mr. Chairman and members of the Commission, the data that you have before you here is data that we immediately started collecting sometime last week in preparation for the briefing of the Commission on this coming Thursday at the Kennedy Space Center. It was our intention to review that data with you at that time, as well as status reports on the data on all other areas.

Let me say up front that we are looking at all areas of the 51-L incident. We are looking at the tank, the SRB and the orbiter and so forth, and anything that we judge sensitive on those

areas we will try to clearly make it available to the Commission prior to any data that we would want to present, and so forth. We will honor Dr. Graham's request to provide that, to the best of our knowledge.

One of the things we would also like to state for the Commission is that much discussion as you have doubtless heard over the weekend about O-rings has appeared in the paper. I would like to say that O-ring erosion, which we are going to address today, is only one of several areas that we are looking at for possible scenarios, anomalous scenarios. There have been some

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concerns on the O-rings. We are going to try to relate that to you today. We will provide you with any additional information that we have in our hands to support that, plus any other things.

We are looking at a lot of additional areas in addition to the O-rings. As I say, there is a lot of additional data on flight readiness reviews. We could probably stack several feet of documents on you in terms of this Commission. What we have tried to do is to excerpt some of those documents, the most relevant pieces of information here today, and as the Commission has additional questions or needs additional information, we would be glad to do our best to provide that.

I would like to do two things here, if I might. I would like to first quickly review for you the agenda that we prepared.

(Viewgraph.)

MR. MOORE: I trust everybody in the back can see this as well.

I am following Dr. Graham's introductory remarks. I'm planning to just give you kind of an overview of setting the stage of this whole activity, and I particularly wanted you and the Commission members to meet the people, some of the people that are involved

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in the Shuttle projects, and in particular, the solid rocket booster project, not only here in my office in Washington but also at the Marshall Space Flight Center, and also several members of Morton-Thiokol who are here as well, that handle the solid rocket booster project, and we would again welcome any questions that you or the members of the Commission may have on the booster activities as we go forward.

Mike Weeks is a deputy in my office at NASA headquarters. He is the Deputy Associate Administrator with emphasis on the word "technical." Mike has been in the program for some time, since about 1979, and is very familiar with the elements of the Shuttle program.

Let me introduce Mike Weeks right here.

Then Mike will also provide for the Commission documentation that we have at NASA headquarters and some concerns that we have had over a period of time on the solid rocket motor, and in particular, on the O-rings.

Then we have also asked Larry Mulloy—and Larry, would you stand up, please? He is right here—who is the Manager of the solid rocket booster project at the Marshall Space Flight Center, to go through some technical areas with you today to give you some historical data on the solid rocket booster and some of

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the performances that we have seen out of the SRB in the course of the Shuttle program.

And finally, I will have a few closing remarks, and one item that I will want to discuss here with you this afternoon, Mr. Chairman and other members, is our participation at tomorrow's session. If you can give us some insight into that, I would appreciate it.

Let me also say before I turn this over to Mike, let me introduce some of the other people that are here.

I would like to introduce Mr. Bill Hamby, Mr. William H. Hamby. He is the Deputy Director of STS Program Integration in my office at NASA headquarters.

I would like to introduce Mr. David Winterhalter. David is the Director of Shuttle Propulsion in my office in Washington.

And I would like to introduce Irving Davids. He is with the Shuttle Propulsion division.

I would like to introduce Paul Wetzel, who is the Chief of the Solid Rocket Booster Programs at NASA headquarters.

And I would also like to introduce Paul Herr. He is in the Shuttle Propulsion Division at NASA headquarters.

And finally, with NASA, I would like to

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introduce Russell Bardos, who is the Manager of Productivity Operations Support, also at NASA headquarters.

These are, in effect, a large percentage of my staff that are working in this particular area. And so I thought if you or other members of the Commission would like to ask them questions directly, we would certainly be happy to try to answer any questions the Commission has.

In addition, I have invited Dr. William Lucas, Director of Marshall Space Flight Center here today, who has responsibility for all propulsion elements on the Shuttle program.

I have introduced Larry Mulloy previously.

I would like to introduce Larry Wear now of the Marshall Space Flight Center. Larry is involved in the solid rocket motor project at Marshall. And I would like to introduce John McCarty, who is Deputy Director of Structures in the Propulsion Lab at the Marshall Space Flight Center.

Now, we have four people here from Morton-Thiokol Corporation in Utah, and I would like to introduce Allen McDonald, who is the Director of the Solid Rocket Motor Project at Thiokol; and I would like to introduce Mark Salita, who is a scientist in the Gas

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Dynamics Section of Morton-Thiokol; and I would also like to introduce Donald Ketner, who is the Supervisor of the Gas Dynamics Section at Morton-Thiokol; and representing the Washington office here of Morton-Thiokol is Frank Ross.

Now, that completes the number of people that I brought here today, and with your permission, Mr. Commissioner, we will proceed with the presentation of the information that we have brought.

And I would now like to introduce Mike Weeks.

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TESTIMONY OF MR. L. MICHAEL WEEKS, DEPUTY ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT (TECHNICAL), NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

MR. WEEKS: I put this briefing together, gentlemen and members, to address first the New York Times article, and there are six documents that obviously were provided to that news media, and first I want to go through those and bring you up to speed on that. [Ref. 2/10-1]

(Viewgraph.) [Ref. 2/10-2]

MR. WEEKS: And then I will go to this chronology of things that have happened on the O-ring problem since we first ran into it. And the first time we really addressed that was way back in 1980, and I will show you that and bring you up to date.

(Viewgraph.) [Ref. 2/10-3]

MR. WEEKS: As was spoken to, and it is in your document there, the first one is the Cook memorandum, and that is a memorandum that was written on the 23rd of July, and it was prepared by the financial analysts over in the Financial Department, and the person is a financial type person and not too knowledgeable of the whole program situation. [Ref. 2/10-4]

I am going along now at page 6. I am right here (indicating) at page 6, and I guess I would suggest to you that that is a less clinical analysis of

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this whole situation because the young chap came aboard about the first of July and was just picking up things in a hallway, and wrote this to his immediate superior.

CHAIRMAN ROGERS: Is he here today?

MR. WEEKS: No, he is not.

MR. MOORE: We could bring him, Mr. Chairman.

CHAIRMAN ROGERS: We didn't expect him. I was just asking. He is still employed by NASA?

MR. WEEKS: Yes, he is.

DR. COVERT: This stuff you call putty, that might be an unfortunate choice of terms. It is really an inhibitor, isn't it? The heat burning from that at the joint might fit together perfectly.

MR. WEEKS: Dr. Lucas ought to handle that.

DR. COVERT: It is zinc chromate, isn't it?

DR. LUCAS: It is a zinc chromate, but it is not in an inhibitor that goes on the end of the train. It is separate from that. I think Larry Mulloy will demonstrate that for you.

MR. WEEKS: Now, the next one, Mr. Chairman and Committee, is a memo dated the 17th of July by Irv Davids over here, who I guess we gave him his 35-year pin some time ago, and he is very senior and very careful, and this is No. 2 up there. [Ref. 2/10-5

DR. COVERT: Is it true that he changed this stuff, as Cook said, from being a sealant that has asbestos in it to being a sealant without asbestos?

MR. WEEKS: No, I believe we still have asbestos in the system, and we eventually have to get

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rid of that, and that is one of our longer term plans. But we aren't concentrating on that today. We are concentrating on O-rings.

DR. COVERT: You are concentrating on—the material that was used in this particular set of boosters was the old material and made by the old manufacturer?

MR. WEEKS: No. This is the second generation because the original vendor, and I have forgotten his name, went out of business, and we had to go to a Randolph type of one.

DR. COVERT: How many firings have you had with the new material?

MR. MULLOY: It was introduced on STS-8.

DR. COVERT: So it is about 17.

GENERAL KUTYNA: Mike, let me ask you, we used this same type of material on the Titan.

Did you change manufacturers also on the Titan?

MR. MULLOY: I do not know.

MR. WEAR: The one on the Titan is an Inmont putty, and——

MR. WEEKS: So we do not fly the Inmont putty?

MR. WEAR: No.

DR. WALKER: Is the putty supposed to be the

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primary or backup seal?

MR. MULLOY: The putty is a thermal barrier, not a sealer.

MR. WEEKS: The pressure goes through the putty and gets down through the exterior of the case, and therefore that pressure of the 1000 psi in the chamber motor does go down to the O-ring.

CHAIRMAN ROGERS: Just so you don't go too fast, let's focus up for a moment on the Cook Memorandum.

As I understand it, you are saying that he was just hired and was in a department where he really didn't have much knowledge of what was going on?

MR. WEEKS: I would believe that you should discount this to a fairly great extent because as you will see in the next memorandum of Mr. Irv Davids, who has been with our program for at least a decade, and is 30 years with the Agency, it is a very careful and thoughtful response to his memo. His memo was created because we had a failure in April of 1985 in which it is the first time in all of the program that we had the secondary seal have any difficulty, and the only time, whereas the other erosions were all in the primary seal, the primary being the one that first sees the pressure, and the secondary being the one that is backing it up,

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if you will.

DR. RIDE: Which flight was that?

MR. WEEKS: That was 51-B.

GENERAL KUTYNA: Mike, you did not have charring of the secondary seal on 51-C?

MR. WEAR: No.

MR. WEEKS: It was 51-D.

DR. RIDE: Did you see a problem with the primary seal on 51-C?

MR. WEAR: We had some erosion on the primary seal in the K joint on 51-C. I will cover all of this later.

DR. COVERT: Did you have the same gel thickness on 51-B, C and so on?

MR. WEEKS: I believe all of these are the thinner steel casings. We started and had the first early flights with the thicker steel casings, but I believe all of the recent flights are thinner casings, and I guess they do get mixed, General.

GENERAL KUTYNA: They do get mixed. We try to use them whenever we can.

MR. HOTZ: Mr. Weeks, could you tell us whether there are any errors of fact in this memo, and if so, would you point them out?

MR. WEEKS: Which one?

MR. HOTZ: The first memo, the Cook memo.

MR. WEEKS: Could you help me out on that, Herb?

MR. MOORE: We can get Mr. Cook here if you

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would like, Mr. Chairman.

CHAIRMAN ROGERS: Yes, we would like that, but let's just go ahead.

MR. MOORE: We will get an answer to that.

CHAIRMAN ROGERS: Just so we understand. I think Mr. Hotz and I asked the same questions. All right. This says: You have asked us or me to investigate reported problems, and then he says, discussions with program engineers show that these are potentially major problems affecting both flight safety and progress cost.

My question is, is what he set forth there accurate, and didn't he talk to the engineers and deduce this information? Isn't this information he got from the engineers?

MR. WEEKS: I think that his statement in here where he says that it might be catastrophic I think is overstated.

CHAIRMAN ROGERS: Well, that may be.

MR. MOORE: I think the best thing for us to do, Mr. Chairman, is to think about getting Mr. Cook here, and then we can ask Mr. Cook to sit down and try to answer your questions on this thing.

CHAIRMAN ROGERS: Yes, but we want to ask questions as we go along——

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MR. HOTZ: We would also like your opinion of whether this is accurate or not. We can get a witness——

CHAIRMAN ROGERS: This is a case where you are saying, in effect, that you didn't have much confidence in this fellow because he was in the wrong department and had been there just a short time, and so we are asking is the material that he reported on accurate?

MR. WEEKS: If I may, I would like to pore over every word and come back to you.

CHAIRMAN ROGERS: Well, is it substantially accurate?

MR. WEEKS: I think it is substantially accurate.

DR. COVERT: I think the other thing, Mike, when you go through it, try to go through it from the point of view of a rather naive sort of guy who hears the words and doesn't necessarily understand all of the nuances but gets an overall picture of things. It has been my experience that sometimes people have amazing insight. Part of the problem is to understand the nuances. Thank you.

MR. WEEKS: Now, if we could go on to Irv Davids' memo, which is on page No. 9, and he is discussing there the two cases, the nozzle-to-case, which is at the back end of the booster, and the case-to-case, which there are three of those joints

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along the length of it, and the one up from the bottom is the suspicious one in the movies of 51-L.

CHAIRMAN ROGERS: Now, it looks to me as if this document preceded the Cook document, is that correct?

MR. WEEKS: It did, yes.

CHAIRMAN ROGERS: So this was not in response to what Mr. Cook wrote.

MR. WEEKS: That's right. This memo was written under the following circumstances. We have the secondary erosion on the flight of the 29th of April, and we asked Mr. Davids and Mr. Hamby to go to Marshall to review it because we were concerned about this being the first case of any erosion on the secondary seal. And that was a fairly small number of .032 inch in that particular case on the secondary seal on the nozzle-to-case joint.

CHAIRMAN ROGERS: Let us take this a glance at a time.

MR. MOORE: Mike, you might point out that Mr. Davids is here.

MR. WEEKS: Yes, he is here.

DR. COVERT: When you say heat affected, was it discolored?

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TESTIMONY OF IRVING DAVIDS, SHUTTLE PROPULSION DIVISION

MR. DAVIDS: Yes.

I would like to just mention that when we went down there, the secondary seal failure that we experienced, or erosion, was at the nozzle-to-case interface and not just the case-to-case.

THE CLERK: I'm sorry, Mr. David, you will have to speak up, please.

CHAIRMAN ROGERS: Excuse me. Would you want to swear the witness in, please?

And this is what we do for all witnesses. This is nothing personal.

THE CLERK: Do you swear the testimony you will give this Commission will be the truth, the whole truth, and nothing but the truth, so help you God?

MR. DAVIDS: I do.

CHAIRMAN ROGERS: This gentleman is a court stenographer, and he has to record what you are saying. So if you could speak a little louder, please.

MR. DAVIDS: I just wanted you to understand that the secondary seal that Mike was alluding to, with the type of erosion that we had was at the case-to-the-nozzle interface and not the case-to-case. That is significant to what we are talking about.

However, when we got that problem due to the

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fact that it was a secondary seal, we thought we had better go down to Marshall and go through the entire area of seals to see what we were doing and what kind of problem we were having, and that is what initiated our visit to the Marshall Space Flight Center. And you will note from the memorandum that we did point out that there was certain—that we did experience some O-ring erosion of the primary O-ring on the case-to-case seals, and the nozzle-to-case seals. And what I wanted to do was obtain all the data that was available so we had a pretty good history of what kind of failure or erosion we were getting on the seals and make sure that all of this data was brought up to top management so that we all were well aware of what the problem was that existed, and we would try to get some pressure to accelerate trying to think about what we could do about clearing up this potential problem that we had, and that was the real intent of why we went down to Marshall.

CHAIRMAN ROGERS: What is the pleasure of the Commission? Would you like to ask Mr. Davids some questions about this memorandum now, or would you want to come back and ask him questions later?

MR. ACHESON: I would like to ask one.

This memorandum refers in the B heading on page 2 to unseating of the secondary O-ring during joint

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rotation.

Is there a measurement of joint rotation made during flight?

MR. DAVIDS: There is no measurement taken, but Mr. Mulloy in his presentation is going to cover that area very specifically. We actually have a joint to show you what the joint rotation is all about, but we have no measurable measurement at all of that.

MR. WEEKS: Not in flight, but as he will show you it has been measured on the ground a couple of times in full scale motors.

DR. COVERT: The suit blow-bys, is that case to case, or case to nozzle?

MR. DAVIDS: That was two cases by the primary seals. If you look at my memo, you will find that the first part of it is just nozzle to case, and the second part of it is case to case. So I specifically separate the two.

DR. WALKER: Do you have a thermal model of what temperatures different parts of the assemblies see, and do you have any measurements of temperature of the various parts such as during firing, etc.?

MR. DAVIDS: I assume we have that.

MR. MULLOY: We do have a thermal model that shows some gradients of temperature, including the motor through the propellant through the

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insulation, through the liner, into the case.

DR. WALKER: Could we have a copy of that?

MR. MULLOY: We can show you some results, yes, sir.

DR. WALKER: You have measurements, too?

MR. MULLOY: No, we do not have any direct temperature measurements.

MR. WEEKS: I am sorry, but I thought we measured some on the static firing out at Morton-Thiokol's factory. We have had many static tests. The static test is a single motor that is fired on the ground horizontally, and I thought we measured a number of temperatures during those early development cases.

Didn't we measure the temperature on the outside?

Well, we will dig into that.

MR. MOORE: Mr. Walker, we will provide you the data on thermal models as well as actual measurements that have been made.

GENERAL KUTYNA: Mike, this letter by Mr. Davids says the prime suspect is the type of putty used, and he notes that you changed manufacturers after STS-10, yet there was erosion way back in 1980, well before this change in putty on STS-10.

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So how do you tie the erosion to the change in manufacturer of the putty if you had that problem before?

MR. DAVIDS: I think Larry will clarify this. If you look at all of the O-ring erosion that we have seen, you find you can actually follow a path through the putty that goes to the spot where

the O-ring erosion occurs. And so it is obvious that you are getting some blow-by through that putty to the point where the erosion occurs, and so you see a clear trace between the erosion and the putty itself.

Now, your question about why we didn't have it on STS-10, I guess my answer would be that we don't have erosion on every flight. There are times when we don't have it and times we did have it.

GENERAL KUTYNA: No, I misstated my question or you misunderstood.

You had erosion prior to changing this putty. Therefore, you had these blow holes or whatever you call them, through the old putty, which you claim was good putty, and now you are saying that after STS-10 you changed putty and therefore you had a problem.

Did you have it beforehand? Is that not true?

MR. DAVIDS: I guess that is true.

MR. WEEKS: You are reading where putty is the

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prime cause of the erosion, General?

GENERAL KUTYNA: Yes. And I am asking, you had this problem as early as 1980, well before this time, well before the time you changed manufacturers.

DR. COVERT: When you say you received these things, I assume that when it comes back after it is hauled out of the ocean, and this comes from disassembly and refurbishment——

MR. DAVIDS: That is correct.

DR. COVERT:—and you take it apart very carefully to make sure that you don't interfere with anything of the visual data?

MR. DAVIDS: That is correct.

MR. WEEKS: In fact, that is, of course, one of the neat things about the Shuttle is this is the first time in history we have been bringing these things back. The Titan, of course, the only thing we can look at there on erosion is the firings they have had on the ground.

CHAIRMAN ROGERS: Do you happen to know whether there is any connection between these two memoranda, or did they just happen to be about the same time?

MR. DAVIDS: I didn't know anything about the Cook memorandum. I had never seen the Cook memorandum.

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MR. WEEKS: The only connection is that in the New York Times article I elected to put them in the order of their occurrence in the article as just a way of formatting this. [Ref. 2/10-6]

Number one, as you will see in there, it shows up in the article on the second page in your handout up at the top where Mr. Cook, down about the middle of the second page is Roman number II which is Irv Davids' memo, and number III is the budget briefing that is spoken to on August 21 and then September 10 shows one of mine, which is the Propulsion Division of Mr. Winterhalter's internal preparation, preparing to come to Jess Moore, which is the next one.

The way we organize every month in the office is that we get the financial data, and then each division, the Propulsion Division, the Orbiter Division, etc., prepare their charts in their own house, and then it is brought to Jess Moore, and then after that we carry it to the Administrator, which is the final thing around the 17th, 18th of the month. And so we go through this ritual every month of how we do this thing. [Ref. 2/10-7]

CHAIRMAN ROGERS: Let me go back just one moment to the Cook memorandum. You will notice at the end of that he ties safety to budgetary considerations.

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He said I would think that any NASA budget submitted this year for fiscal year 1987 and beyond should certainly be based on a reliable judgment as to the cause of the SRB seal problem and a corresponding decision as to budgetary action needed to provide for its solution.

Do you know whether any such action was taken or consideration was given to his memorandum on that point?

MR. WEEKS: I can state authoritatively that no action—I think this is true of Mr. Moore as well, because I didn't see this memorandum until yesterday.

CHAIRMAN ROGERS: Do you know whether anybody else took it seriously then?

MR. WEEKS: We certainly were alert, as you will see as we go through this whole chronology, you will see that we were alert to a problem, but we had not identified a precise amount of money that we thought would be required to fix it.

CHAIRMAN ROGERS: In other words, as I understand it, your memorandum was, as far as you knew, unrelated to anything in the Cook memorandum, so you were just considering the facts that you were dealing with here based upon what had happened in previous flights, and you were making a study of that, and you

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were reporting on it, and you end up with I recommend that we arrange for MSFC to provide an overall briefing to you on the O-rings, including failure history, current status, and options for correcting the problems.

Now, I assume that was done, wasn't it?

MR. WEEKS: Yes. That is, as you will see, in the next set of charts that that occurred on the 19th of August.

CHAIRMAN ROGERS: Now as far as we know from this book, was any follow-up given to the Cook memorandum?

MR. MOORE: Sir, may I comment on the Cook memorandum?

Mr. Cook is in our budget office at NASA headquarters. He is in the Comptroller's office, and that is where you see the BRC. That is a code.

CHAIRMAN ROGERS: I understood that.

MR. MOORE: Now, he wrote this internally to one of the people in the budget office. Mike Mann works in the budget office, and Mike Mann is one of our people who looks after the overall Shuttle budget. To my knowledge, no one in my office, at least in the technical program area here, saw this memo from Mr. Cook.

CHAIRMAN ROGERS: Is Mr. Mann here today?

MR. MOORE: Mr. Mann is in the budget office.

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He is not here today. We would be happy to bring him, too.

CHAIRMAN ROGERS: But as of now, we don't know if he did anything about this? He just treated it as another memorandum?

MR. MOORE: We do not. We are all seeing this for the first time.

I guess we saw it in the newspaper, just like you and other members of the Commission.

VICE CHAIRMAN ARMSTRONG: Am I correct in assuming that what you describe, that in the normal chain of authority, neither Mr. Cook nor Mr. Mann would have anything to do with deciding the technical aspects?

MR. MOORE: Mr. Armstrong, you are exactly right. They are budget analysts, and we at this time of the year are putting together our budget briefing for the Administrator for fiscal

1987, and I am sure the Code B people, our people that you see listed here, are sitting back and looking at areas that they ought to be sensitive to when we come before the Administrator with our budget.

CHAIRMAN ROGERS: Let me say I fully understand that, but we want to be sure that we face the facts. The fact is you have a memorandum, and Cook says certain things he thinks should be done.

All I want to do is find out what was done. If

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it wasn't done, tell us why and we will understand and the record will be clear. That's all.

MR. MOORE: We will have to bring in Mr. Mann and Mr. Cook I think to make sure of that, or we can provide you a written statement.

CHAIRMAN ROGERS: No, we would like to have them.

MR. MOORE: Mr. Winterhalter, do you want to talk about this?

MR. WINTERHALTER: Yes. The very next document, document No. 3, will show that in our budget briefing we referred to the O-ring as a budget threat. We did not specifically add any money to the budget for that but understood that we were studying the situation, that depending if some redesign and some improvement was decided upon, then we would have to have some extra money to cover that outside what we had originally budgeted.

CHAIRMAN ROGERS: Are we going to find out through these papers that you as a matter of jargon, you spoke about budget threat and safety together and related them somehow together?

MR. WINTERHALTER: Not in anything that I have learned.

CHAIRMAN ROGERS: You just spoke about budget

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threat.

MR. WINTERHALTER: We didn't look at that as a safety problem.

MR. MOORE: We identify a budget threat as something that the results of tests or the results of flight may require us to go back and make some changes in it, and so therefore, for the fiscal '87 budget which we were putting together, we identified this area as an area of threat based upon the tests that we had planned on the segments which you will hear about later on in this briefing, and that was the context of the budget threat.

CHAIRMAN ROGERS: Just so we understand it, because this is something that the press will clearly dwell on, I gather you mean when you say budget threat, that if you change conditions to approve whatever it was that you were talking about, that would increase your budget, and therefore it would be a threat to the budget.

Now, doesn't that necessarily relate to the safety of the personnel involved, and that leads to budget considerations?

MR. CULBERTSON: Dr. Graham asked me to sit in a while in his absence.

Let me define what budget threat means within the Agency: anything that could affect current

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projections on the budget is referred to as a budget threat. We ask each of the programs when they submit the budget to do it as realistically as they can but also tell us where that budget may be in error, what kinds of things it can cause. It can range from DOD deciding not to fly a mission on the Shuttle and therefore change our income. It can come from any kind of action, including the results of tests which haven't yet been made, and it can certainly be based upon

somebody's concern that there may be a safety item that could affect our planning and could therefore affect our budget.

The word "threat" is an unfortunate word, but it is what we use, and it is a potential item that may change the budget.

CHAIRMAN ROGERS: So we are likely to find as we study these documents that from time to time when there seems to be a failure of equipment or something that should be improved, that it may be referred to as a budget threat, and therefore nothing should be done about it?

MR. CULBERTSON: No, not that nothing should be done about it.

CHAIRMAN ROGERS: Well, let me correct that.

By the use of the term and relating it to, as

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you did in this case, or as Cook did in his memo where he makes it very clear that that is what he is thinking about, it may appear that from time to time that things were not done in the field of safety because that would present a budget threat. I mean, it just seems to me it is clear, and I understand it, but it seems to me it is clear, it is clear from this memo, the first one you have here.

MR. FEYNMAN: Suppose there is an item which may or may not turn out to be a safety threat, and there is some kind of difficulty, and it may be solved very easily. On the other hand, it may require a large amount of reconstruction of equipment which is quite expensive, and it is not yet known whether it is an important problem either for safety or for anything else, and it is nevertheless potentially a problem, and a problem that the budget has to appreciate may arise. It doesn't mean that they have decided that they are not going to make this change. As a matter of fact, the very fact that they are aware that the budget is going to be threatened represents a statement of the possibility that they will have to repair this for safety purposes, and there may not be an obvious relationship between safety and the budget.

CHAIRMAN ROGERS: Well, as I say—and I don't think there is any point of having a further discussion on it, but I think we can see that the way

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these documents are written would suggest that, and I think that that is something that you would want to think about before we come to a public session because it says so. It says—well, the last paragraph, I don't have to read it. It says there are certain safety problems. My engineers tell me there are safety problems. You have asked me to make a check, and then he says we have got to consider the budget action needed to provide for the solution.

So I think there is no point of saying that you haven't thought about it. I mean, as Dr. Feynman says, it may very well, the documents would reflect that you did give full consideration and you decided that it didn't have to be fixed. I understand that.

But the phrase "budget threat" is very unfortunate.

MR. CULBERTSON: We do not use it as an indication that budget limitations threaten the possibility of taking corrective action. I don't know that it is every really used in that way in NASA, but you certainly can read that connotation into it.

MR. WEEKS: I want to make two points in this regard. In the manned programs, as I know them, through their history, the people making those decisions first look at if it is safety and it is mandatory, we find a way

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in the budget to get it done.

Now, in our budget that Mr. Moore carries across to the Administrator, we have two ways of taking care of things like this. We have changes in upgrading that we can absorb some of these, and we also have a reserve account. In NASA it is called a PA. It is program activity to be allowed for. And so there are ways.

And when we have over the six years I have been here in the program, when we find a genuine safety issue, and we do quite often, we find the money, and have in the six years I have been there, are able to some how, in some way change other things to fit and get the safety not compromised. I cannot think of a case that we have ever said that we will not fix a safety item because of money. Sometimes it has been rather excruciating.

GENERAL KUTYNA: Mr. Chairman, along your line of reasoning, I would ask Phil, might there be some unfortunate choice of words here that ties safety to schedule threat also?

MR. CULBERTSON: Well, it could be used that way.

GENERAL KUTYNA: Are there some you are aware of in your research of the documents, because those

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could be misinterpreted.

MR. CULBERTSON: The way we use schedule threats as to the schedules is the same way we use budget threat for the budget, and nothing further is inferred. The budget people worry about threats to budget; schedule people worry about threats to schedule. The program worries about the overall quality of it.

CHAIRMAN ROGERS: Because we are going to have a public session and this is going to be discussed for a long time to come, in the Cook memorandum he says, he has talked to progress engineers, and in discussion with progress engineers, shows it to be a potentially major problem affecting both flight safety and program cost. And last, he says it should be noted that Code M management, what is that?

MR. WEEKS: Code M—Jesse Moore is the head of Code M; the Associate Administrator for Space Flight is called Code M.

CHAIRMAN ROGERS: He says it should be pointed out that Code M management is viewing the situation with the utmost seriousness. From a budgetary standpoint, I would think that any NASA budget submitted this year for FY 1987 and beyond should certainly be based on a reliable judgment as to the cause of the seal problem and a corresponding decision as to budgetary

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action needed to provide for its solution.

Now, that memorandum either had not received much attention, on the one hand, which is understandable, assuming that there wasn't much confidence in Mr. Cook or based upon his experience, or it was followed up, and some decisions were made on it, and I guess that is what I think we have to keep in mind.

MR. MOORE: Mr. Chairman, let me just add one quick point to that, if I might. In the case of a situation that Mr. Cook describes, we have been following up, and we have been following up this O-ring concern for some time. In fact, you will see a program laid out that we have had under way leading up to some tests that are scheduled for the month of February.

So he is right in that particular aspect, and he is also right in the sense that it did represent in his common knowledge a budget threat, that we may come over and ask for a substantial amount of money in the budget request.

CHAIRMAN ROGERS: I think that is the answer to my questions.

MR. MOORE: You will see, Mr. Chairman, the program that we have laid out has been under way for some time in this whole question about O-rings.

MR. WEEKS: Now, I think that we could——

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DR. WALKER: Excuse me. When you have a situation where a system doesn't perform as you predicted, do you have some mechanism where you look at that and decide what the implications might be for safety or schedule, or whether something can be slipped? Do you have some procedures that you go through?

MR. MOORE: Mr. Walker, we have two major paths that we undertake. One is we have got a program path which people in my office work with the corresponding center people, and the center people work with the corresponding contractor people to go and address that problem from a standpoint of how do we resolve it. And then independent of that, using some of the same people, however, we have a whole flight readiness review process to determine if this particular problem is enough of a concern that we should not fly. That is done in parallel, and actions come out both in our flight preparation process that we described the other day, as well as in our program process where we go through that analysis. And I hope as we go through this today you will see some of that come out.

But we have those two major paths that are followed up.

DR. WALKER: It might be useful to be sure that that documentation is available and in place in

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case you might need it.

MR. MOORE: You will see, Mr. Walker, the program side of this whole question of concern about O-rings today. We have laid that out in very great technical detail today, and we will tell you the actions that the program has taken in terms of trying to get a handle on this problem and so forth. So you will see that as part of the data that is presented to you today.

CHAIRMAN ROGERS: Well, I have been urging members of the Commission not to interrupt, and I have been the worst offender.

So please go ahead.

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MR. WEEKS: I rather think that Roman II, Mr. Irv Davids, will be in better context, because his memo was followed up by this August 19th which Mr. Mulloy is going to speak to in great detail. And I think that will be a better context to put that whole memo in.

And then the next one is this briefing number three, which is the budget briefing material on August 21st, which was, as Mr. Moore said, around the August time frame is when we are putting together the budget for the following February 3rd, 4th, whenever it is submitted to the Congress.

And so this was a budget recommendation briefing that was going to the budget administrator on the 21st. It was made up on the 16th, as you can see there on the front page, and as it actually happened, this particular briefing because of the press of time did not get to the administrator in that particular case. A number of other briefings on the orbiter did get to him.

And so this is just a budget threat item that we in general tell the administrator about so that he isn't blindsided that we haven't told him of some threat that he may have to help us get money moved around in the agency to hold a tough problem together. Usually Jess Moore can handle it within his own resources, but

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quite often he cannot and the administrator has to jump into the situation.

Then on number four, which is September the 10th, which is Mr. Winterhalter's preparation for this monthly meeting I spoke to you about, this is just a checklist of the erosion of the particular case on 51-I; that he is telling Mr. Moore at our monthly meetings about what the problems may be.

And then December 10th is the monthly status report. You will see there that number two on that one, which is now—and now I'm on page 15, and there is the case to case, nozzle, O-ring charring or erosion. When you see "charring" you will see Mr. Mulloy's pitch. Erosion—if you will use those interchangeably. [Ref. 2/10-8]

We are a little sloppy. We all know charring and erosion are fundamentally different, but sometimes we get lax with the imprecise language.

DR. RIDE: Can I ask you a question? Back on number three—and I guess I am back on the budget threats. You've got this SRM O-ring charring listed as a potential budget threat. What sort of threat to the budget was it being considered as?

In other words, were people thinking of it as a threat because they needed lots more O-rings, or were they thinking of it as a threat because there was a

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potential redesign of the solid rocket? In other words, how serious a safety consideration was this and what kind of budget implications did it have?

I mean, when people were briefing this were they saying we may have a solid rocket design or redesign, or were they saying we need more O-rings?

MR. WEEKS: We had seen some of the alternates of the type of design that it might require, and some of them were quite livable in Mr. Moore's budget and some would have been very difficult to handle.

CHAIRMAN ROGERS: What about this one?

MR. WEEKS: Well, you see, as Mr. Moore stated, we really haven't figured out what this failure is, at least not to my knowledge, and so we haven't come down in any way on what——

MR. MOORE: Excuse me.

CHAIRMAN ROGERS: You're not answering the question.

MR. MOORE: Let me try and answer the question from a budget threat point of view. What we had under way, we did not have a safety of flight concern in our program area that said we should not fly the shuttle at this point in time.

We did look at this thing as being a long term, because we had a different design on the filament wound case, that because we were seeing some erosion, that we might have to change that design and it would be better off to change that design in the long term.

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So a program was put in place to look at the results of the filament wound case activity and some other tests to decide if we wanted to continue to fly with the erosion concern we had or whether or not we wanted to go back and redesign.

And that was the context of the budget threat and there was a question of how much money was in that thing. We didn't have a feel for particular money in there. We just said, we may have to go back and get good tests out of this filament wound case, and we may have to go back and redesign.

MR. WEEKS: Well, I think that if we could proceed and get past the New York Times thing and get into the genuine chronology, I think that would come

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through a lot better.

GENERAL KUTYNA: Before you do, Mr. Chairman, I would like to call your attention to page 17. And when we look at the Cook memo you have a statement that the failure of the seal would certainly be catastrophic, and it was stated that that was overstated.

And if you look at page 17, here's another group saying the same thing. It says: "Failure mode and causes," and then about the fourth of fifth box down, "failure effects summary."

MR. WEEKS: Now, this is the document that is the December 1982, and that is when it was signed by myself, on the 28th of March in 1983. The critical items list were changed from a one redundant to a criticality one period, which means the redundance was to some degree compromised.

GENERAL KUTYNA: My problem is the New York Times kind of problem. Here it said that Cook says it's going to be catastrophic and here is another guy says loss of mission, vehicle, and crew. Somehow we've got to be able to explain in the open session tomorrow why this is different from what you said.

MR. WALKER: What action was taken as a result of this analysis?

MR. WEEKS: As you will see over the time

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period, you will see in Mr. Mulloy's testing many, many things that were done as regards this O-ring seal deflection which he speaks to here as a problem.

MR. WALKER: Some changes were made?

MR. WEEKS: Changes were made and tests were done to identify how much erosion was liveable, how much deflection really occurs as a result of this CIL back three years ago.

Now, this critical items list, as you can see there at the bottom, here we were, after we had had eight static test firings, we had five flights. We had 180, 54 field and 126 factory joints that were tested with no evidence of leakage.

We also had the Titan III program, which is a single seal instead of a dual seal, and they had about a thousand joints that, to the best of our knowledge, had not had a problem.

MR. WALKER: It looks like the Titan seals have different characteristics.

MR. WEEKS: Well, in one of the critical things, it is not redundant. In one of the critical things, the dual is better.

DR. COVERT: Maybe it's a different pressure.

MR. WEEKS: I can't authoritatively speak to the comparative difference in the two joints. I haven't seen any

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numbers on that. We know what our deflections were.

DR. LUCAS: I think you said we changed something there. Are you speaking of the filament would case? I don't believe a change was made on the steel case flight, was it?

MR. WEEKS: I stand corrected.

CHAIRMAN ROGERS: Could I make one comment? We're not talking about the New York Times article now. The New York Times article called the whole thing to the public's attention. Now we're talking about the documents that you produced, and let's forget the New York Times.

We're not analyzing the New York Times; we're analyzing your documents. As far as I'm concerned, that is what we're talking about, and that is what we will be talking about tomorrow.

I mean, we're not here to decide whether the New York Times writes good stories or not. We're here because of the critical nature of the subject matter.

We're here to consider what NASA did in view of its own internal documents.

MR. WEEKS: If I could I think go to page 2, which is page 18 that shows the test program that was done, in which the O-ring withstood 1600 psi, which is the actual operating pressure. The test program

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withstood—and this is back now three years ago—that the O-ring can withstand four pressurization cycles before any damage to the rings can occur or did occur.

We had over 540 joints exposed to the pressurization levels at the mean operating pressure, which is essentially 1,000 psi, with no leakage past the primary O-ring.

DR. COVERT: With the liner and the insulation in? What was the configuration in the tests?

MR. WEEKS: In the liquid pressurization system, I'm quite sure that that is oil out of the factory in Utah, and there is no insulation.

DR. COVERT: So there's no insulation at all?

MR. WEEKS: None. This was a liquid test.

DR. COVERT: So it is just the steel shell?

MR. WEEKS: And the O-rings, et cetera.

DR. COVERT: Essentially an isothermal test?

MR. WEEKS: Yes. I'm sure it's at room temperature.

And so this is a document that is our——

DR. COVERT: Is this thing lying on its side or vertical?

MR. WEEKS: I think they're tested vertically.

A critical items list is, if it is deemed to

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be category one, and you will see that this was changing from category 1-R, which is redundant, to category 1, which meant the redundancy was not full. As you can see here, full redundancy exists—this is in the middle of the first paragraph: "Full redundancy exists at the moment of initial pressurization."

And that is of course a very critical time, because the pressure in the motor is coming up in about 600 milliseconds to the 900 psi. And this joint rotation—and the reason that this particular CIL was written was it was found that that joint does rotate, and in Mr. Mulloy's pitch you will see a detailed picture of the amount of rotation which lifts off the secondary seal to about 42 to 60 thousandths of an inch.

DR. COVERT: Is this natural frequency?

MR. WEEKS: I can't answer that. Can anybody answer that?

DR. COVERT: I would appreciate that information, because that's going to relate to the importance of the 600 milliseconds.

MR. WEEKS: We will get that for you.

DR. COVERT: Thank you.

CHAIRMAN ROGERS: Could I suggest that you make the answers to the Commissioner who asked the

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question.

MR. MOORE: Yes, sir.

MR. WEEKS: And so, a critical item one which has to be signed off at the associate administrator level is a safety of flight of the crew or the airplane or both, and therefore it is changing from 1-R to 1 or signing off a critical items list, Roman I.

DR. RIDE: So in late 1982 this was identified as a criticality one problem and signed off based upon tests and past performance and all that sort of thing. And then you had subsequent problems with the O-rings, or at least subsequent charring on the O-ring.

MR. WEEKS: Correct.

DR. RIDE: Did you go back and revisit the CIL?

MR. WEEKS: Well, I think that you will see each step that we went through as we found each of the flights, Sally, that got different amounts of erosion. We were in effect re-reviewing this document as to whether it was liveable or not.

MR. MOORE: Let me add, Larry Mulloy, you might comment on that, because each program element in the shuttle is required to go back after an anomaly and carry it out through the entire project.

MR. MULLOY: None of the data really changed.

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It changed the basis for the acceptance of it as a criticality one item, but all of the data that we received in terms of the joint rotation and the reason we were getting the erosion—so yes, we did look at that, and we felt the margins we were seeing—and I will explain some of this—during the time that it takes to fill the gap between the primary and the O-ring, that it is an acceptable situation. And I have no data today to change that.

CHAIRMAN ROGERS: Not even today?

MR. MULLOY: No, sir.

(Viewgraph.) [Ref. 2/10-10]

MR. WEEKS: Now, if I could go on to the Titan experience. And here is the history as we best understand it on the Titan, that it is a design similar. But, whoever asked the question whether it was identical or not——

CHAIRMAN ROGERS: Could I go back to your last answer? Are you suggesting that you have come to the conclusion that these things did not cause the accident?

MR. MULLOY: Sir, I'm not aware of anything that has caused the accident yet.

CHAIRMAN ROGERS: Well, I asked you if you would have a concern today, based upon what happened, and you said no.

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MR. MULLOY: Sir, I said I have no data that changes the basis for that being a criticality one item. The thing that changed it from a criticality one redundant to a criticality one is still valid today.

CHAIRMAN ROGERS: I don't think that anybody who would hear that could understand it. Could you explain it so that the public would understand it?

MR. MULLOY: Yes, sir. If I could get to my charts, I will explain what happens during this rotation and why we think that it is a criticality one but the design is safe, given the criticality one design, and redundant.

DR. COVERT: Mr. Chairman, might I suggest that some of these things might be resolved by data? And I would hope that sooner or later we're going to get some numbers on these things, so we can get a feel for what they are. And I would suggest that some of these judgments might be withheld until we have some numbers.

CHAIRMAN ROGERS: Yes, I think what I was suggesting is we want to be careful that NASA doesn't suggest by his answer that nothing has changed. That would be a devastating comment. I think the answer to that is, we're not sure yet, that is what we're studying.

MR. MOORE: Yes, sir, I think that's exactly right.

MR. CULBERTSON: The thing that hasn't changed, Mr. Chairman, is that this is still a

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criticality one item. It is not a redundant system. That is what that piece of paper says. It placed it in a non-redundant category, causing it to get a lot more attention than would have been the case if it is a redundant item, and it remains that way.

DR. FEYNMAN: If I understand what "criticality one" means, it means it is important for safety of the flight. Is there a higher category than that?

MR. WEEKS: No.

DR. FEYNMAN: So that a failure of criticality one doesn't mean it was safe.

MR. WEEKS: It means if there is a failure of a criticality one item you can live through it.

DR. FEYNMAN: It doesn't mean there can't be any failures, of course. I heard someone suggest that something was criticality one and you would fly with it. So we still have to discuss later, I presume, the evidence that this criticality one was sufficiently unlikely or something that we could fly with it.

MR. WEEKS: I'm going to suggest, Mr. Chairman, that I finish these two or three charts on Titan. And then what I think would be more meaningful is that the chronology is there in your documents, and I think that if we went to the technical briefing of Mr. Mulloy immediately after that, we will get to the

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heart of this matter much quicker and most everybody in this room will be happy.

CHAIRMAN ROGERS: I think that's a good idea. Proceed.

MR. WEEKS: Okay. So just to tidy up the Titan thing. Now, General Kutyna may stand me corrected. The 26 ground test, which is the only place you will see any of the charring—and there was very little. And think we looked up something, and the worst one that existed was 10 thousandths on the single O-ring on the Titan, and there are 20 of the five-segment. That was the earliest version.

There were four of the seven-segment, which never went into production, but was just a development in the laboratory, and then two five and a half segments, which was a way of getting a little additional performance. And I believe every one of them flying now is the five and a half segment device.

And there is not any leakage, but there was this 10 thousandths. And there have been 77 flight tests, in which we have used 154 motors, and over 800 joint experiences.

MR. WALKER: Are you arguing the Titan experience applies directly to the shuttle experience?

MR. WEEKS: I am inferring that in that CIL

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that was signed three years ago, that was a germane thing that gave us some degree of confidence that we could proceed to sign up on it.

MR. ACHESON: Question: Would there be stresses set up by differences in design between the Titan and the shuttle flight assembly which would produce different types of rotation and different values for rotation?

MR. WEEKS: It certainly would, I'm almost certain, even though I do not know of their rotation numbers.

MR. ACHESON: Could those be simulated on the ground in any effective way?

MR. WEEKS: They certainly on those 26 tests—and I don't know whether maybe General Kutyna can tell us, but I'm sure they instrumented these and you can measure it on the ground. That's your best thing to do.

MR. ACHESON: My question, though, is whether the differences in the stresses in rotation between the two systems could be measured on the ground?

MR. WEEKS: I believe they could.

GENERAL KUTYNA: Let me address this. In talking to the space division, if they were on the stand right now they might say that their joint was stiffer

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than the NASA joint and therefore the rotation was not as great as the NASA joint, and so the joints should not be compared as essentially the same.

MR. WEEKS: I wouldn't quarrel with that. But when I signed that document, of course, I did not know the details of the Titan joint.

(Viewgraph.) [Ref. 2/10-11]

This is the detail of the single joint of the Titan, and it shows here, this is the inside. The centerline of the motor is here, and this shows the single one fitting down in, when 850 psi comes down and it pushes up against that O-ring.

But it is similar, of course. This detail is very similar to what you will see in Mr. Mulloy's pitch, similar, but I accept General Kutyna's point that the amount of rotation could be slightly different.

GENERAL KUTYNA: Mike, let me point out what the differences would be. They say it's a beefier joint, it's longer, it may be heavier, and actually there is a crosshatch section or actually a compression and they have to sit for a while before they can get the pins in, to the extent that almost all of the putty is squeezing out of it. So there is very little putty within those two surfaces.

So it is a compression joint, versus the kind of joint that you will see on the shuttle, which is an open one.

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MR. WEEKS: Well, General Kutyna, I sure wouldn't expect the stiffness coefficients of this insulation to be the same overall.

GENERAL KUTYNA: No, not very much.

MR. WALKER: Is that insulation ceramic?

MR. WEEKS: I assume that insulation is rubber.

MR. McDONALD: I'm pretty sure it is NBRO. I don't know. I didn't put it in.

(Viewgraph.)

MR. WEEKS: Now, this chronology, which is in your second frame, let me tell you what it is. But I think that if we went to Mr. Mulloy, which has extensive detailed numbers of calculations and so forth, I think we will be better off in this chronology, and then let us decide to come back to that.

CHAIRMAN ROGERS: All right, let's go to Mr. Mulloy. And do you want to swear him in.

(Witness sworn.)

Th^ Shuttle Inquiry: Flight Safety 'Compromised'

NASA HAD WARNING OF A DISASTER RISK POSED BY BOOSTER

ENGINEERS FEARED LEAKS

Internal Reports Cited Erosion of Rocket Seals — Agency Declines to Comment

By PHILIP M. BOFFEY

Special to The New York Times

WASHINGTON, Feb. 8 — The space a---cy was warned last year that seals space shuttle's solid-fuel booster s might break and cause a catastrophic accident, according to documents from the agency's files.

The documents show that engineers at the headquarters of the National Aeronautics and Space Administration and its Marshall Space Flight Center in Huntsville, Ala., were concerned that leaks might occur where segments of the booster rockets are mated.

Such leaks would allow hot gases and flames to escape through the side of the rocket instead of through the nozzle that channels the gases out the rear, possibly causing severe damage to the shuttle or an explosion, according to space experts.

A Stark Warning

One NASA analyst warned in an internal memorandum last July that flight safety was "being compromised by potential failure of the seals." He added: "Failure during launch would certainly be catastrophic."

A 1982 "critical items list" for the booster also warned that if the seals

Navy divers began an effort to examine and retrieve underwater fragments from the shuttle. Page 31.

should fail the result could be "loss of vehicle, mission, and crew due to metal erosion, burnthrough, and probable case burst resulting in fire and deflagration," or rapid, intense burning.

It is not clear what action if any, NASA might have taken in response to the engineers' memorandums, but the issue was still listed as a matter of concern in agency documents as recently as December.

Space agency officials declined today to respond to questions about the concerns raised in the internal documents.

Two Officials Are Informed

David W. Garrett, chief of the agency's news and information branch, was informed of the substance of those documents and said he had informed both L. Michael Weeks, deputy associate administrator of the office of space flight, and David L. Winterhalter, acting director of the agency's shuttle propulsion division. He said they had declined to comment.

Jesse W. Moore, associate administrator for space flight, the agency's top shuttle official, did not return a telephone call to his home, although a family member said he was there.

The internal documents describing problems with the seals were made available to The New York Times by a solid-fuel rocket analyst who has worked closely with propulsion engineers from the Kennedy Space Center in Florida, which assembles the booster rockets; the Marshall center in Alabama, which is responsible for their design, and NASA headquarters in Washington.

The Leading Theory

Although no one knows exactly what caused the explosion that destroyed the Challenger Jan. 28, space agency officials have said that the leading theory, based on films of the flight, is that a plume of flame emerged from one side of a booster and set off an explosion of the shuttle's giant external fuel tank.

Space officials have said they cannot identify precisely where the plume emerged and thus do not know whether it burned through a seam or through the metal side of the rocket. "It did appear to happen at least near a seam," Dr. William R. Graham, Acting Administrator of the space agency, said last Sunday. He said the plume appeared to start "near one of the field joints" but that measurements had not yet established whether the plume occurred "at the seam or just near the seam."

The safety of the seals also became an important issue Thursday at the first meeting of a Presidential commission that is investigating the causes of the accident. The space agency acknowledged that it had consulted with the rocket's manufacturer, Morton Thiokol Inc., about concerns that cold weather at the launching site might have weakened the seals. But an agency official told the commission the manufacturer had concurred that the launch should proceed.

The official, Judson A. Lovingood, deputy manager of shuttle projects at Marshall, also acknowledged that there had been concern after previous shuttle flights about erosion damage to some of the seals, but he indicated that this problem had been thoroughly investigated.

The seals are needed because the booster is not a single long structure but rather four large cylindrical segments that are bolted together, along with other components, at the Kennedy Space Center when the rocket is being prepared for launching. Although the side of the rocket may look leakproof to the naked eye, there is room for gases to escape at the seams. Thus rocket engineers have devised a series of seals and other barriers to keep the gases in.

The two most important seals are O rings, essentially large doughnut-shaped pieces of synthetic rubber that fill the tiny gap between two cylindrical segments that are bolted together. The O rings are themselves protected from heat and flame damage by an initial barrier of putty.

Tighter Under Pressure

If flames and hot gases are to escape through the joint between segments of the rocket, they generally must first pass through the putty, then through the primary O ring, and finally through the back-up ring. The rubbery O rings are designed to seal especially tight when they are hit by the high-pressure gases, much as a rubber washer on a faucet seals tight to prevent water from leaking.

At the Presidential commission's meeting Thursday, Mr. Lovingood, from the Marshall space center, was asked if experts had looked at the joints in the re-usable boosters after previous shuttle flights to see if there was any evidence of leakage. "We have seen some evidence of erosion of those seals, the primary seal," he said. "We've never seen any erosion of a secondary seal. But we have seen evidence of soot in between the two seals."

When asked if this was a cause for concern, he replied: "Oh, yes. I mean that's an anomaly and that was thoroughly worked, and that's completely documented on all the investigative work we did on that."

The possibility that cold weather might weaken the seals, by causing shrinking or stiffening or through some other effect, is not explicitly addressed in the internal documents. Instead, those memorandums focus on erosion and heat effects observed on the seals after previous flights. One memorandum does suggest, however, that "environmental effects such as moisture" could be an indirect factor in causing erosion.

[Ref. 2/10-1 1 of 3]

A memorandum prepared within comptroller's office at NASA headquarters last summer used dire terms to describe the potential problems of charring and erosion that might damage the effectiveness of the seals. The memorandum, dated July 23, 1985, was addressed to Michael B. Mann, head of of the resources analysis branch for the shuttle program, from Richard C. Cook, a subordinate.

Mr. Cook warned that "the charring of seals," which had been observed on recent shuttle flights, posed "a potentially major problem affecting both flight safety and program costs." In the joint between the nozzle section of the rocket and the adjoining segment, the memorandum said, "not only has the first O ring been destroyed, but the second has been partially eaten away." The memorandum did not say how often this had occurred.

The joint referred to in the memorandum is the one nearest the spot where the plume of flame was seen to emerge from the side of the rocket just before the explosion that destroyed the Challenger.

The memorandum said the cause of the erosion problem had not been determined. "There is little question, however, that flight safety has been and is still being compromised by potential failure of the seals," it said, "and it is acknowledged that failure during launch would certainly be catastrophic."

The memorandum said the leadership of the space flight program "is viewing the situation with the utmost seriousness."

Another memorandum prepared at roughly the same time by Irving Davids, an engineer in the shuttle rocket booster program at NASA headquarters, described a visit he made to the Marshall Space Flight Center on July 11, 1985 to discuss "seal erosion problems" that had affected the O rings on several shuttle flights.

A Dozen Instances

This memorandum said there had been "12 instances during flight" where there had been some erosion of the primary O ring at the seam where the nozzle segment of the rocket is bolted to the adjacent segment.

The memorandum said that in two cases soot actually blew by the primary seal, and in one case the backup seal showed erosion as well. This appears to contradict Mr. Lovingood's assertion that no erosion of a secondary seal had been observed.

The document added that the prime suspect in causing the erosion was the type of putty used. It said Morton Thiokol, the manufacturer of the booster rocket, believed that the putty, made by another unidentified manufacturer, could develop holes under certain conditions and that these holes would have a "jetting affect," an indication, apparently, that the holes could focus hot gases on the seal. "There doesn't appear to be a validated resolution as to the effect of the putty," Mr. Davids wrote.

The memorandum also described erosion of the O rings at the joints between other major segments of the rocket. It said there had been five occurrences during flight where the primary ring showed erosion and one case where the back-up ring was affected by heat although not actually eroded.

Problem With Back-Up Ring

One critical problem, it added, was that rotational forces generated as pressure builds up within the rocket caused a "lifting off" or "unseating" of the secondary ring, a problem which "has been known for quite some time." One proposal for eliminating this problem, the memorandum said, was a "capture feature," not otherwise described, which would prevent the seal from lifting off.

The memorandum from Mr. Davids was addressed to Mr. Moore, the associate administrator for space flight.

Through the rest of the year, the O rings continued to be a concern to some engineers and budget analysts.

On Aug. 21, 1985, a budget briefing prepared for top-level NASA officials listed charring of the rings as one of the top "budget threats" to the solid-fuel booster program, apparently a reference to the fact that fixing the problem could become costly.

On Sept. 10, 1985, a status report and briefing prepared by NASA's propulsion division said that the most recently completed shuttle mission showed "one minor erosion" on the primary ring at the joint between the nozzle and the adjacent segment but no such damage at the other joints. It also listed charring of the rings at the top of a list of "solid rocket booster issues."

In December 1985, a monthly status report again listed ring charring as one of seven issues regarding the booster.

Concerns about the seals had been expressed in agency documents at least as far back as 1982. A "critical items list" for the solid-fuel booster rocket, dated Dec. 17, 1982, described the joints as in the most important category.

The document also said that "joint rotation" as the pressure rose in the rocket might knock out the back-up ring. This was the same problem that, according to Mr. Davids's memorandum, had still not been solved in July 1985, although it had been "known for quite some time."

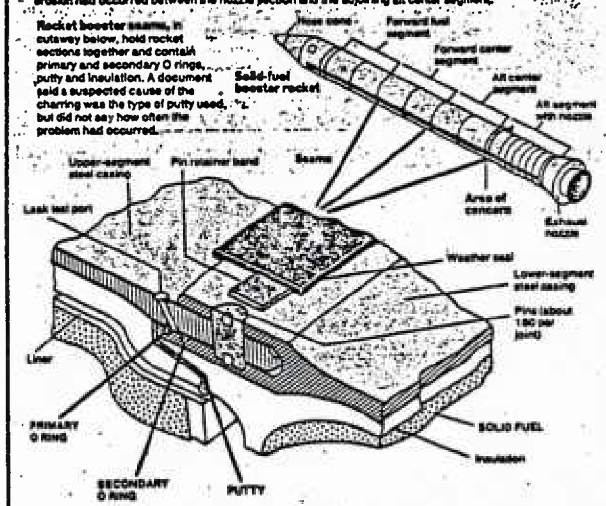
[Ref. 2/10-1 2 of 3]

NASA Had Warning of Booster-Caused Accident

Critical Seams Between Rocket Booster Segments

Documents show that seams between segments of the space shuttle's booster rockets were causing problems last year and indicated they might force the rockets to break on launching with catastrophic results. Important parts of the seams are synthetic rubber O rings that are protected from flame by a barrier of putty. If any flames or heat do escape through the joint, they must do so by first burning through the putty, then the primary O ring, then the secondary O ring. One document said charring and erosion had occurred between the nozzle section and the adjoining aft center segment.

Rocket booster seams, in cutaway below, hold rocket sections together and contain primary and secondary O rings, putty and insulation. A document said a suspected cause of the charring was the type of putty used, but did not say how often the problem had occurred.



[Ref. 2/10-1 3 of 3]

THE NEW YORK TIMES - FEBRUARY 9, 1986

"THE SHUTTLE INQUIRY: FLIGHT SAFETY COMPROMISED"

THE DOCUMENTS REFERENCED

I. NASA HEADQUARTERS, COOK MEMORANDUM OF JULY 23, 1985

II. NASA HEADQUARTERS, IRVING DAVIDS' MEMORANDUM OF JULY 17, 1985

III. BUDGET BRIEFING MATERIAL OF AUGUST 21, 1985

IV. NASA HEADQUARTERS PROPULSION DIVISION MONTHLY STATUS REPORT - SEPTEMBER 10, 1985

V. NASA HEADQUARTERS PROPULSION DIVISION MONTHLY STATUS REPORT - DECEMBER 1985

VI. SRB CRITICAL ITEMS LIST REQUIREMENTS - DECEMBER 17, 1982

[Ref. 2/10-2]

STS - SOLID ROCKET MOTOR O-RING CHRONOLOGY

4	SEPTEMBER 1980	SPACE SHUTTLE PROGRAM RESPONSE TO VERIFICATION/CERTIFICATION ASSESSMENT - SPECIAL STAFF RECOMMENDATION - PROPULSION
9	DECEMBER 17, 1982	SRB CRITICAL ITEMS LIST
12	MARCH 30, 1984	STS 41-C FLIGHT READINESS REVIEW
16	MARCH 30, 1984	STS 41-C PROGRAMMATIC ACTION ITEM
17	MARCH 1984	PROPULSION DIVISION MONTHLY REVIEW
18	MAY 1984	PROPULSION DIVISION MONTHLY REVIEW
19	NOVEMBER 1984	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW
20	DECEMBER 1984	PROPULSION DIVISION MONTHLY REVIEW
21	DECEMBER 1984	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW
22	JANUARY 1985	PROPULSION DIVISION MONTHLY REVIEW
23	JANUARY 1985	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW
24	FEBRUARY 1985	PROPULSION DIVISION MONTHLY REVIEW
25	FEBRUARY 1985	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW
26	FEBRUARY 21, 1985	STS 51-E FLIGHT READINESS REVIEW
28	JUNE 1985	PROPULSION DIVISION MONTHLY REVIEW
29	JUNE 18, 1985	GENERAL MANAGEMENT STATUS REVIEW
31	JUNE 28, 1985	NOTE TO MR. WEEKS FROM MR. WINTERHALTER
36	JULY 2, 1985	STS 51-F FLIGHT READINESS REVIEW

[Ref. 2/10-3 1 of 2]

STS - SOLID ROCKET MOTOR O-RING CHRONOLOGY (CONT'D.)

38	JULY 1985	PROPULSION DIVISION MONTHLY REVIEW
42	JULY 1985	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW
46	JULY 17, 1985	IRVING DAVIDS MEMORANDUM TO ASSOCIATE ADMINISTRATOR
48	JULY 22, 1985	RUSS BARDOS MEMORANDUM TO MR. WINTERHALTER
50	AUGUST 1985	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW
53	AUGUST 15, 1985	STS 51-I FLIGHT READINESS REVIEW
55	AUGUST 19, 1985	MSFC/MTI PRESENTATION TO NASA HEADQUARTERS
56	AUGUST 21, 1985	SRB FY 1987 BUDGET RECOMMENDATION TO ADMINISTRATOR
58	AUGUST 26, 1985	MTI PRELIMINARY SRM NOZZLE/FIELD JOINT SEAL CONCEPTS
134	AUGUST 26, 1985	PAUL HERR MEMORANDUM TO MANAGER, SRB PROJECT (MSFC)
135	AUGUST 30, 1985	MTI PROGRAM PLAN FOR IMPROVEMENT OF SPACE SHUTTLE SRM MOTOR SEALS
151	SEPTEMBER 1985	PROPULSION DIVISION MONTHLY REVIEW
152	SEPTEMBER 1985	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW
154	OCTOBER 2, 1985	MTI ENGINEERING STUDY OF O-RING COMPRESSION SET
172	NOVEMBER 1985	PROPULSION DIVISION MONTHLY REVIEW
174	NOVEMBER 18, 1985	STS 61-B FLIGHT READINESS REVIEW
176	DECEMBER 11, 1985	SRM O-RING TASK FORCE STATUS AND QM 5 RECOMMENDATIONS
198	DECEMBER 1985	PROPULSION DIVISION MONTHLY REVIEW
202	DECEMBER 1985	ASSOCIATE ADMINISTRATOR MONTHLY REVIEW

[Ref. 2/10-3 2 of 2]

MEMORANDUM 7/23/85

TO: BRC/M.Mann

FROM: BRC/R.Cook RC

SUBJECT: Problem with SRB Seals

Earlier this week you asked me to investigate reported problems with the charring of seals between SRB motor segments during flight operations. Discussions with program engineers show this to be a potentially major problem affecting both flight safety and program costs.

Presently three seals between SRB segments use double O-rings sealed with putty. In recent Shuttle flights, charring of these rings has occurred. The O-rings are designed so that if one fails, the other will hold against the pressure of firing. However, at least in the joint between the nozzle and the aft segment, not only has the first O-ring been destroyed, but the second has been partially eaten away.

Engineers have not yet determined the cause of the problem. Candidates include the use of a new type of putty (the putty formerly in use was removed from the market by EPA because it contained asbestos), failure of the second ring to slip into the groove which must engage it for it to work properly, or new, and as yet unidentified, assembly procedures at Thiokol. MSC is trying to identify the cause of the problem, including on-site investigation at Thiokol, and OSF hopes to have some results from their analysis within 30 days. There is little question, however, that flight safety has been and is still being compromised by potential failure of the seals, and it is acknowledged that failure during launch would certainly be catastrophic. There is also indication that staff personnel knew of this problem sometime in advance of management's becoming apprised of what was going on.

The potential impact of the problem depends on the as yet undiscovered cause. If the cause is minor, there should be little or no impact on budget or flight rate. A worse case

COPY.

[Ref. 2/10-4 1 of 4]

-2-

scenario, however, would lead to the suspension of Shuttle flights, redesign of the SRB, and scrapping of existing stockpiled hardware. The impact on the FY 1987-8 budget could be immense.

[Ref. 2/10-4 2 of 4]

It should be pointed out that Code M management is viewing the situation with the utmost seriousness. From a budgetary standpoint, I would think that any NASA budget submitted this year for FY 1987 and beyond should certainly be based on a reliable judgment as to the cause of the SRB seal problem and a corresponding decision as to budgetary action needed to provide for its solution.

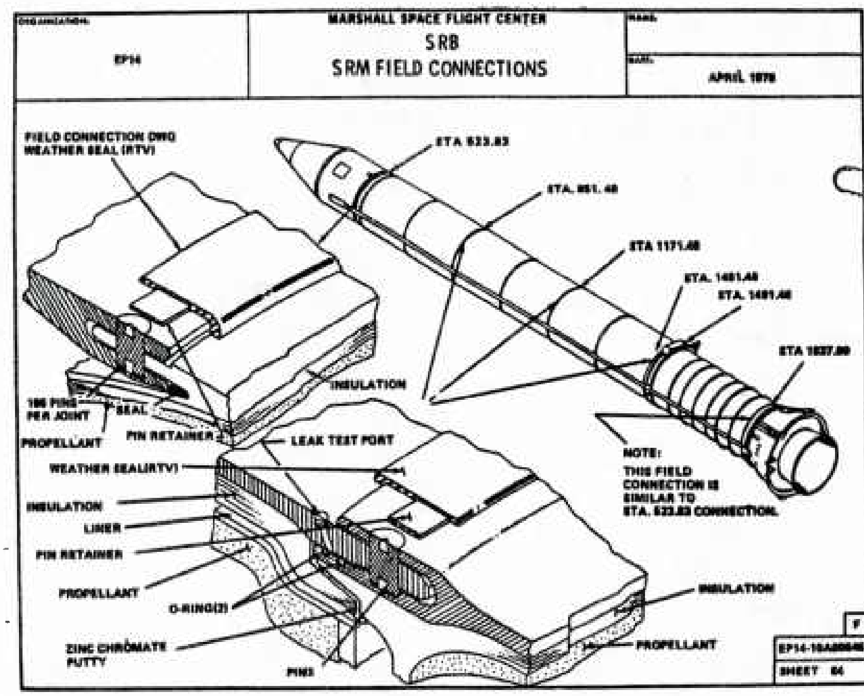
Richard C. Cook
Program Analyst

Michael B. Mann
Chief, STS Resources Analysis Branch

[Ref. 2/10-4 3 of 4]

Gary B. Allison
Director, Resources Analysis Division

Tom Newman
Comptroller



[Ref. 2/10-4 4 of 4]

NASA
National Aeronautics and
Space Administration

Washington, D.C.
20546

JUL 17 1985

Reply to Attn of: MPS

TO: M/Associate Administrator for Space Flight

FROM: MPS/Irv Davids

SUBJECT: Case to Case and Nozzle to Case "O" Ring Seal Erosion Problems

As a result of the problems being incurred during flight on both case to case and nozzle to case "O" ring erosion, Mr. Hamby and I visited MSFC on July 11, 1985, to discuss this issue with both project and S&E personnel. Following are some important factors concerning these problems:

A. Nozzle to Case "O" ring erosion

There have been twelve (12) instances during flight where there have been some primary "O" ring erosion. In one specific case there was also erosion of the secondary "O" ring seal. There were two (2) primary "O" ring seals that were heat affected (no erosion) and two (2) cases in which soot blew by the primary seals.

The prime suspect as the cause for the erosion on the primary "O" ring seals is the type of putty used. It is Thiokol's position that during assembly, leak check, or ignition, a hole can be formed through the putty which initiates "O" ring erosion due to a jetting effect. It is important to note that after STS-10, the manufacturer of the putty went out of business and a new putty manufacturer was contracted. The new putty is believed to be more susceptible to environmental effects such as moisture which makes the putty more tacky.

There are various options being considered such as removal of putty, varying the putty configuration to prevent the jetting effect, use of a putty made by a Canadian Manufacturer which includes asbestos, and various combination of putty and grease. Thermal analysis and/or tests are underway to assess these options.

Thiokol is seriously considering the deletion of putty on the QM-5 nozzle/case joint since they believe the putty is the prime cause of the erosion. A decision on this change is planned to be made this week. I have reservations about doing it, considering the significance of the QM-5 firing in qualifying the FWC for flight.

[Ref. 2/10-5 1 of 2]

It is important to note that the cause and effect of the putty varies. There are some MSFC personnel who are not convinced that the holes in the putty are the source of the problem but feel that it may be a reverse effect in that the hot gases may be leaking through the seal and causing the hole track in the putty.

Considering the fact that there doesn't appear to be a validated resolution as to the effect of putty, I would certainly question the wisdom in removing it on QM-5.

B. Case to Case "O" Ring Erosion

There have been five (5) occurrences during flight where there was primary field joint "O" ring erosion. There was one case where the secondary "O" ring was heat affected with no erosion. The erosion with the field joint primary "O" rings is considered by some to be more critical than the nozzle joint due to the fact that during the pressure build up on the primary "O" ring the unpressurized field joint secondary seal unseats due to joint rotation.

The problem with the unseating of the secondary "O" ring during joint rotation has been known for quite some time. In order to eliminate this problem on the FWC field joints a capture feature was designed which prevents the secondary seal from lifting off. During our discussions on this issue with MSFC, an action was assigned for them to identify the timing associated with the unseating of the secondary "O" ring and the seating of the primary "O" ring during rotation. How long it takes the secondary "O" ring to lift off during rotation and when in the pressure cycle it lifts are key factors in the determination of its criticality.

The present consensus is that if the primary "O" ring seats during ignition, and subsequently fails, the unseated secondary "O" ring will not serve its intended purpose as a redundant seal. However, redundancy does exist during the ignition cycle, which is the most critical time.

It is recommended that we arrange for MSFC to provide an overall briefing to you on the SRM "O" rings, including failure history, current status, and options for correcting the problems.

Irv Davids

Irving Davids

cc:
M/Mr. Weeks
M/Mr. Hamby
ML/Mr. Harrington
MP/Mr. Winterhalter

[Ref. 2/10-5 2 of 2]

SOLID ROCKET BOOSTER

FY 1987 BUDGET RECOMMENDATION

TO THE ADMINISTRATOR

(AUGUST 21, 1985)

AUGUST 16, 1985
11

[Ref. 2/10-6 1 of 2]

SOLID ROCKET BOOSTER

BUDGET THREATS

- FWC STA TEST PROGRAM
- FWC REIMBURSEMENTS (7TH FLIGHT SET AND SUBS)
- SRM "O" RING CHARRING
- ACHIEVEMENT OF PLANNED TURNAROUND IMPROVEMENTS
- DEFINITION OF FWC REQUIREMENTS
- BUDGET BASED ON SRM 3RD BUY PROCUREMENT OF 90 FLIGHT SETS
- POTENTIAL SRM DUAL SOURCE PROCUREMENT

[Ref. 2/10-6 2 of 2]

PROPULSION DIVISION
SOLID ROCKET BOOSTER

- STS-51-I
  - ASCENT PERFORMANCE
    - SRM PERFORMANCE CHARACTERISTICS WITHIN SPECIFICATIONS
      - BURN RATES HIGHER THAN PREDICTIONS

	PREDICTED	ACTUAL
LH	.373	.375
RH	.371	.375

    - ALL SUBSYSTEM OPERATIONS NOMINAL (TVC, E&I, SEPARATION, RECOVERY)
      - ONE SLUGGISH CHUTE
  - POST FLIGHT HARDWARE INSPECTION
    - BOTH BOOSTERS IN GOOD CONDITION
      - NO REPORTABLE NOZZLE ERUSION (<.250")
      - NO STRUCTURAL DAMAGE
      - NO SEGMENT FIELD JOINT "O" RING DAMAGE
        - ONE MINOR EROSION ON NOZZLE TO CASE PRIMARY "O" RING

- FWC
  - STA RE-TEST SCHEDULED FOR JANUARY - NO IMPACT ON MARCH VAFB LAUNCH ANTICIPATED
  - HI PRODUCTION TO CHANGE TO 3-8-7 TO SUPPORT V-2 LAUNCH IN SEPTEMBER 1986 AS A RESULT OF DIVERTING (2) FLIGHT SEGMENTS FOR STA RE-TEST
  - POTENTIAL OF RESCHEDULING QM-5 TEST FOR NOVEMBER 1985

[Ref. 2/10-7]

PROPULSION DIVISION
MONTHLY STATUS REPORT
DECEMBER 10, 1985

[Ref. 2/10-8 1 of 2]

SOLID ROCKET BOOSTER
ISSUES

- FWC STA FAILURE
- CASE TO CASE/NOZZLE "O" RING CHARRING
- ~~ACHIEVEMENT OF PLANNED TURNAROUND IMPROVEMENTS~~
- CONTINUED AVAILABILITY OF ASBESTOS FILLED INSULATION
- ~~PARACHUTE SEPARATION~~ AT ~~WATER~~ IMPACT/NOZZLE SEVERANCE AT APOGEE
  - REEVALUATION OF RECOVERY SYSTEM REDUNDANCY
- APU GAS GENERATOR INJECTOR TUBE PROCESSING
- GREATER QC ACTIVITY NEEDED AT MTI

NEW ENTRY

[Ref. 2/10-8 2 of 2]

77
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Add

SRB CRITICAL ITEMS LIST

Sheet 1 of 2

Subsystem: SOLID ROCKET BOOSTER	Criticality Category 1 Reaction Time Immediate to Sec.
Item Code: 10-01-01 Item Name: *Case, P/N (See Retention Rationale) (Joint Assys, Factory P/N 1U50147 Field: 1U50747	Page: A-6A Revision:
No. Required: 1 (11 segments, 3 field joints, 7 plant joints) FMEA Page No. A-6 of MSFC-RPT-724 Critical Phase: Boost	Date: December 17, 1982 Analyst: Garber Approved: [signature]

Failure Mode & Cause: Leakage at case assembly joints due to redundant O-ring seal failures or primary seal and leak check port O-ring failure.

NOTE: Leakage of the primary O-ring seal is classified as a single failure point due to possibility of loss of sealing at the secondary O-ring because of joint rotation after motor pressurization.

Failure Effect Summary: Actual Loss - Loss of mission, vehicle, and crew due to metal erosion, burnthrough, and probable case burst resulting in fire and deflagration.

RATIONALE FOR RETENTION

Case, P/N 1U50129, 1U50131, 1U50130, 1U50185, ~~1U501473,~~ 1U51473 1U50715, 1U50716, 1U50717

A. DESIGN

The SRM case joint design is common in the lightweight and regular weight cases having identical dimensions. The SRM joint uses centering clips which are installed in the gap between the tang O.D. and the outside clevis leg to compensate for the loss of concentricity due to gathering and to reduce the total clevis gap which has been provided for ease of assembly. On the shuttle SRM, the secondary O-ring was designed to provide redundancy and to permit a leak check, ensuring proper installation of the O-rings. Full redundency exists at the moment of initial pressurization. However, test data shows that a phenomenon called joint rotation occurs as the pressure rises, opening up the O-ring extrusion gap and permitting the energized O-ring to protrude into the gap. This condition has been shown by test to be well within that required for safe primary O-ring sealing. This gap may, however, in some cases, increase sufficiently to cause the un-energized secondary O-ring seal to lose compression, raising question as to its ability to energize and seal if called upon to do so by primary seal failure. Since, under this latter condition only the single O-ring is sealing, a rationale for retention is provided for the simplex mode where only one O-ring is acting.

The surface finish requirement for the O-ring grooves is 63 and the finish of the O-ring contacting portion of the tang, which slides across the O-ring during joint assembly, is 32. The joint design provides an OD for the O-ring installation, which facilitates retention during joint assembly. The tang has a large shallow angle chamfer on the tip to prevent the cutting of the O-ring at assembly. The design drawing specifies application of O-ring lubricant prior to the installation. The factory assembled joints have NBR rubber material vulcanized across the internal joint faying surfaces as a part of the case internal insulation subsystem.

A small MS port leading to the annular cavity between the redundant seals permits a leak check of the seals immediately after joining segments. The MS plug, installed after leak test, has a retaining groove and compression face for its O-ring seal. A means to test the seal of the installed MS plug has not been established.

The O-rings for the case joints are mold formed and ground to close tolerance and the O-rings for the test port are mold formed to net dimensions. Both O-rings are made ~~for~~ of high temperature, low compression set fluorocarbon elastomer. The design permits five scarf joints for the case joint seal rings. The O-ring joint strength must equal or exceed 40% of the parent material strength.

B. TESTING

To date, eight static firings and five flights have resulted in 180 (54 field and 126 factory) joints tested with no evidence of leakage. The Titan III program using a similar joint concept has tested a total of 1076 joints successfully.

[Ref. 2/10-9 1 of 3]

SRB CRITICAL ITEMS LIST

Sheet 2 of 2

CX 23 Add

Subsystem: SOLID ROCKET BOOSTER	Criticality Category 1 Reaction Time Immediate to Sec.
Item Code: 10-01-01	Page: A-68
Item Name: *Case, P/N (See Retention Rationale) (Joint Assys, Factory P/N 1U50747 Field: 1U50747	Revision:

RATIONALE FOR RETENTION (CONT'D)

A laboratory test program demonstrated the ability of the O-ring to operate successfully when extruded into gaps well over those encountered in this O-ring application. Uniform gaps of 1/8-inch and over (TWR-13486) successfully withstood pressures of 1600 psi. The Hydroburst Program (TWR-11664) and the Structural Test Program (STA-1) for the standard weight case (TWR-12051) and the Lightweight Case Joint Certification Test (TWR-12829) all have shown that the O-ring can withstand a minimum of four pressurizations before damage to the ring can permit any leakage.

Further demonstration of the capability of joint sealing is found in the hydro-proof testing of new and refurbished case segments. Over 540 joints have been exposed to liquid pressurizations at levels exceeding motor MEOP with no leakage experienced past the primary O-ring. The only occasions where leakage was experienced was during refurbishment of STS-1 where two stiffener segments were severely damaged during cavity collapse at water impact.

A more detailed description of SRM joint testing history is contained in TWR-13520, Revision A.

C. INSPECTION

The tang -A- diameter and clevis -C- diameter are measured and recorded. The depth, width and surface finish of the O-rings grooves are verified. The surface finish of the tang is also verified. Characteristics are inspected on each O-ring to assure conformance to the standards to include:

- Surface conditions
- Mold flashing
- Scarf joint mismatch or separation
- Cross section
- Circumference
- Durometer

Each assembled joint seal is tested per STW7-2747 via pressurizing the annular cavity between seals to 50 ± 5 psi and monitoring for 10 minutes. A pressure decay of 1 psig or greater is not acceptable. Following seal verification by QC, the leak test port plug is installed with QC verifying installation and torquing.

D. FAILURE HISTORY

No failures have been experienced in the static firing of three qualification motors, five development motors and ten flight motors.

[Ref. 2/10-9 2 of 3]

PCIN
021CS

CR NO.
M22106L

SPACE TRANSPORTATION SYSTEM
LEVEL I
CHANGE REQUEST

PAGE 1 OF 1

ORIGINATOR
LA3/J. B. Jackson Jr.

16

RELATED CHANGES
S22106L

TITLE
SRB Critical Items List (CIL) Requirements

SYSTEM/ELEMENT(S) AFFECTED:

- / / Shuttle System
- / / Orbiter
- / / Space Shuttle Main Engines
- / / External Tank
- /X/ Solid Rocket Booster
- / / Launch and Landing
- / / System Software
- / / Crew-Related GFE
- / / Shuttle Carrier Aircraft
- / / IUS
- / / Spacelab
- / / Payloads
- / / Operations
- / / Other(Specify)

DESCRIPTION OF CHANGE: / / Mandatory / / Improvement /X/ Other(Specify) Waiver

This change identifies Criticality 1 critical items which do not meet the fail safe requirements of Paragraph 2.8 of the Space Shuttle Program Requirements Document, Level I, dated June 30, 1977. These critical items contain 3 new items and 5 items which were previously approved Criticality 1R by Level II Change Request S02106G and are now being reclassified Criticality 1.

SUBSYSTEM - SOLID ROCKET MOTOR

Subsystem - Electrical and Instrumentation

CRITICALITY	ITEM NUMBER	ITEM	FMEA NO.
1	1U50129	Case	10-01-01
1	1U50130		10-01-01
1	1U50131		10-01-01
1	1U51473		10-01-01
1	1U50185		10-01-01
1	1U50715		10-01-01
1	1U50716		10-01-01
1	1U50717		10-01-01

RECOMMENDED EFFECTIVITY: STS-6 and subs

WEIGHT IMPACT:	SCHEDULE IMPACT:	COST PER FLIGHT IMPACT:
None	None	None

COST	PROJECT	FY	FY	FY	FY	REMAINDER	TOTAL
DDT&E							
PROD.							
OPS. NONE							

REASON FOR CHANGE:
Critical items which do not meet Level I fail-safe requirements require submittal per NASA Headquarters letter MHR-7 dated February 21, 1979, which states that all waivers to Level I redundancy requirements be submitted to the Level I PRCB for review and approval.

FORWARDING AUTHORIZATION
/X/ Space Shuttle
/ / Other(Specify) PRCBD S22106L

SIGNATURE
Glynn Lunney

DATE
3/2/83

LEVEL I ACTION
/X/ Approved
/ / Approved with Revision, See Pages
/ / Disapproved

SIGNATURE
L M Weeks

DATE
28 MAR '83

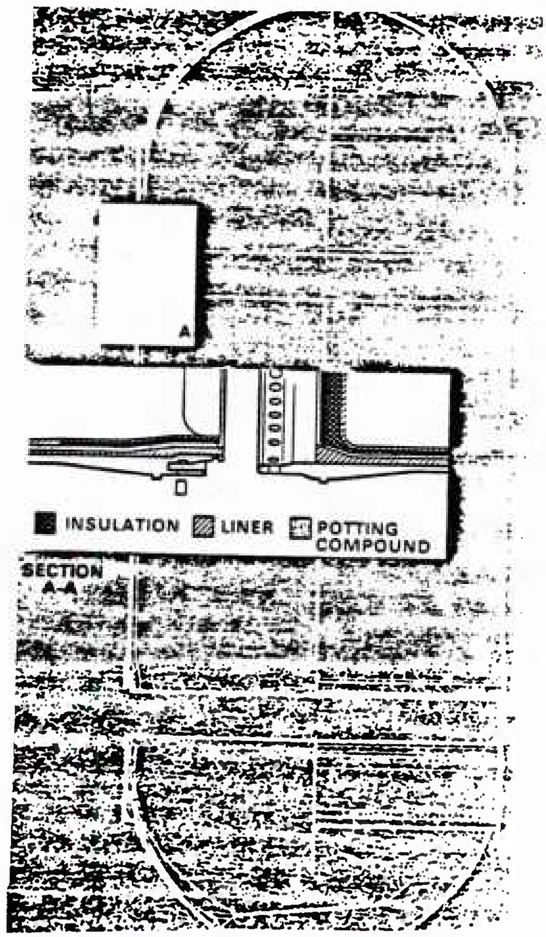
[Ref. 2/10-9 3 of 3]

SOLID ROCKET MOTOR

MAJOR TITAN III EXPERIENCE

- o JOINT DESIGN SIMILAR TO SHUTTLE ROCKET MOTOR
- o MULTIPLE JOINTS PER MOTOR
- o NO HISTORY OF JOINT PROBLEMS
  - o 26 GROUND TESTS
    - o TWENTY 5 SEGMENT
    - o FOUR 7 SEGMENT
    - o TWO 5 1/2 SEGMENT
    - o NO EVIDENCE OF ANY LEAKAGE
  - o 77 FLIGHT TESTS
  - o OVER 800 JOINT EXPERIENCES

[Ref. 2/10-10]



[Ref. 2/10-11]

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TESTIMONY OF LAWRENCE B. MULLOY

MR. MULLOY: Mr. Chairman and members of the Commission:

What I will give you today is an overview of the SRM case joints, some of the experience that we have had with those case joints, the erosion that has been experienced in the O-rings in those joints, and how we have addressed those as we have progressed through the program.

The CIL or critical items list document that has been under discussion as generated in December of 1982, we generated shortly after I took over the SRB program, where we had a recognition from the structural static testing that we had done at Marshall and some hydro-proof testing, where we actually measured the rotation of the joint, that we did determine that we did not have redundant seals, which was the initial design intent.

Now, Mr. Chairman, as you have asked if we can explain this in some terms that are understandable, I hope to be able to do that. The simple fact of the matter is that, due to this joint rotation, which I will explain, one of the seals is not effective, in that it is essentially lifted off its sealing surface.

And the rationale for the retention of that is the analysis and

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the testing that I will go through, as being we would like for it not to be a criticality one, we would like for it to be a criticality three. What is done in the process is to look, is there any practical way to make something a criticality three that isn't, and can you make it a criticality two; and if you can't make it a criticality two, which is simply loss of mission and no loss of life, then you make it redundant. And if you can't make it redundant, is it a reasonable risk to continue with the single failure of the system leading to a catastrophic failure?

MR. WALKER: Can I ask a question?

MR. MULLOY: Yes, sir.

MR. WALKER. Does that mean it's a single point failure, category one?

MR. MULLOY: Loss of mission and life.

MR. WALKER: It doesn't necessarily mean it has been a negative connotation?

MR. MULLOY: That is correct. It simply means you have a single point failure with no backup and the failure of that single system is catastrophic.

(Viewgraph.) [Ref. 2/10-12]

To orient you as to all of the joints that we have in the motor, what I have here is a profile of the motor. The two joints that I will be concentrating on because they are primary areas of interest to NASA and to this Commission are the Nozzle-to-Case Joint, where at the aft end of the motor case the nozzle is assembled into the motor case. This is the aft dome of the motor, and then the nozzle noses in. That is, it is bolted in with

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a bolt circle, and it goes around that aft segment.

The other joint—this is a factory-made joint. That joint is made at Thiokol.

The other segment joints that we show here are made at Kennedy Space Center, because we ship the motors as an aft segment with its fixed nozzle attached, we install the nozzle extension at KSC, and then have a center segment, a center forward segment, and then a forward segment.

Those joints are—these individual casting segments contain a joint also. That joint in between there is the same type of joint as the field joint. In fact, when we recycle the hardware what is a factory joint on one flight may become a field joint on another flight.

This joint we have never had any problem with, because we had the layer of insulation over that joint, because that joint is made prior to laying the rubber in the motor and casting the insulation.

The configuration of this nozzle to case joint is considerably different, as you can see by this diagram, than what we call the field joint, where we have this clevis. And we brought a section that is a little easier to see exactly what you're dealing with here. This is a section cut from a motor.

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This portion here is called the clevis, and this is the direction that it is assembled. The clevis is pointed up and the assembly is made. The tang end of the motor segment is aft. It is lowered into the clevis for the assembly.

There are two O-rings and two O-ring grooves that are in the clevis, and then there are pins of one inch diameter, high strength steel pins. There are 177 of these pins that go around the circumference, that secure the joint in place.

Then in between where the insulation is, in this area right here, is where the zinc chromate asbestos-filled putty goes, in here, as you can see in the diagram. These two O-rings in this case joint are 280 thousandths, .280 inches, in diameter. They are of Viton material, which is a Dupont fluorocarbon material.

We have a specification for the minimum squeeze, the squeeze being the compression on the O-ring when this is joined together. That minimum compression is 20 thousandths of an inch or 7.54 percent compression on that. That is a calculated squeeze. You can't measure it, and what we have done is measure the dimension of the clevis, the dimensions of the tang, and then we have assumed a minimum diameter O-ring, because

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there is a tolerance on it, of 280 thousandths minus 3 plus 5 thousandths.

So we assume the minimum diameter. We assume the maximum stretch on the O-ring, because there is also a tolerance on the stretch. And then we assume a certain, about eight percent compression set, because under that load the O-ring is sitting there for months in that configuration. And so when it is energized and the joint spreads, there is some compression set. So we don't count on that.

And we require a minimum squeeze on that O-ring. The purpose of having that minimum compression in that O-ring is such that when the motor is pressurized this putty in here will not take 1,000 psi. I don't know, we haven't found a putty that would sustain 1,000 psi.

So the gas has to go through the putty. The putty is there as a thermal barrier. What has been happening to us is, you might get a very small hole through the putty. So during that ignition transient when you're coming up to pressure, you essentially have a jet, a hot gas jet here.

At the same time, due to the pressurization loads, the joint is much stiffer than the rest of the case, and so this part of the case tends to expand more

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than the joint. Also, you're putting tension loads into the joint due to the pressure in the motor, and it's pulling on these pins, which tends to rotate the joint.

So when we speak of joint rotation in this presentation, what I'm talking about is during the pressurization cycle this inner clevis tends to spread in this direction, which tends to reduce the squeeze on the primary and secondary O-ring.

DR. FEYNMAN: I'm sorry to interrupt, but I think when you were explaining the origin of the tension that you pushed your hands the wrong way, which way the material bends.

MR. MULLOY: I'm sorry. Inside out.

GENERAL KUTYNA: Could I ask, how did you determine the number of pins you put in there? Are more pins better?

MR. MULLOY: Well, primarily it's based upon the pitch, the design standards for the pitch between holes in the structure. And we have 177 of those pins in there. There are three of them that are aligning pins, so there's really 180 of them, and they're divided at 2½ inches with 1-inch pins, and that's about as close as you want to go.

GENERAL KUTYNA: This is not meant to fit one against another. The Titan has 237.

MR. MULLOY: What size?

GENERAL KUTYNA: I don't know. They might be

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smaller.

DR. LUCAS: Larry, mention the clips. Do you want to mention the space there?

MR. MULLOY: Yes, we have—and this was introduced early in the design phase of the program. There is a 32 or 36 thousandth inch shim. And Mike did mention that there had been some changes made, and one of these was after some of the early testing. The concern was for getting a good squeeze on the O-rings.

At each pin we put one of these shims in here, the intent being to increase the squeeze on these two O-rings here. It's driven in with a leather mallet.

DR. FEYNMAN: That is in the 51-L?

MR. MULLOY: Yes, that has been in all the flights. That was incorporated early in the test program.

Now, there are other joints on the motor that I won't talk about today. One of them is the igniter joint. This is where the igniter assembly goes into the forward dome. We did have a little minor problem with that that concerned us.

We look very carefully at this hardware when we get it back. As someone mentioned earlier, this is the first for bringing back solid rocket boosters and

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reusing them, and it also provides a wealth of information and it provides some learning that we never had the opportunity to experience before.

But we did see a little minor erosion in this igniter. We tracked that to a problem where this is not an O-ring, it is a gask-O, what is called a gask-O seal. It's a metal seal that has a little rubber bonded to it, and we were finding that the manufacturers were putting an overfill condition in there and we weren't getting a good seal.

And we subsequently fixed that.

CHAIRMAN ROGERS: When did you discover that?

MR. MULLOY: That was DM-7, I believe, or DM-6. That was the fall of 1984. That was one that said, hey, we're seeing something here that we could go by the secondary part of that gask-O seal, and we still had a tertiary seal there, a third seal, and there's a donut seal around the bolts for any kind of a gas leak. So we cut down on the overfill.

There's another joint here we won't be talking about today, except in this context as I get further along. This is the filament wound case field joint. The filament wound case has steel rings, just like we have on the steel motors that we're flying out of Kennedy.

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But it has another clevis at the other end of it where a 1.6 inch carbon-phenolic material—or, excuse me, graphite epoxy material—phenolic's in the nozzle, but graphite epoxy material that is bonded into another or pinned into another clevis. And what we have there is a much greater joint eccentricity, so the rotation problem on this one is greater than it is on the steel case.

Here you will see something I will talk about later. We put what we call a capture feature, which is a little tang machined in here to keep this joint from rotating. That also can be applicable to a steel case.

MR. WALKER: I notice that where the O-rings are in the sample there's a gap between the pieces of metal. Is that how it is actually?

MR. MULLOY: Between the tang and the clevis, yes. And I will show you what the dimensions are. We are dealing with a 146-inch diameter motor case here, which is designed to be used 20 times. And they're not perfectly round after you have brought them back three of four times, and so there is a tolerance built in there to assure assembly.

MR. WALKER: Ordinarily with these O-rings there would be compression in the metal, between the metal groove and the clamp or contact.

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MR. MULLOY: You try not to have metal to metal.

MR. WALKER: Have you analyzed this difference?

MR. MULLOY: Yes, sir. We have done that in a lot of our testing. What it means is you have a cavity, and in this erosion situation, obviously, if you can get an interference fit and have and an interference fit on the insulation and eliminate the putty, it will be very hard to get any kind of flow down to the O-ring.

The inference of what you're saying is there is a volume here which takes in between the putty and the O-ring, and then this gap here which takes time to fill, which allows flow to impinge on the O-ring before it can be sealed.

The O-ring extrudes. This O-ring does not seat by compression. It seats by extruding into the gap, or seals by extruding into the gap.

DR. RIDE: If you were not going to reuse the SRB segments, would you have designed that to have metal to metal behind the O-ring?

MR. MULLOY: I still think you have to design it so you can put it together, and that requires a tolerance. And I think we have about the minimum tolerance that you can, even for single use. And I'm just saying that that gets exacerbated by continued

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use.

We use a rounding procedure at KSC by using a two-point pickup, a three-point pickup, or a four-point pickup, and let the motor hang until it meets its mating part. And the direct answer

is I think, even for a single use the tolerance should not be introduced you would have to count on it being able to be put together for a segmented motor.

Now, on the bottom down here is typical when you talk about O-ring erosion, and I wish I had time to bring an eroded O-ring with me. Possibly we can get one for you. This is a cross-section of that 280 thousandths.

And looking back here at the nozzle-to-nozzle case joint, I would point out another difference in the design of the nozzle-to-joint versus the case-to-case joint. The primary O-ring in the nozzle-to-case joint is what we call a bore seal, just like the case-to-case joint. But the secondary O-ring is a face sealer, whereas on the case joint they're not.

Now, the kind of erosion that we have been seeing, which our tests demonstrate is a margin of two or three less than what we can sustain and still seal the O-ring into the gap, is this type of thing here from the jet impingement.

Now, this is the exposed side of the O-ring

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looking into the gas flow, here or here, and then it erodes away. And the time that it takes to do that is about 300 or 400 milliseconds of that jet flow. Then the O-ring extrudes into the groove and the pressure equalizes and the flow stops, and I will tell you some more about that.

DR. COVERT: Is that always on the same part of the periphery?

MR. MULLOY: Generally yes, but I will show you some diagrams where we have had some rather severe erosion, where it doesn't tend to follow that. But generally it is right there where the gas jet is impinging right in this area.

DR. COVERT: I didn't ask my question correctly. There are also some fittings on this where the solid rocket motor attaches, and if I call that zero phase angle, because I don't know anything else to call it, then these erosion points are always in the same clockwise position?

MR. MULLOY: No. I will show you that. It has been one that we have been looking for: What is the common denominator? What causes some O-rings to erode and others not to, and is there a common denominator in anything, materials, process, putty, gap, reuse, particular joint, temperature?

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DR. COVERT: How about temperature gradient?

MR. MULLOY: I can't answer that.

MR. WALKER: What about the azimuthal extension of erosion? Is that always about the same?

MR. MULLOY: No, it is not, and I will show you a couple of examples. It generally, for a 32 mil erosion, the aspect ratio is usually about 100 to one, if they have 32 mils of erosion over a three-inch area.

(Viewgraph.) [Ref. 2/10-13]

Let me show you in some more detail now on the joints, the dimensions and so forth. This is our case-to-case joint. As I pointed out, we have 280 thousandths O-ring, plus 5 thousandths and minus 3 thousandths, with a design gap in there which is 10 thousandths, and the groove is .305 to .310 on the bore seals and the pin is one inch diameter.

As I said, there are 180 of them, although three of them are alignment pins and they don't have a retention pin in them. And then we have gap dimension criteria that the segment has to meet on inspection before we use that motor.

Now, what we have come up with, we're beginning to get what we call matched pairs. We're getting pairs where the clevis has spread to the point where we have to use the high side of a case that has a wide tang on it in order to meet these requirements.

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But we're very sensitive to that, and dimensional inspections are very thorough to assure that when we assemble a case-to-case joint we have a minimum of a 7.5 O-ring compression.

DR. COVERT: The drawing leaves the impression here that the inner side of the outer clevis is metal to metal.

MR. MULLOY: No, it is not. As I showed you in there, there is a shim in there. That is a graphics error.

GENERAL KUTYNA: You said the temperature had little effect?

MR. MULLOY: I didn't say that. I said I can't get a correlation between O-ring erosion, blow-by an O-ring, and temperature.

GENERAL KUTYNA: 51-C was a pretty cool launch. That was January of last year.

MR. MULLOY: It was cold before then, but it was not that much colder than other launches.

GENERAL KUTYNA: So it didn't approximate this particular one?

MR. MULLOY: Unfortunately, that is one you look at and say, aha, is it related to a temperature gradient and the cold. The temperature of the O-ring on 51-C I believe was 53 degrees. Is that right, Al?

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MR. McDONALD: Yes.

MR. MULLOY: We have fired motors at 48 degrees. 51-C was one of the worst case-to-case joint erosions.

GENERAL KUTYNA: And what was the temperature on 51-L?

MR. MULLOY: The temperature of the O-ring is calculated to be about 25 degrees.

GENERAL KUTYNA: Much, much colder?

MR. MULLOY: Yes. I think the coldest was 48 degrees on a static firing motor, if I recall.

DR. RIDE: Do you have a thermal model that you calculate that upon, based upon the ambient temperatures?

MR. MULLOY: Yes, it is based upon ambient and the temperature cycle. We have modeled the propellant and we have modelled the liner and the insulation and the steel case, and then, given a beginning environment, we can take a 24-hour period and calculate the mean bulk temperature and the temperature of the structure and get a temperature gradient.

VICE CHAIRMAN ARMSTRONG: Was the proximity of the external tank included in that model?

MR. MULLOY: Yes, it is. The effect is primarily through like a heat short in a strut, rather than a

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convective action. The temperature of that tank seldom gets below freezing on the surface itself. On very cold days you might get some ice formation on the tank, but that is not a normal condition.

Normally the temperature of the tank is above freezing.

DR. RIDE: Are you running this temperature model at Marshall pre-launch?

MR. MULLOY: Yes, we're running it at Marshall.

DR. RIDE: And you must have various temperatures that you check?

MR. MULLOY: During the count? No, we have not been doing that. You mean like 24 hours during the count? We do predict the temperature on the day of launch, but the thermal model is not, to my knowledge, is not run during the 24 hours to launch.

The launch commit criteria is that the vehicle can be launched in a 31 degree ambient environment, with a five degree sky. That is the LCC.

There is a TMX that explains how you apply that model. We have been looking at that and searching our certification and qualification on the motor. There are two thermal requirements: that the motor operates at 40 degree PMBT, propellant mean bulk temperature, the

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average of all of the particles in the propellant; and that it will operate in a 31 degree ambient environment, looking at a five degree sky.

DR. COVERT: Do you address how long?

MR. MULLOY: That is a TMX. I cannot answer your question. We are researching that now: How is that to be applied per the TMX? Rationally, in the absence of that, we have been discussing this since—I guess my opinion would be to start out with a 60 degree motor and you set it in a 31 degree environment until the propellant mean bulk temperature is 40 degrees, and that is the design environment. That is one rational approach to it.

DR. COVERT: What is the griddle-ductile transition temperature on the Viton motor?

MR. MULLOY: Since I've heard about two or three numbers in the last two days, I hesitate to quote one. Tests are being run, and the reason is tests are being run—and as you know, depending upon how you run a test, you will get different results, and there is a range of that right now.

The literature says one thing. There are some tests that are floating into the system that I think are wrong. So I just don't care to quote a number on what that is right now.

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DR. COVERT: But you're using a band?

MR. MULLOY: Yes, sir. I will tell you that the Mil Spec that Viton material is procured to requires that it operate at minus 30 to 500 degrees F.

DR. FEYNMAN: I don't really understand how this seal works, and let me explain what my question is. It says that you have .280, you might even have .277.

MR. MULLOY: That is correct.

DR. FEYNMAN: Now, it is round and sometimes it could be under compression for a while.

MR. MULLOY: We want it in compression all the time.

DR. FEYNMAN: So then we have a condition where, if you open it, it doesn't go back to round, it stays oval. And that is about ten percent.

MR. MULLOY: We use eight, sir.

DR. FEYNMAN: Now, use .28 and use 10.

MR. MULLOY: That's fine.

DR. FEYNMAN: That's easy to do. You have 10 percent of .280, and now it's only .252 wide now. It's compressed.

MR. MULLOY: I'm sorry, I misquoted. The compression set in the O-ring is 8 mils and not 8 percent. I apologize. There is 8 mils allowed in a compression set in calculating the minimum O-ring at the

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time it has to operate.

DR. FEYNMAN: I will go back and check, because I thought I saw a test which had ten percent, but I may be incorrect. Therefore that could solve my arithmetic problem. If the percentage change was that great, this would just fit, it would just touch. So I must be wrong.

But there is one more question that I wanted to make. When you have 900 or 700 pounds per square inch on the left and virtually nothing on the other side, how much do I have to have touching in order for this to work?

If it's only touching at a thousandth of an inch I would expect that it would go through. So can you tell me anything about how much excess I have to end up with so I will be able to hold?

MR. MULLOY: I hope I'm correct in this, but at 800 psi you don't have to have any of it in compression, because at 800 psi the O-ring has extruded into this gap and it seals by extruding into the gap and not by compression.

MR. LUCAS: Larry, would this be a good time to explain the pressure check between those O-rings?

MR. MULLOY: Yes, I think it is a very good time. I'm going to talk about it later.

We have a leak check port that is installed in

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the tang between the two pressures to the O-ring seals. In the pressure check we run, we take the pressure up to 200 psi and hold that for five minutes with an open source. And what we want to do is assure that we have seated the secondary O-ring in a position to extrude into the gap.

It unfortunately drives this O-ring in the wrong direction, and the reason we use 200 psi is because the putty that is in this joint out here, that I showed you on the previous diagram, we have found will mask a leaking primary O-ring at up to about 100, 150—well, 100 psi. At 150 psi it always blows through the putty. So the leak check is done by going to 200 psi with an open source, holding that for five minutes, and then doing a 50 psi leak check for ten minutes, with one psi pressure drop allowable during that ten minutes.

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GENERAL KUTYNA: On 51-L, for a matter of interest, the leak check on this one there was no pressure drop on this particular joint.

CHAIRMAN ROGERS: Could I just break in for a minute here to talk about our schedule? It's obvious you have a lot more information to present here, and we talked at lunch—some of us had lunch together—about repetitious testimony and we're going to have to do pretty much the same thing tomorrow, so I think there is something to be said for coming to a conclusion pretty soon and then starting again tomorrow.

We will have the whole day tomorrow. And I think if we continue much longer today that we will all be tired. I think that it would be helpful, at least for planning purposes, if you could tell us what you think you would like to present today to finish up and then we will have the whole day to present anything else tomorrow.

I think it would be a little repetitious if we go over it all twice, and I'm sure you feel the same way about it. Is there anything that you would like to tell us now that we should know about that it would not be wise to disclose in public at this time? We want to try to be sure that we don't do anything or say anything which injures or impairs your investigation, but, on the

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other hand, we want to disclose as much as we can in the public session.

Is there anything that we should not disclose tomorrow, as far as you know?

MR. MOORE: Certainly the O-ring is an area we're spending a lot of time investigating.

CHAIRMAN ROGERS: And everybody who is here today will be available tomorrow?

MR. MOORE: Everyone will be available tomorrow as well and, Bill, I don't know. Do you have anything else you want to add?

DR. LUCAS: I would suggest, if I might, that out of Larry's presentation you might want to skip over to the actions that we began taking in about June of 1985, because that goes across this correspondence that was referred to.

CHAIRMAN ROGERS: I think that would be very helpful. As much as you can when you come to charts and things of this kind, if you can explain it to begin with and then say this is reflected in the chart or whatever it is, because for a lot of people it is very difficult to understand what it is that you've done.

So if you could give the explanation as you go along and say what this means and that when we were told about this particular problem that we did the following

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things, and then we had a meeting and at that time it was decided that we would do so and so, and these are the reasons why we decided that.

MR. MOORE: I don't know what your thoughts are specifically on tomorrow, but I thought we would go back this afternoon and try to put a textual story together to give you some context, and then probably ask Larry to go through some of this stuff that he is doing here tomorrow.

CHAIRMAN ROGERS: I think that is good. Well, let's go ahead and you do whatever you think.

MR. MOORE: Larry, let me ask that you go through the actions that have been taken, the chronology of the Marshall actions that have been taken, and kind of a summary and conclusions and recommendations as part of yours. And then if you would like to stop at that point in time, we can, and then we can discuss what you think we ought to see tomorrow.

CHAIRMAN ROGERS: Well, it would be helpful to summarize what you think will be presented, but you can do it in more detail. As I say, the details we can wait and do tomorrow. But if you can summarize what it is you plan to present, then when we leave today I will have Mark explain that NASA provided the material we asked for, there was a full discussion of the matters

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contained in the documents, and we will continue to discuss that information in a public session tomorrow. Then we will start all over again and do it.

But you can make it a little bit briefer tomorrow. What we would really like to acknowledge—and I think we're getting a lot of very useful information in the chronology of things that concerns you—how you dealt with those concerns, who made the judgments about what went on. And I think if you can do that it will alleviate a lot of the problems that have developed.

DR. RIDE: Is there any internal correspondence on potential concern over the operation of the O-ring or the joint? Because I think that is probably the next thing. Since we've dealt with erosion, that's going to be the next question.

MR. MOORE: I will ask Larry. Do you know of any documentation at Marshall on O-ring operation at low temperatures? And I will ask McDonald at Thiokol if they know of any documentation on O-rings.

MR. MULLOY: There are documents that are test results that are even now in progress of some tests that have been done previously to understand the resiliency of O-rings at various temperatures.

DR. LUCAS: I believe also, Larry, that

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there was a discussion in close proximity to the launch between you and other people and Thiokol.

MR. MULLOY: Yes. Now that is not correspondence. What we did have on the evening of the 27th that came out in your first hearing—Dr. Lovingood mentioned that—when we stood down on the 27th it was known that the temperatures were predicted to be below freezing during the night and into the morning on the 28th.

As a matter of routine, those of us working the launch asked our technical people and our contractors what concerns we might have for low temperatures, and that was immediately after the stand-down. The only concerns that were presented to me were for the recovery battery temperatures. The recovery batteries are located in the forward skirt, the forward end of the solid rocket boosters.

And there was some concern expressed for the adequacy of the GN to gaseous nitrogen that went into the aft skirt of the boosters. That was analyzed and it was concluded we did not have a problem at the recovery batteries, that we would be above the red line limit on the recovery batteries at launch time, given the temperatures, and we did not have any problem with that amount of GN to the gaseous nitrogen purge going into the aft skirt, the primary concern there being because the temperature of the fuel service module contained

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hydrozene fuel that provided the power steering for the solid rocket booster.

At about 7:00 on the evening of the 27th I received a phone call from Stan Reinartz, who is my immediate supervisory, Stanley Reinartz, who is the manager of Shuttle Projects Office at the Marshall Space Flight Center, who works directly for Dr. Lucas, and he had been informed by our resident manager that Thiokol had looked at the conditions for the solid rocket motors and wanted to discuss the situation as they saw it for launch and what they were looking.

And Stan Reinartz and I went out to our resident office and we had a telecon. That telecon involved Reinartz, myself and a Thiokol representative and Al McDonald at KSC—Kennedy Space Center—a number of people at Marshall Space Flight Center, including the deputy director of engineering, Dr. Lovingood, who is Stan Reinartz deputy, and John McCarty, who is our deputy chief of propulsion, and probably eight or nine other people and probably a dozen or so people at Thiokol.

Thiokol presented to us the fact that the lowest temperature that we had flown an O-ring or a case joint was 53 degrees and they wanted to point out that we would be outside of that experience base.

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CHAIRMAN ROGERS: Who did that for Thiokol?

MR. MULLOY: That was the Director of Engineering for Thiokol, a gentleman named Bob Lund.

CHAIRMAN ROGERS: Were you there at the meeting?

MR. McDONALD: Yes, I was there.

MR. MULLOY: Also present at Utah was General Manager and Vice President of the Wasatch Division and manager of the space division at Wasatch—they have three divisions there; space division, tactical division and strategic division—all of the related propulsion people and the project manager, who is Mr. Joe Kilminster.

After hearing the discussion, we all concluded that there was no problem with the predicted temperatures for the SRM and I received a document from the solid rocket motor project manag-

er at Thiokol to that effect that there was no adverse consequence expected due to the temperatures on the night of the 27th.

DR. RIDE: I guess maybe what I'm asking is we read in the New York Times about NASA internal memos where people within NASA were suggesting problems with erosion before, and I guess I am wondering whether similar memos exist relating to problems of launching with the O-rings at low temperatures.

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MR. MULLOY: I'm not aware of any such documents at Marshall. That is not to say that there aren't any, and we will go research the files to see if there are any.

CHAIRMAN ROGERS: Can we then make it clear that we have requested such documents, if there are any, and that you will provide them?

MR. MOORE: Yes, sir.

CHAIRMAN ROGERS: Secondly, there was a report not about Thiokol but about I think Rockwell, that shortly before the launch, 20 minutes or something to that effect before the launch, someone from Rockwell called and expressed concern about the icing conditions. Do you have any recollection of that call and who made it?

MR. MOORE: As you asked Mr. Aldrich the other day, we had no direct contact with anyone from Rockwell who made that phone call. What we did have was, we did have—Mr. Aldrich held a meeting at about 8:00 in the morning of the launch—and recognize the launch was at 11:38—with a group of technical people who sat down and talked about the ice conditions down on the launch pad. And, as he reported the other day, Rockwell did express some concern initially at that point about some ice that may be coming off the launch platform and

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impacting the tiles on the orbiter—and that is the only tie that we have been able to find out that Rockwell may have had a concern.

CHAIRMAN ROGERS: Will someone from Rockwell be there tomorrow?

MR. MOORE: We will see if we can get someone from Rockwell there tomorrow. They are out on the West Coast. I will go back to the office and see if we can get somebody in from Rockwell tonight.

CHAIRMAN ROGERS: Well, I'm not sure it's essential, but I think it is important that we have an answer to that. If Rockwell was the one that raised the concern, then we want somebody from Rockwell to say I raised the concern, we talked it over, and my concern was satisfied and we said go ahead. As long as we still have that concern on the part of Rockwell, if you testify or someone testifies from NASA that there was the meeting and everybody was reasonably satisfied, then someone from Rockwell comes along and says that's not so, we told you not to go ahead and you went ahead anyway, that is the kind of thing we want to try to deal with at these meetings.

DR. FEYNMAN: I just want to go back to something that I would need to know in order to help me to determine what caused the accident rather than these

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other problems. When you were giving the thermal data, I've seen some thermal data which may be the same as you are talking about about the O-ring response to the compression set at different temperatures.

But the obvious question is how fast did it return, and I didn't see any data that told me it was millisecond, a second goes by; how much do temperatures vary. I mean, that is typically what a temperature does, is it changes an apparent viscosity, and I would like to get some idea if

the low temperature could have made it so that when things separated temporarily that the joints moved, that it did not do the usual thing and close the gap so quickly so as to let the gas go through.

This seems to me an interesting question and I would like to know as much information as you have on that.

MR. MULLOY: All right, sir, we will collect those test data.

CHAIRMAN ROGERS: On that, might I suggest that you deal directly with Dr. Feynman and give him that information. Then he will give that information to the rest of us. He can perform a very useful function because of his background and knowledge of analyzing that. After he's done that, then he will brief the Commission on his knowledge.

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MR. MOORE: Yes, sir. I have made notes where specific members of your Commission have requested data, and I plan to supply that directly to them.

DR. FEYNMAN: I have another request, if I can ask directly, and that is that the leak test point where you are putting pressure in at 200 psi and so forth that you are talking about, is it an escape route that is a principal possible for any gas which goes past the primary seal, and the secondary seal is no longer redundant for another reason; even if it was successfully maintaining the seal, if the gas were to be able to escape through that port because it was closed effectively, would you have also a danger?

Therefore, I would like to get information eventually, and I know you've got so many things to do, but eventually I will be asking about the technique for closing the leak port to be assured that it doesn't itself leak.

MR. MULLOY: Yes, sir. That is one of the trails.

MR. MOORE: Dr. Feynman, our task force which has been set up is trying to get all of the close-out photos and we will make all of that data available to the Commission, hopefully when they come to Florida.

MR. ACHESON: How many leak ports are there in a joint?

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MR. MULLOY: One per joint, and on the case joint it is located—one of them is on what is called the minus-Z, the side away from the orbiter, and on the other booster it is on the plus-Z, and they are all lined up on the case-to-case joints.

DR. FEYNMAN: On the right side booster which side is it on?

MR. MULLOY: On the right side it's on the minus-Z away from the orbiter, and the reason they are are clocked that way is we can interchange from left to right the boosters.

DR. LUCAS: But it is in proximity to where the news media photographed, that the leak port is on the same place where the flame was seen.

MR. ACHESON: That is why I asked the question. I can't visualize whether the axis of the port is in the direction toward the tank or the orbiter, or where it is.

MR. MULLOY: The direction of the port is in a direction away from the bottom side of the orbiter on the right-hand booster.

DR. LUCAS: At about a 45-degree angle up?

MR. MULLOY: Yes, sir. And there's one in each joint.

DR. RIDE: So it is in the same area as the

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plume was seen?

MR. MULLOY: The same quadrant, I would say, the same 90-degree segment. I can't get any closer than that.

DR. COVERT: About the same distance from the nozzle-up?

MR. MULLOY: I cannot tell.

MR. ACHESON: I don't want to anticipate how it works, but at the Thursday meeting one of the data we asked for was data relating to what conditions bring about cracking of the propellant material in the motor and if one were to pursue, for example, the hypothesis of not a joint failure but a burn-through on the side, I would assume one would be interested in cracking.

MR. MULLOY: Yes, sir.

MR. ACHESON: So at some point we would like to see that, I don't know whether when we go to the Cape or at some time later, and I just wanted to remind you that that is a request outstanding.

MR. MULLOY: That is one of the trails that the team is following also. The propellant temperature on 51-L was about 56 degrees.

CHAIRMAN ROGERS: My thought is if it is agreeable to the other members of the Commission that we have this public session tomorrow and we will go as long

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as we can. We want to be able to obviously answer all of these questions, but then we take a day off and go to the Cape Wednesday night and be there Thursday and Friday.

MR. MOORE: Yes, sir. Our task force is preparing for the Committee to come.

MR. MULLOY: Mr. Chairman, Al McDonald from Morton-Thiokol wanted to make a point.

MR. McDONALD: I wanted to say a point about the meeting. That meeting was called by Thiokol and I got a telephone call to set up a meeting at the Cape, and the meeting was set up at the Cape and we tied Marshall in and Thiokol back in Utah about the concerns of the lower temperatures. The meeting was set up to send material on the fax so that people could review data and concerns and the basis for that concern.

That data was transmitted to Kennedy and also to Marshall from our office at Thiokol. Our Vice President of engineering, I asked him to give that briefing. He did that. The recommendation at that time from the data that was sent out from Thiokol was not to launch below 53 degrees Fahrenheit because that was our lowest acceptable experience base and did demonstrate some blow-by from a year ago and also we had some data that indicated the poor

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resiliency of response of the Viton seal to low temperatures, so that was the first transmittal of information saying you should be aware of that, and where the data was discussed.

GENERAL KUTYNA: You said not to launch below 53, and what was the actual temperature?

MR. MULLOY: The actual temperature predicted at that time, based upon Thiokol's calculations, was 29 degrees.

CHAIRMAN ROGERS: Could you stand up again and say that a little louder so we could hear it? I'm not sure we all understood what you said.

MR. McDONALD: What I said was when the concern of the predicted cold temperatures at the Cape were transmitted to our plant our engineers were asked to examine that and see if there was any concern about launching the SRM or any concern with any component. The people who were working the O-ring seal problem were concerned. They called me at the Cape and said we looked at these predicted temperatures. It looks like the O-rings are going to be very cold.

We have run some tests in the past few months that shows the resiliency of the O-ring is very sluggish at low temperatures. It's very hard. And we would like to review that information. And I said yes, I will set

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up a meeting to review that information. We need to review the information and recommend whether we want to launch or what temperature we're willing to launch.

I called Mr. Mulloy at Merritt Island at the Quality Inn and didn't reach him. We had the wrong number or something. And I called Mr. Cecil Houston, who is a resident office manager of Marshall at Kennedy and told him the concern, and he said fine, I will get everyone on the network. I have a four-wire system right off my office in the conference room and we will get all the proper parties involved.

You tell your plant to be available at 8:15 and we will transmit the charts and the data that you have both to Kennedy and to Marshall and I will contact Marshall and have the appropriate people there. He set that meeting up. I went to Kennedy. There was a group of people at Marshall and a group of people at Thiokol.

The material was a little bit late getting there. We waited for about a half hour and then finally the material was transmitted from Thiokol, and in that material was the data that we had on our erosion history and the fact that a year ago we did see some blow-by of the primary seals of the case joint. That was the lowest temperature we calculated the O-rings to be in the flight vehicle. That caused us some concern.

We also had run some resiliency tests on the

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O-ring where essentially they were squeezed in between the plates and we removed it very rapidly and the pressurization motor would not respond. We were concerned about what impact that might have in low temperatures and as a result of that our recommendation at the time was to not launch below 53 degrees, because we didn't know how much farther below that we could go and be in the acceptable range.

VICE CHAIRMAN ARMSTRONG: Was that ambient?

MR. McDONALD: That was the temperature of the O-ring. To calculate the temperature in the O-ring containing area based upon what the ambient conditions would be. And, of course, the steel does cool off fairly rapidly. But the liner is somewhat of an insulator so we could tolerate some temperature difference there for a while. But the O-ring finally gets to that temperature. That is what we calculated the temperature of the O-ring at launch a year ago because the local ambient temperature is actually higher than that.

But, if you recall, there were several days of very cold weather on that previous launch a year ago, and we calculated from that history what we thought the O-ring temperatures might be.

DR. COVERT: That was on 51C?

MR. McDONALD: That is correct.

DR. COVERT: What was that temperature?

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MR. McDONALD: We calculated it to be about 53 degrees Fahrenheit in the O-ring area.

GENERAL KUTYNA: And on this launch it was 29 degrees?

MR. MULLOY: No. That was the single dimension analysis that Thiokol had run during the discussion we had on the 27th. Since then we have run a multi-node thermal model and I believe it is 25 degrees. Bill, do you recall?

CHAIRMAN ROGERS: Before we come to that, I'm not sure I understand. Am I hearing you say that you recommended against launch and you never changed your mind?

MR. McDONALD: No, I did not say that. We did change our mind afterwards.

CHAIRMAN ROGERS: What brought you to that decision?

MR. McDONALD: Well, the data that was reviewed, NASA concluded that the temperature data we had presented was inconclusive and indeed a lot of the data was inconclusive because the next worst blow-by we had ever seen in a primary seal in a case-to-case field joint was about the highest temperature we had launched at, and that was true—the next worst blow by.

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DR. RIDE: Which one was that?

MR. McDONALD: I can't remember exactly. I have it in my notes.

MR. MULLOY: I have it here.

MR. McDONALD: That was true. We did not calculate the effects of all that from the data that we had, but we did have some data that indicated that the timing function of the O-ring seal was going in the wrong direction, in the direction of badness. The O-ring was getting harder. The grease in there was getting more viscous. The time to seat the O-ring took longer and it would be more difficult to extrude it because of the hardened O-ring.

We didn't know exactly where the right temperatures were that would make it so it could not seal, but it was in the wrong direction. And the temperatures that were being reported for the 51-L were so much away from our experience base that we didn't feel comfortable operating that far away.

MR. MULLOY: I don't remember which one it is. We could get it. But one of these it was 75 degrees.

MR. McDONALD: That is 22-A. If you will look at the double asterisk, we saw a certain amount of soot in the primary O-ring even though we didn't see any

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erosion. Apparently some gas got past the primary O-ring between the two O-ring seals.

CHAIRMAN ROGERS: Could I say that on this—well, go ahead.

DR. FEYNMAN: It's just a matter of understanding and I want to be clear that I understand that you said you launched at a temperature "that was below 50 degrees Fahrenheit". This was presumably a temperature in the neighborhood of the O-ring?

MR. McDONALD: Yes.

DR. FEYNMAN: At the same time some estimate was being made as to what the temperature in the neighborhood of the O-rings would be as a consequence of the weather at that site at the time of the launch?

MR. McDONALD: Right.

DR. FEYNMAN: The first figure was 50 degrees. What was the second figure?

MR. McDONALD: The 53 degrees was what the O-ring was from a year ago that had the problem.

DR. FEYNMAN: I don't want that.

MR. McDONALD: We calculated, on the projected temperature that was given to us from the Cape, between 26 and 29 degrees Fahrenheit would be the O-ring temperature.

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DR. FEYNMAN: In other words, to make it absolutely transparent to me, you are saying that you said, at least at that time, that you didn't want to launch if the O-rings were below 50

degrees and, secondly, you made an estimate, in view of the history of the weather, that the temperature of the O-rings might be as low as 26 or 30 degrees?

MR. McDONALD: That is correct.

DR. FEYNMAN: Therefore, below 50 degrees.

MR. McDONALD: Well below.

DR. FEYNMAN: I just want to be sure. I got the 53 mixed up.

MR. McDONALD: I'm sorry if I mixed you up.

DR. FEYNMAN: So as far as 51-L is concerned, 50 degrees was your first statement, and I'm going to find out what the change in their mind was in a minute. But there were two numbers that didn't match—the 50 that you said you shouldn't fly and the distinctly below 50 they did fly. I just wanted to be sure that I understood.

CHAIRMAN ROGERS: I still don't understand your explanation. Did you change your mind?

MR. McDONALD: Yes. The assessment of the data was that the data was not totally conclusive, that the temperature could affect everything relative to the seal. But there was data that indicated that there were

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things going in the wrong direction and this was far from our experience base.

The conclusion being that Thiokol was directed to reassess all the data because the recommendation was not considered acceptable at that time of the 53 degrees. NASA asked us for a reassessment and some more data to show that the temperature in itself can cause this to be a more serious concern than we had said it would be. At that time Thiokol in Utah said that they would like to go off-line and caucus for about five minutes and reassess what data they had there or any other additional data.

And that caucus lasted for, I think, a half hour before they were ready to go back on. When they came back on they said they had reassessed all the data and had come to the conclusion that the temperature influence, based on the data they had available to them, was inconclusive and therefore they recommended a launch.

CHAIRMAN ROGERS: When you say inconclusive, what does that mean?

MR. McDONALD: Well, the fact is––

CHAIRMAN ROGERS: You told them the day before not to do it and now you've got some more data and you say its inconclusive and so you changed your mind?

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MR. McDONALD: I was not back at Wasatch when that discussion was being held. I was at Kennedy and I do not know what other data they were looking at other than the charts that I had in front of me and others had in front of them at both KSC and Marshall. I do not know. I do know they came back on and said they had reassessed it and concluded that it was OK to launch, and at that point in time Thiokol was requested to put that in writing.

CHAIRMAN ROGERS: Well, I think in view of the very serious nature of this and the fact that it will be scrutinized for years that we should have precisely what the data was before we present it.

MR. McDONALD: I have that in my notes, sir.

CHAIRMAN ROGERS: Well, you are just conveying information that pertains to a decision somebody else made.

MR. McDONALD: I have the Fax's that were distributed at both of those meetings in my book that were transmitted, all of the charts from the original meeting and the one afterwards.

CHAIRMAN ROGERS: Who made the decision from Thiokol?

MR. McDONALD: I do not know who made the final decision. I do know that the fax was signed by Mr. Joe Kilminster, my boss, the vice president.

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DR. WALKER: So there's no evidence that the evidence was looked at in this caucus that persuaded Thiokol that your first view was incorrect, that perhaps the first view wasn't based upon solid evidence?

MR. McDONALD: I cannot say specifically about that because I was not there to see it, and I think you need to get people that were at that meeting to discuss that.

CHAIRMAN ROGERS: I'm sure you can see the logic of what you're saying. You recommended against a flight on one night and then you have meetings with NASA people and they seem anxious to go ahead, or at least they were asking questions about it, and they gave you

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some data and you checked back with your home office and you got word back from home office to go ahead because the evidence is inconclusive.

DR. COVERT: I believe, if I'm correct, he said that there was first evidence of the low temperature in 51-C, where there was soot but no erosion. And this was a cold launch. This was a 50-degree launch. And you went back to the plant and talked to them and subsequently they were looking around for other cases, if I understand what you said, and they found another case where there was soot, and this was A, which took place in 75-degree temperature.

So at this point the thing suddenly becomes less clear. Do I remember that correctly?

MR. McDONALD: That is correct.

DR. COVERT: So that the additional data, then, was the discovery of 61-A in leafing through the files, where they launched on a warm day and then they decided they didn't know.

DR. RIDE: They also said that they had the data on rebounding or whatever it was of the O-rings.

DR. COVERT: I just wanted to bring up what the other data seemed to be.

DR. FEYNMAN: It would seem to a rational person there might be some other reason why there was a

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blow-by at high temperature and that is there was a likelihood at low temperature that you could cause difficulty because of the slowness of response and, therefore, there must have been some technical discussion out there in the plant. And I wasn't quite clear.

You said you had some things with you about the rates of flow. Is that material available to us or could it be made available to us? I don't want to get involved with that as a specialist, but the consequences would be that just because the decision looks like it was technically sound, so I would like to know what kind of logic and so forth was gone into. Is it possible for me to have that?

CHAIRMAN ROGERS: Oh, sure.

DR. FEYNMAN: I would like as much detail on the logic or the apparent logic as was there at that time from that plant; is that OK?

CHAIRMAN ROGERS: Absolutely. And could I suggest that you give Dr. Feynman whatever he wants, but I would express the caution that it's hearsay as far as you're concerned. We really want to get the people before the Commission who made the decisions and ask them why did they appear to change their minds and ask them specifically. Unfortunately, you are just

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conveying information.

DR. FEYNMAN: He said something about having something.

CHAIRMAN ROGERS: He does have some data there from the people who made the decision, I guess.

MR. McDONALD: I have some of that, but I also have the material that was reviewed at the meeting by all three parties, because I was party to that.

CHAIRMAN ROGERS: Which meeting now are we talking about?

MR. McDONALD: This was the first meeting scheduled to review why we had any concerns about low temperature, and I have that as a matter of record.

CHAIRMAN ROGERS: Let's be sure that we make it clear if it's one meeting, two meetings, who was there, when was it held, because otherwise it gets all blurred in the minds of the listener, and I gather you had several meetings on this subject—two meetings, three meetings.

MR. McDONALD: Well, there were probably three meetings, yes.

CHAIRMAN ROGERS: Well, make that clear or have your people make that clear.

GENERAL KUTYNA: In your opinion what is the greatest indication of the problem—the amount of erosion or the fact that you have some soot, i.e.,

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something that has a lot of erosion like this? Is that more of a problem, than something that has a little soot and no erosion?

MR. McDONALD: I think soot was more of a problem than the erosion. That shows that you had violated the primary seal in some way to get gas between the two seals. The erosion itself, as long as you don't violate the seal it still has the integrity and is not a problem.

GENERAL KUTYNA: So on the coldest day of launch you had erosion and soot and on the hot day of launch you had very little erosion but you had soot?

MR. McDONALD: You had no erosion, but you had soot.

CHAIRMAN ROGERS: Okay.

MR. MOORE: Larry, why don't you go to the last couple of charts there on the program and then let's kind of summarize.

CHAIRMAN ROGERS: Why don't we take a short break?

(A brief recess was taken.)

CHAIRMAN ROGERS: If we could get started again, I will take just a moment, if I may, to introduce to the members of the Commission who don't already know him Al Keel, who is going to be our Executive Director. He comes with the highest qualifications. He was a

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student of Dr. Covert's, but, in addition, he has other minor qualifications, such as he is now and has been for three years—well, you go ahead, Al. You were over in OMB, the Office of Management and Budget.

MR. KEEL: As certainly most of the NASA people around the room have seen my bearded face and even seen it when it had less gray in it, I have been associate director of OMB for international affairs and national security, which deals with everything in the budget except Medicare, Medicaid, and NASA.

For about the last three years, and I've gone through four budgets and before that I was Assistant Secretary of the Air Force for Research, Development and Acquisition, and about the last honest work I did for a living was one minor part of doing some space shuttle aerodynamics about ten or 12 years ago.

But I'm delighted to be on board, Mr. Chairman.

CHAIRMAN ROGERS: I was hoping you weren't a doctor.

(Laughter.)

MR. KEEL: My educational credentials are bachelor of aerospace from the University of Virginia and then a doctorate at the University of Virginia, and

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then I did some post-doctorate work at UC-Berkeley, which I've hidden from our Administration. —(laughter.)—on high altitude aerodynamics.

CHAIRMAN ROGERS: We are very fortunate to have Al with us. He had made some plans to do other things, but because the White House wanted him to work with us and because he realized how important it was, they talked to him about coming with us. So we are very pleased. It looks like we picked exactly the right man, and he will now proceed to develop a staff as soon as he can.

We wanted to continue to get people with the highest qualifications for our staff because we feel it's very important.

Okay. Dr. Mulloy?

MR. MULLOY: Mr. Chairman, you heard Mr. Moore's suggestion that I go to the last three charts I had here that we laid out and have been doing.

(Slide.) [Ref. 2/10-14]

MR. MULLOY: To attempt to better understand the phenomena that you are seeing here with the occasional O-ring erosions, there has been a great deal going on up until August, but it was specific testing that was related to what had we observed on the last

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flight and what was our tolerance to that, and has anything happened that changed our rationale that we had from the CIF?

And when we see a particular occurrence we have run analyses and special tests to determine what is the limiting mechanism. Are we still in a situation where we're assured that we still have a safe crit one system? So they were kind of, in that period of time it was more or less ad hoc testing from flight to flight.

And then, as Mike Weeks explained in his presentation, around April, as we began to see more of this and decided what we needed was a systematic effort to see what would be a long-term permanent solution to this, as opposed to living with a limited type thing, we developed and presented in that August 19th briefing a plan that we have essentially followed since that time. And it was to better understand the mechanism and our tolerance to that mechanism and what limiting mechanisms might be in effect to be sure that they were still the same.

I will just describe these. I don't have any diagrams of them. But a cold flow test is essentially just putting air through a joint to understand flow in the joint and in the crevices to see what type, given the input flow, what type of flow we're getting at the O-ring. And we did that on the nozzle because the

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nozzle is subject to circumferential flow in the joint, as opposed to the case across the nozzle gimbals and thus gets pressure differential as the nozzle swivels. There is a delicate pressure from one side to the other which can be set up in that O-ring grove as a circumferential flow.

So we ran those tests to understand that, to support what Mark Salita has done in the modeling of this particular event. We decided that the case cold flow wouldn't tell us anything. We dropped those.

The other key point, as I mentioned, is the rotation of that joint. We only have two measured data points on what that rotation really is, but one of them was on our structural test article, which was the first structural test that we did at the Marshall Space Flight Center, and the other was on a hydro pretest pressure test of a lightweight case, and we got two different results there.

So we're going to run some tests to better understand it.

CHAIRMAN ROGERS: Can you tell us what the chart means? Looking at it, the average person doesn't know what it says. It looks like a series of tests. Can you tell us, not necessarily today, what the tests show?

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MR. MULLOY: Yes, sir, I can do that. I won't go any further with that one.

(Slide.) [Ref. 2/10-15]

MR. MULLOY: Then this chart just simply says that with that we have our last qualification motor, which is called QM-5, which was in the stages of assembly. It is a filament-wound case qualification motor and we wanted to take the opportunity for any improvements that we could make in this joint configuration to put more margin into this critical point system and to incorporate that into what is called QM-5. That motor is now assembled at Thiokol.

We had been planning to fire that on February 13. We have that on hold right now. But I will show you that out of all of this testing that was on that previous chart what the conclusion was as to a fix that didn't move us totally outside of the experience base that had taken us through 48 successful motor firings in flight and nine successful motor firings on the ground.

DR. COVERT: I gather that you are satisfied that the rotation isn't the source of the leaking, that this has moved out of the realm of a hypothesis into the realm of fact?

MR. MULLOY: The rotation is a major contributor to the source of leakage. I am satisfied

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with that. I don't know that it is the sole cause.

DR. COVERT: I guess I haven't seen any data one way or the other.

MR. MULLOY: That the rotation is or is not?

DR. COVERT: Yes. Is there some data we can look at?

MR. MULLOY: Yes. The data is that the minimum squeeze on the O-ring is 20/1,000ths of an inch.

DR. COVERT: How did you determine the separation distance when you pressurize this exceeds that 20/1,000ths?

MR. MULLOY: By having a hole drilled into the clevis and a direct measurement of the rotation at that point, a direct measurement under pressure and load. It is measured.

DR. COVERT: How many measurements?

MR. MULLOY: I'm not sure exactly on the hydroproof. There were several of those measurements. The mean measurement on hydroproof turned out to be 42/1,000ths rotation. The mean measurement on structural test article turned out to be 60/1,000ths.

DR. COVERT: This is a deflection really and not a rotation?

MR. MULLOY: It's the measurement of the spreading of the clevis from the tang; that is correct.

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DR. COVERT: And this is through the clevis and through the tang and to where the O-ring cavity is?

MR. MULLOY: That is correct, a direct measurement.

DR. RIDE: Have you calculated any distortion that you might expect in the joint as a result of the loads put on the SRBs before liftoff when it's still held down?

MR. MULLOY: Yes, that is calculated. The joint—from ignition the joint is always under tension within 600 milliseconds. The pressure overrides the compressive loads immediately after the ignition when the joint goes into tension, and the tension, of course, varies because when the SSME is ignited it tends to put pressure on one side, more pressure on one side than the other. But that is calculated and the distribution of that is known.

DR. FEYNMAN: This is just a minor technical point and I shouldn't even interrupt you, but don't you hold the outside with some kind of a skirt or something and the nozzle doesn't get any torque on it with winding?

MR. MULLOY: The skirt is held. The skirt on the motor is held at four points and the nozzle is attached to the aft dome of the SRMs, as I showed in that previous picture. But with the twang, yes, there

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is some response of the nozzle also.

MR. MOORE: Let me comment. That is one of the critical things that we're looking in terms of the post-flight analysis.

MR. HOTZ: I can't hear you.

MR. MOORE: Let me comment that we're trying to understand completely the loads at the time the SSME's came on the liftoff during the entire flight of 51-L is one of the primary tasks that our group is trying to work right now.

Johnson Space Center is working those loads as well as the other Centers to try and understand the effects of loads on this particular flight, and we should have some indication of the status of that when you come to Florida on Thursday. We're not sure we will have a complete loads analysis done, but we will certainly give you a status of where we are.

And the other thing we're looking at in addition to a loads analysis, as I said the other day, is a very detailed time line sequence of events of different kinds of forces on the flight as a function of time. So we will try to give you what we can on Thursday of what we have available.

DR. COVERT: I would like to ask one more question and that is this type of a test is vertical

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and, therefore, I assume this forward part is down and not the nozzle.

MR. MULLOY: It depends upon which segment you are testing. The closure is on the aft end and then there's a hydroproof dome put on the center segment joints in order to put pressure into the segment.

DR. COVERT: Do I misunderstand then when you hydro test that you have the whole assembly in one big long—

MR. MULLOY: Oh, yes. The hydro test is an individual casting segment, which is two segments with a factory joint.

DR. COVERT: So you have never done any pressure testing or any load measurement of this thing when it's all put together as opposed to just these?

MR. MULLOY: Yes. The structural test article was intended to simulate that. The structural test article is an aft segment, an external tank attach segment and a forward segment and a forward dome. And that had loads and pressure on it.

DR. COVERT: Is that vertical?

MR. MULLOY: No, sir, horizontal.

DR. COVERT: And you put tension on that to simulate the thrust?

MR. MULLOY: Yes, and we put 1,000 psi on it and then put the loads that, as Jesse was mentioning,

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the bendover loads, and we measure the joint deflection during all of those tests.

DR. COVERT: And you did that with the heavy gauge and the light casing?

MR. MULLOY: With the light case and the hydroproofing.

GENERAL KUTYNA: Mr. Mulloy, on the 17 July letter from Mr. Davids discussing the changes that might be incorporated in the test, there is a sentence that says, "I have reservations about incorporating the changes considering the significance of the QM-5 firing qualifying these cases for flight." Was that a schedule consideration?

MR. MULLOY: No. I haven't been able to go through all of the information that I have here, but that was a very correct reservation about some of the concepts that were being considered, and I think his reservation was that I hope none of these concepts get on the QM-5, and those concepts are not even programmed because they were not good ideas. I believe that is correct.

I want the QM-5 to represent what we have out at Vandenberg right now, which is the QM configuration and I will.

VICE CHAIRMAN ARMSTRONG: Just one further question. Would you tell us simply what was intended at

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the hydro testing? It seems to me it's primarily a load kind of test and it wouldn't tell you much about leakage or those kinds of considerations.

MR. MULLOY: The main driver, as I understand it in researching records on that, the main driver was that the lightweight case just took about 26/1,000ths of an inch off of the steel cases. The concern was that's going to increase joint rotation first because now you've got a wet membrane, the joint hasn't been changed and therefore you have more joint rotation. That was a driver.

And the other thing was just to run the ultimate load test to be sure that that case would take 140 percent of the maximum extended operating range.

VICE CHAIRMAN ARMSTRONG: So it is a loads deflection kind of measurement?

MR. MULLOY: Yes.

VICE CHAIRMAN ARMSTRONG: What is the configuration again of the test article for the hydro test?

MR. MULLOY: The configuration of the test article, it is mounted into the test fixture a segment, a segment that is shipped to Florida and assembled. And then it has a dome, a test dome that is put onto it, bolted and sealed, such as you would put pressure into

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the segment.

VICE CHAIRMAN ARMSTRONG: What's the other end?

MR. MULLOY: The other end is the test fixture. You just set the clevis down on the test fixture, seal it, and the closure end on the top is the forward dome of the motor.

VICE CHAIRMAN ARMSTRONG: So it is not considered necessary in this application to have case-to-case joint?

MR. MULLOY: That is correct. Or an alternate pressure test. We use the structural test article, the single test article, to get all the joints in, to put the combination of loads and pres-

sures on them to 140 percent of the limit load with extensive instrumentation, such as we could by test and analytical extrapolation qualify the basic structure.

And then each article goes through a hydroproof test before it is used and reused, which is 112 percent of the maximum expected pressure. And then we thin the walls down and we run 140 percent test on one article to measure the joint rotation and assure the pressure integrity of the thinned-out wall. We have been flying that since STS-8.

MR. WEEKS: Mr. Armstrong, the test he is speaking of has two sections in it and there is a case-to-case joint there, but it is a factory joint and

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it is covered with propellant when it is shipped to the Cape for launch.

VICE CHAIRMAN ARMSTRONG: Is it the same configuration as the cross section that we passed around here?

MR. WEEKS: Yes, it is the same, but it is not critical because it is covered with propellant and there is no joint, and wouldn't have putty or any of that sort of thing so it is all cast in a double-sided flank. Each of those sections of the big steel material is about 12 feet long and your whole thing is about 24 feet long when you ship it, but there is a joint there, but it is a factory joint and then covered with propellant.

VICE CHAIRMAN ARMSTRONG: I need to understand the question information that you extract from such a test is applicable to a case-to-case joint where it is not a factory joint and there isn't a propellant across there?

MR. WEEKS: Well, it is a joint, but, you see, there is no putty and no thing for the pressure to enter and break the seal.

MR. MULLOY: Dr. Armstrong, when that test is run, they would not know whether that was a factory joint or a field joint because at the time the test is run there is no propellant and insulation in there.

VICE CHAIRMAN ARMSTRONG: That was the point I

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missed.

MR. MOORE: It's tested and then taken out and then loaded and then shipped.

MR. MULLOY: Mr. Chairman, the question you asked, what is the bottom line of all of that, is that we concluded—we had planned to fire this QM-5 in December. I made the decision in September to hold that configuration until we could complete this testing to look at what would be the most feasible improvement that would go toward reducing the incidents of erosion and blow-by.

CHAIRMAN ROGERS: What was your conclusion?

(Slide.) [Ref. 2/10-16]

MR. MULLOY: The conclusion was that the best thing to do at this time—and we ran an assembly test to make sure we could do this, but the thing that constrains the size of the O-ring that you can put in a groove is that in putting it together that you don't shear the O-ring or do damage to the O-ring in putting it together.

We concluded through assembly tests that we could go up in the size of the O-ring in the case-to-case joint from .280 to .292, and we demonstrated that that could be accomplished. Now what that does, it increases the initial compression on the

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O-ring and enhances the capability for it to maintain a compression such that when the motor pressure hits it that it can extrude into the gap and give us a good primary seal.

Now if we have a good primary seal we now have a reliable, a good, reliable primary seal. The concern for having the liftoff situation where the secondary seal cannot be pressure actuated is reduced somewhat.

The other thing we did—and I can show you a picture here from all of that cold flow testing—is in the nozzle joint. This refers to a Turcon spacer, and Turcon is an unfilled Teflon. We found on this nozzle joint——

(Slide.) [Ref. 2/10-17]

MR. MULLOY:—that this groove had been made as wide as it is. Because this nozzle has to slide over this and there's an interference, almost an interference fit at this location. The design was made with a very wide groove there to allow that O-ring to move as you install the nozzle to assure that we didn't damage that O-ring.

We found that through assembly test that if we can put the nozzle on in the vertical instead of the horizontal, when you lay the motor case down in the horizontal it develops an ovality and now you're trying

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to mate a round part to an oval part. We found that if we would install the nozzle in the vertical we could reduce the size of this gap.

Now the effect that that has, we thought we would have a leak check port between the two seals such that when we run that leak pressure test at 200 psi and then 50 psi for ten minutes to validate that we have a good seal that tends to move this O-ring in the wrong direction.

This is the motor pressure side over here, and so now when the motor pressure comes through this putty and impinges on this O-ring that's sitting over here and it's not sealed—it has just got compression on it—that O-ring has to travel from this side of the groove to this side of the groove and then extrude into this gap. That is when you are getting your erosion, while that hot gas jet is impinging on that O-ring.

So what we concluded was put this Turcon spacer in there such that when we run the leak check we don't move this O-ring all the way back over to the other side of the gap, which now should reduce the incidence of erosion that we're seeing on the nozzle joints.

DR. FEYNMAN: Why? The same gas comes.

MR. MULLOY: But the time of blow-by is not as long.

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DR. FEYNMAN: There's blow-by either way?

MR. MULLOY: If you don't have initial compression there is, but if the O-ring has the 7.54 percent compression and it's over here, it is going to immediately extrude into the gap. If it is over here, it has got—1,000 psi has to move that O-ring to the other side during the time it leaks through because that compression is not adequate to hold 1,000 psi until the O-ring gets over here and extrudes in the gap.

So the bottom line of all that testing was these—putting a bigger O-ring in the joints and putting a spacer in this is the best solution of all of the testing that we did.

CHAIRMAN ROGERS: Is that what you did in 51-L, then?

MR. MULLOY: No, sir. 51-L is the same thing.

CHAIRMAN ROGERS: This is just a consideration for improvements in the future?

MR. MULLOY: That is what we had been doing for the last year, is running all of these tests that were on the bar graph to determine what kind of a fix might work to reduce the incidence of erosion.

CHAIRMAN ROGERS: This was for the future, though, and didn't affect 51-L?

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MR. MULLOY: It did not affect 51-L.

CHAIRMAN ROGERS: These are studies you had been making to try to improve it?

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MR. MULLOY: Yes, sir, and my point is we would never put a change on a flight vehicle before we qualified it.

DR. RIDE: This was just for the filament wound cases?

MR. MULLOY: No, I wanted to qualify it on the filament wound case, but the case-to-case joint other than the rotation is not any different—is not really any different than on the steel cases. The nozzle joint is identical.

DR. RIDE: So this really is something you are thinking of.

MR. MULLOY: What I wanted to do was qualify this on the QM-5. If I qualified it on QM-5, based upon the other testing that I have already done and the analysis that Mark has done, I would put the spacer in the nozzle joint, and I don't have to do anything. I don't have to get any metal parts or anything, and I can improve that O-ring situation. As far as the case joint, .292, if it would extrude into the gap on QM-5 where the joint isn't rotating, it would certainly extrude into the gap on steel, and I am

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putting a .292 O-ring in the seal.

DR. WALKER: What is QM-5?

MR. MULLOY: That's the last qualification motor on the filament wound case program. The nozzle-to-case joint on QM-5 is identical to the steel casing.

MR. MOORE: Let's get to the last couple of charts.

MR. MULLOY: That's it.

MR. MOORE: I understand that.

CHAIRMAN ROGERS: We will have plenty of time to study the other things you have been doing to improve things, but I think the focus of attention is going to be what caused this accident.

MR. MOORE: Sir, what we tried to do with this briefing today was to bring you all the documentation that I have available in my office that deals with the question of O-rings because that is a highly suspicious

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area. So we wanted to give the members of the Commission the data that we've got on the O-ring situation, and I think tomorrow that is something that you ought to talk about, I guess, after this briefing, what area do you think from the Commission's standpoint you would like to discuss tomorrow, and we will go back and work this thing.

Larry has got two more charts, and that's it.

(Viewgraph.) [Ref. 2/10-18]

MR. MULLOY: I think we can wind up with this one. This chart is the one right out of the August 19th briefing. This is general conclusions about what happens with O-ring erosion. That was good on August 19, and it is still good today.

DR. COVERT: Does it follow logically if you eliminate erosion or you eliminate soot?

MR. MULLOY: No, sir. As long as that O-ring has to move before it extrudes into the gap, you will have gas blow-by. If the gas burns grease in the process, you will get soot. If there is not much grease there, we have indications of blow-by where it has been clean.

DR. COVERT: Mark, is it your view that there would be blow-by——

MR. SALITA: No intuitively I would say not. There are going to be many times

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when you have enough pressure or enough O-ring squeeze that you will not have blow-by. We have already seen four instances of soot behind the O-ring, and we have fired 177 field joints so far, flight and test motors. Of the 171 we have looked at, we have only seen four cases of any evidence of soot behind the primary O-ring.

DR. COVERT: Well, if you could have blow-by without soot——

MR. SALITA: Well, what evidence do we have that we have any heat effect in the zone? We haven't seen any heat effect in the zone that I've seen.

MR. ACHESON: Could you explain why erosion in the nozzle joints is more severe due to eccentricity?

MR. MULLOY: Yes. I was explaining we assemble the nozzle in the horizontal. The nozzle is round. The case is laying in the chocks and tends to get oval, and so you are putting a round object into an ellipse, so there is eccentricity there.

(Viewgraph.) [Ref. 2/10-19]

MR. MULLOY: Now, the recommendations that were presented in August are these. This recognizes the concern that the lack of a good secondary seal in the field joint is most critical, and ways to reduce the joint rotation should be incorporated as soon as possible to reduce criticality.

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Now, what we did in response to that——

(Viewgraph.) [Ref. 2/10-20]

MR. MULLOY:—is even before that recommendation was made in August, in July I had moved out to procure larger billets for the motor. The way these motor casings are made, ladisch, a proprietary process, rolls these things with no welding on them, so we just told them to build the billets with three inches less ID so that we would have extra material inside such that we could machine a capture feature just like we have on the filament wound casing.

MR. FEYNMAN: None of these things went into 51-L?

MR. MULLOY: No. I was just responding to the recommendations of August, and we now will have the first of those parts delivered in 1986, in August of 1986, where we can reduce them, and respond to that recommendation to reduce joint rotation in the field joints.

(Viewgraph.) [Ref. 2/10-21]

MR. MULLOY: This recommendation was to go do that testing that we did in order to determine what the proper fix was and incorporate it in QM-5. It was recommended that we use the QM-5 or an alternate putty. It has nothing to do with a better putty. People who use asbestos are stopping making that material, and the

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reason we went from Fuller-O'Brien to Randolph was because Fuller-O'Brien wasn't making it and Randolph might quit making it. So we discussed what to do with the filament wound case in that first flight out of Vandenberg. But again, a recommendation to do additional testing, which we have done, that was the program plan I showed you, and the conclusion at that time was that the analysis of the existing data indicates that it is safe to continue flying existing designs as long as joints are leak checked with a 200 psig stabilization pressure, and are free of contamination in the seal areas, and meet the squeeze requirements.

DR. RIDE: How do you determine the contaminations?

MR. MULLOY: Well, that's a visual inspection. We go to great lengths to be sure that we keep the O-rings clean. The joints are initially greased and put into what we call the short stack. The aft assembly is put on the aft skirt and the motor and then moved over, and that is all degreased and inspected and then regreased, and great caution is taken to be sure that the surface is clean, and when we get the motors back we do a thorough inspection of that to assure that we haven't gotten any pitting due to the fact that they have been in the water. We take great care to be sure that we have got good seals. And then the O-rings are brought in to Wasatch, in Utah.

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There the O-rings are inspected, cleaned, greased, and then they are sealed, and they are not taken out of the bag until the O-rings are delivered at the assembly point to assure that we don't pick up any contamination.

DR. RIDE: Have you ever found water in any of the soot?

MR. MULLOY: It has been reported to me, and that is being verified because we have not found any documentation yet that indicates it, but it is reported that on STS-9 that we had to roll back and destack STS-9 because of the 8A nozzle erosion. It is reported that when they pulled the pins on that joint, that water came out of the joint.

It is also reported when they broke a segment open one time, that water came out of a joint. That is being—KSC is tracking that, and we are looking for any documentation that indicates that. That was reported to me after the 51-L accident.

DR. WALKER: That would be water trapped between the two O-rings?

MR. MULLOY: No, it would be water in the—if I went back to my joint that I ran, the clevice was up.

CHAIRMAN ROGERS: Go ahead, but I really want

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to get to a discussion of what we are going to do tomorrow.

What you are saying is we have conducted extensive tests of this type and that type, and we have concluded as follows, and these are the recommendations, but anybody there is going to be interested in what are the things that happened that relate to 51-L. I mean, others are going to say that this is a filibuster.

Now, you have told us now, and we will have a chance to ask a lot of further questions, but if we appear to be ducking the issue, the issue is what happened prior to the launch of 51-L.

DR. COVERT: Sir, it seems to me when we first sat down and went through the first letter, one of the concerns was have you fellows done anything to react to this—I don't want to use the word "threat" because of the discussion today—but this problem that may come up in the joint, and the last few viewgraphs here have been the result, as I understand it, of what they have done in the last year to try to make the integrity of this joint be more satisfactory.

CHAIRMAN ROGERS: Yes, I think that is good. But I just think we ought to shorten it.

DR. COVERT: I misunderstood what you said.

CHAIRMAN ROGERS: I think that is fine, but if

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you went through all of the things that Larry has gone through and then the last question is does any of this relate to 51-L, and the answer is no, everybody is going to say well why did you spend all of that time on it? I mean, he can say it and we can report it in our report and all of these things, but I think these last three charts were fine.

GENERAL KUTYNA: The next to the last bullet says it is safe to fly if you pass the 200 pound test.

Did 51-L pass the 200 pound test?

MR. MULLOY: Yes. There were no anomalies reported to me yet. In fact, I understand the people that did the psi leak——

DR. RIDE: Do you have closeout photos on that?

MR. MULLOY: Yes, we have gotten closeout photos in my task force at the Cape, and we also have had a very preliminary report on the leak check—the supervising of the leak check process out at Kennedy Space Center, and the preliminary indication is there were absolutely no anomalies in the leak check on this particular flight.

GENERAL KUTYNA: But Jess, do you see my dilemma, that it is safe to fly if you pass this test, and you passed the test?

DR. COVERT: Maybe the test we have been pursuing is not the source of the accident.

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DR. LUCAS: If it turns out that the O-ring is a problem, and we haven't determined that.

VICE CHAIRMAN ARMSTRONG: I am concerned, Mr. Chairman, in looking ahead at the perspective of the audience and the people listening, that we will have demonstrated that there was a concern in this particular technical area, but it wasn't deemed dangerous to fly, it was deemed safe to fly, but it was also deemed that it ought to be fixed and be better, and work was going on, which makes an understandable story to me, but I am not sure that it will be understandable to everybody else.

Why was it safe? You know, they are always trying to simplify points to the things, of give me a black or white or yes or no answer, was it safe or not? And if it was, why were you fixing it? And that concerns me a little bit.

MR. MOORE: It concerns us as well, too.

MR. FEYNMAN: If you look at this from a naive point of view, which many people will, I am just emphasizing his point that it was—presented this way, it does look terrible because if you juxtapose the first sentence, the first item, with the next to the last item, you find two sentences—and I am just emphasizing your point to make it more dramatic—that

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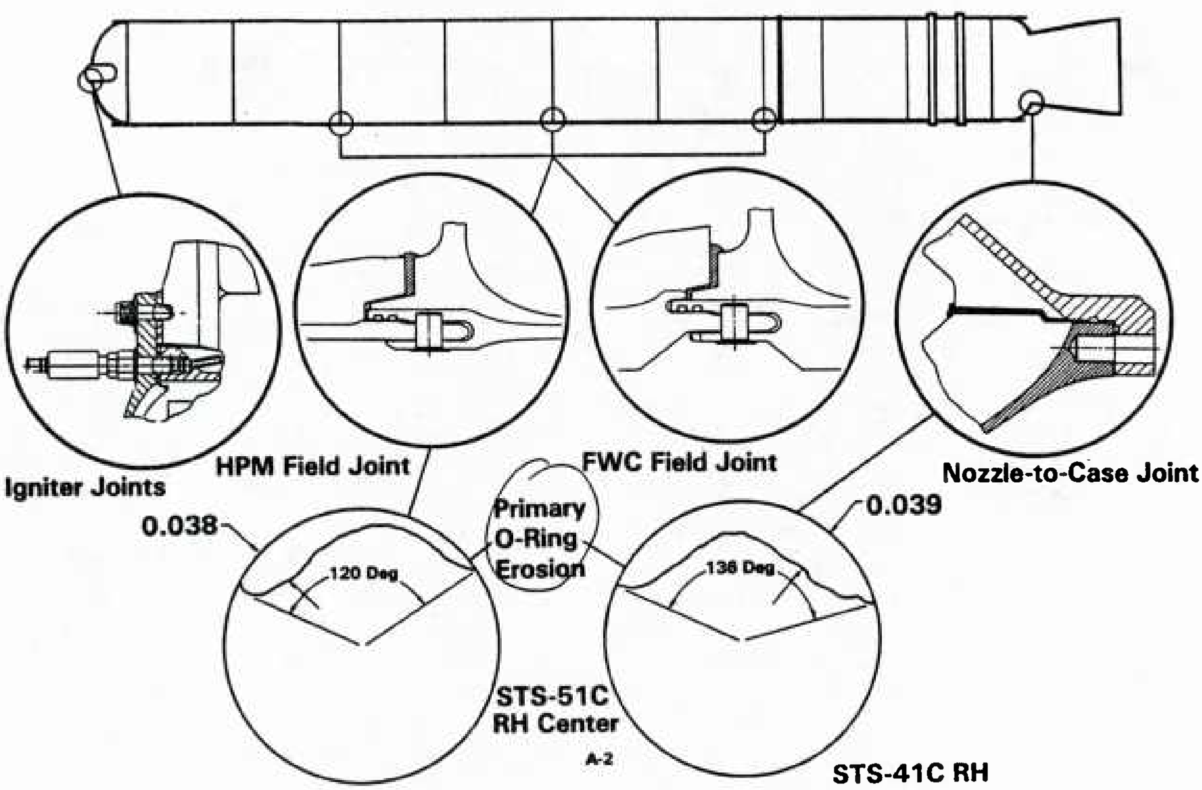
say—and I am giving the report—the lack of a good secondary seal in the field joint is most critical, and ways to reduce joint rotation should be incorporated as soon as possible. Analysis of the existing data indicates that it is safe to continue flying.

CHAIRMAN ROGERS: That is really what it is saying, but you are just making it more dramatic.

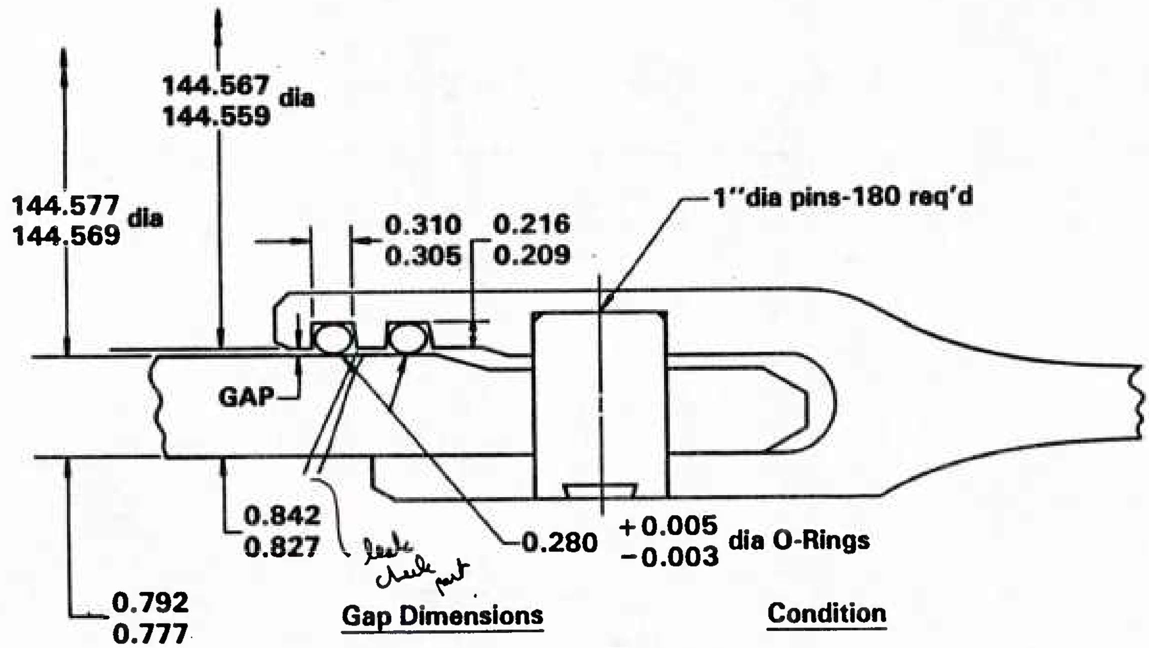
MR. HOTZ: You have got a real problem with that.

Space Shuttle SRM Joints



[Ref. 2/10-12]

Space Shuttle SRM Segment Joint

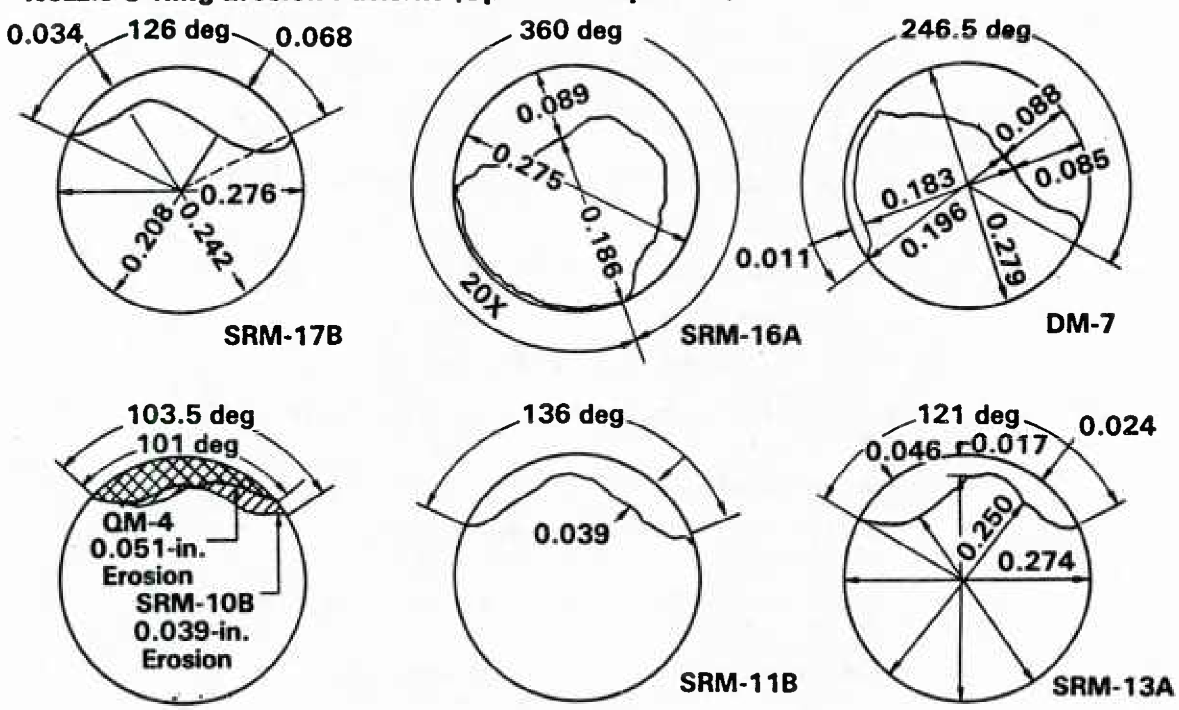


Gap Dimensions	Condition	
0.005 ± 0.004	Concentric	Diameter Basis
0.010 ± 008	Non-symmetric	
0.033 Max	Non-symmetric	Gathering

[Ref. 2/10-13]

Past History Comparison

Nozzle O-Ring Erosion Patterns (Optical Comparator)



[Ref. 2/10-14]

QM-5 Options

- Putty options
  - Randolph putty, asbestos-filled, presently flight qualified
  - Inmont-Canada putty, asbestos-filled, tested on DM-7 aft field joint and nozzle-to-case joint
  - Inmont-St. Louis putty, asbestos-free, tested on DM-7 center field joint
- QM-5 approach and rationale
  - Randolph putty in igniter joints
    - Presently installed
    - Previous tests and flights verify acceptability
    - Other putty may be used later based on similarity to use in nozzle joint
  - Inmont-Canada asbestos-filled putty in field and nozzle joints
    - Qualify alternate material as being equivalent or superior to Randolph putty while introducting least risk to firing
    - Field joint layup per DM-7 aft and center joint configuration based on known joint gaps
    - Nozzle joint layup same as current flight configuration

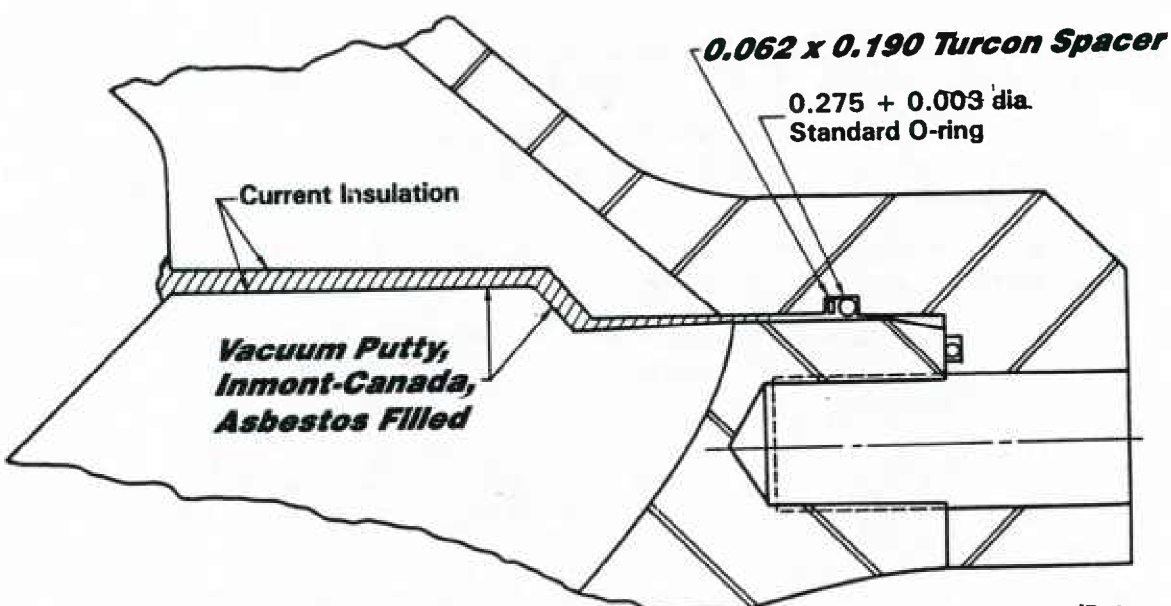
[Ref. 2/10-15]

Concepts Implemented in QM-5

- ***0.292 in. diameter O-ring in center and aft field joints (vs 0.280 standard)***
- ***Turcon spacer plus standard 0.275 in. diameter O-ring in nozzle joint (see chart D-2b)***
- ***Vertical assembly of QM-5 nozzle***
- ***Inmont-Canada putty in all field joints and in nozzle joint using currently demonstrated layup configuration***

[Ref. 2/10-16]

QM-5 Nozzle Joint



[Ref. 2/10-17]

General Conclusions

- All O-ring erosion has occurred where gas paths in the vacuum putty are formed
- Gas paths in the vacuum putty can occur during assembly, leak check, or during motor pressurization
- Improved filler materials or layup configurations which still allow a valid leak check of the primary O-rings may reduce frequency of O-ring erosion but will probably not eliminate it or reduce the severity of erosion
- Elimination of vacuum putty in a tighter joint area will eliminate O-ring erosion if circumferential flow is not present - if it is present, some baffle arrangement may be required
- Erosion in the nozzle joint is more severe due to eccentricity; however, the secondary seal in the nozzle will seal and will not erode through
- The primary O-ring in the field joint should not erode through but if it leaks due to erosion or lack of sealing the secondary seal may not seal the motor

[Ref. 2/10-18]

Recommendations

- The lack of a good secondary seal in the field joint is most critical and ways to reduce joint rotation should be incorporated as soon as possible to reduce criticality
- The flow conditions in the joint areas during ignition and motor operation need to be established through cold flow modeling to eliminate O-ring erosion
- QM-5 static test should be used to qualify a second source of the only flight certified joint filler material (asbestos-filled vacuum putty) to protect the flight program schedule
- VLS-1 should use the only flight certified joint filler material (Randolph asbestos-filled vacuum putty) in all joints
- Additional hot and cold subscale tests need to be conducted to improve analytical modeling of O-ring erosion problem and for establishing margins of safety for eroded O-rings
- Analysis of existing data indicates that it is safe to continue flying existing design as long as all joints are leak checked with a 200 psig stabilization pressure, are free of contamination in the seal areas and meet O-ring squeeze requirements
- Efforts needs to continue at an accelerated pace to eliminate SRM seal erosion

Ref. 2/10-19]

Capture Direction

- *Billet informal direction—July 1985*
- *Drawing release with informal direction—October 1985*
- *ECP delivered to MSFC—20 Jan 1986*
- *Rohr/Ladish acceleration informal discussions—4 Feb 1986*
- *Planned formal release to Rohr—14 Feb 1986*
- *Will deliver first parts—August 1986*

[Ref. 2/10-20]

Concepts Implemented in QM-5

- *0.292 in. diameter O-ring in center and aft field joints (vs 0.280 standard)*
- *Turcon spacer plus standard 0.275 in. diameter O-ring in nozzle joint (see chart D-2b)*
- *Vertical assembly of QM-5 nozzle*
- *Inmont-Canada putty in all field joints and in nozzle joint using currently demonstrated layup configuration*

[Ref. 2/10-21]

CHAIRMAN ROGERS: Well, thank you very much.

Let's talk a little bit about tomorrow. We will meet at 10:00 o'clock. We will say that we had material presented in some detail, and we want to go over pretty much the same information. You can summarize some of it because we heard it, and we want to deal with some of the questions that have been raised and that were publicly made, and we will try to relate our investigation tomorrow to those issues.

Now, we have to decide who we should ask to testify or to make statements, and I would think that Larry is fine, but now we have got to get down to answering the specific questions about what was done, and in 1985 you had these concerns, and it seems to me somebody should deal with it in chronological order and say yes, we had these problems; here's what we did: we

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had a meeting, we wrote a report, here's what we said. We still had concerns. We did the following things. Here's how we did it, and just have a chronology in narrative form of what happened, because otherwise it looks as if—and I heard some of the people the other day felt it was too long or windy. Well, I don't think we have to worry about it as long as we tell a narrative story in which the conclusions that you reach—and I mean, conclusions in the sense that you say well, after those studies were made, and so forth, that we made the decisions, and here is why we made them. Somebody has to say that.

Now, everybody recognizes that you are going to make mistakes in judgment, but at least we have to show that it wasn't done in a careless fashion and that there were meetings and you thought about it and who was there and things of that kind.

Now, whether you can do that tomorrow or not, I don't know.

MR. MOORE: I would like to volunteer to start it off tomorrow and try to put some of this in context and so forth to try to give the flavor that you were just citing, Mr. Chairman, about the context of the actions that were taken.

CHAIRMAN ROGERS: Would you give us a list of

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the people that you think you may want to call?

MR. MOORE: I think the only two people that we would like to have testify is probably myself and I think Larry Mulloy will talk tomorrow, and those are probably the two principal ones.

I think I would put a question mark on Mike, and I think Mike and I can sit down and talk tonight and decide that. I don't know if we need that precise answer immediately, but I think we will go back to NASA headquarters and begin working on that.

CHAIRMAN ROGERS: What documents can we provide and make available to the public?

MR. HOTZ: Is this whole thing going to be available?

MR. MOORE: No, sir. What my proposal would be, I made these documents specifically for the Commission. My proposal would be to go back and take a subset of this documentation, much smaller than that, and present it to you and the other members of the Commission as we will testify on it tomorrow, and we will go through on a slide by slide basis the charts as we did in our testimony to you on Thursday.

[Material deleted.]

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MR. HOTZ: Mr. Chairman, I wonder if the memos that are specifically referred to in the New York Times story will be presented as part of our public record.

MR. MOORE: Yes, sir. I would propose to do that in the morning, absolutely. I would propose to put those memos in the context is why I suggested, if it was acceptable to you, that I would plan to initiate this whole discussion to try to put those memos in context.

CHAIRMAN ROGERS: Are there any memos in here that haven't been made public?

MR. MOORE: No, sir.

CHAIRMAN ROGERS: I mean, if we say some documents were given to us but we can't make them public, that creates a whole new question.

MR. MOORE: No, sir. The thing we also can't control, Mr. Chairman, is there is none of this documentation that is, you know, of a classified nature and being held tightly. I mean, this is information in our files, and it is hard to tell where copies of the information are, and why we tried to present you and the

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Commission with as comprehensive a set of documentation as we can, is so we don't get surprised in the future by somebody showing up with another document.

DR. RIDE: Are there any documents in here that were not made public in the New York Times?

MR. MOORE: Yes, there was a document in there by Russ Bardos who wrote a document to Mr. Winterhalter suggesting that the SRM system should continue to fly and we should not make any quick fixes to the SRM in preparation for the QM-5 flight testing.

CHAIRMAN ROGERS: Why don't we make all of these documents public?

MR. HOTZ: That's a good idea.

CHAIRMAN ROGERS: I mean, if——

MR. HOTZ: It would be very salutary if we made a very complete record public, more than what the New York Times had. That would defuse a lot of this issue.

MR. MOORE: Could I suggest that the Commission consider releasing the document we provide you tomorrow which will have all of the memos and so forth? There will be the New York Times data plus additional data, and I would suggest to you, Mr. Chairman, that that be the document released to the public, although I personally don't have any problem if

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you want to release this.

(Material deleted.)

MR. HOTZ: No, just the relevant material.

MR. MOORE: Sir, we may appear a little disorganized here today, but we put this stuff together very quickly, and I would like to have a chance to go back together and organize it.

CHAIRMAN ROGERS: Well, why don't you do that, but why don't you proceed on the theory that all of this is going to be made public. If we don't do it tomorrow, it will be—I mean, it will be a mess for the press, and it is hard for us, and I am not being critical, but you do what you think you should do, and you say we are going to make all the documents available to the press, and if there are any classified documents, we won't, but as far as we can tell, these are not classified.

MR. MOORE: To my knowledge, in my office back there and my people that are working there that have

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identified that, we do not have any additional data that is relevant to this subject that is not compiled in this document here.

VICE CHAIRMAN ARMSTRONG: Mr. Chairman, I think it would be helpful if at some early time they describe how the seal is intended to work. I think we sort of had to dig that out of them here. If you could describe how it is supposed to extrude and so on, and what a normal course that that seal would seek, the normal environment that it would seek, it would be helpful for everybody to understand.

CHAIRMAN ROGERS: We can also say, of course, tomorrow that we are going to meet on Thursday and Friday when we have an opportunity to see them first hand, but I think you are right, I think tomorrow if you could assume that you are talking to people who are not experts in it, because you have got the public there, and explain it to them.

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(Deletion here.)

MR. MOORE: What about the Rockwell person also?

CHAIRMAN ROGERS: Well, I don't think, if nobody knows anything about it, I mean, you have already testified to that here today.

MR. MOORE: I testified—Arnie Aldrich and I did not receive to my knowledge any comments or any calls from Rockwell management persons.

CHAIRMAN ROGERS: And no one is here from Rockwell today?

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MR. MOORE: No, sir. No one is here from Rockwell.

CHAIRMAN ROGERS: Well, I think it might be a good idea for you to check and ask them so we are not surprised again.

MR. MOORE: I will do that. Someone is supposed to be doing that down in Florida, and I just haven't gotten a report back.

How long should we plan on going tomorrow, Mr. Chairman?

CHAIRMAN ROGERS: Well, I would guess it would go all day, maybe not. If we could get out earlier, I would rather do it, but I think you will find tremendous interest as we expected. I mean, we have been shown a list of newspapers and networks. I mean, we want to try to be—I tell you, I like the questions that were asked today, and I wouldn't hesitate to ask the same questions tomorrow because it shows that members of this Commission have a lot of background on this subject, and I don't think we should hesitate to ask these questions again, and I thought they were all good.

DR. COVERT: We might dig up some new ones overnight.

GENERAL KUTYNA: We spent all day on the seals. What if we were to ask you what other avenues

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you are pursuing?

MR. MOORE: We would be able to talk about that. As a matter of fact, I would be able to tell you in general terms what other avenues we are putting, not fault trees but problem trees, if you want to call it, together in all areas. None of the systems in the Shuttle system have been exonerated at this point in time. My task force is just getting started, and I see it running for many months, and no system has been checked off as being completely exonerated from any cause of the 51-L incident, and that is one of the opening statements that I was going to make in the morning about the activities that we are going through.

CHAIRMAN ROGERS: And another thing you could say is that when we are down there we are going to meet with the head of the team so that we get a better idea of how they are proceeding and how we can cooperate and coordinate our activities.

MR. MOORE: Maybe I should give a brief status of what the task force that I have been charged with is doing down there.

Sir, do you think on Thursday and Friday's meeting, will you want to have it closed, or do you want to have it open?

CHAIRMAN ROGERS: Well, I think it has got to be closed. We are not going to have a meeting. We are

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going to look at things, and we are going to have a lot of private discussion. So we are not having a meeting as such.

And let's decide whether we want a meeting on Friday or not. Let's just say we are going to be there and we have no meeting scheduled.

MR. MOORE: We want to make sure that you see some of the hardware and see the SRBs and the stacking and those kinds of things.

CHAIRMAN ROGERS: So if Thursday if we decide to have a short meeting, sort of summarizing what we have seen or telling people what we have done, we can do that, but let's not preempt ourselves.

DR. COVERT: Is there a travel schedule prepared?

CHAIRMAN ROGERS: Do we have any schedule?

MR. THOMPSON: What I would like to do is sit down this evening with Al and try to put together between——

CHAIRMAN ROGERS: Are we all going to be leaving from Washington?

MR. THOMPSON: I am anticipating that.

CHAIRMAN ROGERS: I said when we had lunch that I will not prevent any question from being asked. At least I won't answer any question that relates to

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anything more than 12 hours away.

(Laughter.)

DR. WALKER: When will we start on Thursday?

MR. THOMPSON: Well, we tentatively plan to start at 10:00 a.m. on Thursday.

MR. KEEL: The notion that John had was to have a plane leave early from National on Thursday morning so we wouldn't have to stay overnight.

We will stay overnight Thursday.

MR. THOMPSON: There are a couple of things I would like to talk to the Commission about.

CHAIRMAN ROGERS: Do you want to do it in executive session?

MR. THOMPSON: Yes.

MR. CULBERTSON: If I could say this, and I have not been involved in the investigation and all, and that is this, that there is a concern I always have. We have internal investigations about everything, but I can see it coming. NASA is by its nature an organization in .

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which we try to encourage everybody to express his point of view, and the meeting that we have on the kind of decisions that we have gone through here today, we go around, and if anybody has a dissenting point of view, we want it expressed.

When we get ready for a launch, as Jesse said, he polls 27 people or some such number, all the contractors and all the government people, are we ready, and in a lot of the kinds of things that we do there are no black and white answers on these issues. They are grey. And I don't know, if what I am going to say is going to influence what you are doing, but I would hope that the system that we use to draw out points of view is not impugned by the Commission judging the way we do things. It is an important process.

CHAIRMAN ROGERS: Well, if I understand your question, it breaks down into two parts. Anything that happened prior to the launch or immediately after is public property. It is all going to be in the public domain, no matter what you think.

MR. CULBERTSON: I'm not saying that. I think we ought to be forthright in giving all the data we have got, including every point of view, but don't expect not to find many points of view which would say don't do that or make a different decision because those were

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all expressed, and as you go through this, you will find many, many times eight people said yes and one person said no, and that person may have been right, and you are going to have to evaluate all of those things.

CHAIRMAN ROGERS: That is true throughout government generally. Everybody knows that. Anybody that has lived in government in Washington knows all of those things come out, whether it is the President of the United States or anyone else. I mean, he has all kinds of people saying what they told him and so forth.

I think the more difficult problem is it is your investigation. We don't want to intrude on your investigation, and you should go ahead and try not to. At the conclusion of your investigation you will give us information and you will come to some conclusions, and we can then take that and not—we are not going to tackle everybody who has been involved in the investigation, but let me tell you—and I told some of the members of the Commission, I have two letters from Congress, one from the Senate, one from the House, saying they are going to not do anything while we are going through this process, but as soon as it is over with they are going to analyze the report we make, and they are going to have an overview of us. So at that time we will be in the same position you fellows are in now. We will

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be spending three or four or five days up there and trying to say why we said what we said in the report.

And that is just the way it operates.

MR. CULBERTSON: Well, as I said, I don't know how what I said could influence what you are doing, but you must expect that you probably haven't seen the last of these memos where somebody says hey, don't do it. You will probably come across many.

We are searching our files to find those things.

CHAIRMAN ROGERS: Well, that's fine.

MR. McDONALD: Could I find out when you want the people from Thiokol?

CHAIRMAN ROGERS: It won't be tomorrow. Maybe you will have to say something tomorrow, but if you do, depending upon how it goes—but if you do, I will just say that we have asked you to have other witnesses with firsthand information a little later on.

DR. WALKER: Where are we meeting tomorrow?

MR. KEEL: In the State Department.

CHAIRMAN ROGERS: Okay, thank you very much.

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(Whereupon, at 6:05 o'clock p.m., the Commission recessed, to reconvene at 10:00 o'clock a.m., Tuesday, February 11, 1986, in open session.)

PRESIDENTIAL COMMISSION ON THE SPACE SHUTLE CHALLENGER ACCIDENT—TUESDAY, FEBRUARY 11, 1986

Washington, D.C.

The Commission met, pursuant to recess, at 10:10 a.m.

PRESENT:

WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ARTHUR WALKER
DAVID C. ACHESON
DR. RICHARD FEYNMAN
MAJOR GENERAL DONALD KUTYNA
ROBERT HOTZ
DR. EUGENE COVERT

ALSO PRESENT:

AL KEEL, Commission Executive Director

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PROCEEDINGS

CHAIRMAN ROGERS: I would like to call the Commission to order, please.

Yesterday we had a meeting in executive session, at which time the NASA officials and others produced documents at our request, and memoranda, dealing principally with the O-rings and seals on the booster rockets. They complied fully with the request that we made and were very forthcoming in discussing all aspects of it that we were able to discuss at the meeting.

This morning we will start the meeting with officials from NASA, particularly dealing with the matter of seals on the booster rockets. And I would like as much as possible to limit our discussions today to that one subject matter.

We will attempt to advise the press of our plans as they are formulated, so that you can plan your own schedules. We are contemplating at the moment tomorrow having a day off, hopefully to get a little better organized and getting our staff organized. On Thursday, we plan to go to Kennedy and probably stay over Friday in order to see the facilities, discuss with the NASA officials all aspects of the launch, and to be available to have presented to us any other matter that

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NASA feels would be appropriate.

During that time we will have informal discussions with people. We expect those will not be open sessions, because physically it is impossible. We may divide into subcommittees or we may

have individual interviews, but we will keep you advised about that. We would hope at that time, keeping in mind that we are going to focus today on the seals of the booster rockets, we would hope at that time that NASA will be in a position to show us and to show the media more of the pictures of the flight itself.

And also, we hope that we will be able to get some more information about telemetry and measurements that are being studied now. We want to do this in a way that will not be intrusive as far as the investigations under way are concerned, but we want to get as much information as quickly as we possibly can.

Later on—and I'm not sure when, probably next week—we will have meetings, either in executive session or closed meetings, dealing with the problems involving the weather and all of the weather-related problems, and we will take testimony from the Thiokol people and from NASA officials and get information about meetings that were held prior to the launch, all aspects of the weather and how the weather might have related to

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the launch.

So today we will start with NASA officials, and first I would like to ask Dr. Graham, the Acting Administrator of NASA, to take the stand and address the Commission.

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STATEMENT OF DR. GRAHAM, ACTING ADMINISTRATOR OF NASA

DR. GRAHAM: Good morning, Mr. Chairman. Do you wish to swear me in or shall I proceed?

CHAIRMAN ROGERS: No, you have been sworn in. You may proceed.

DR. GRAHAM: Thank you.

Mr. Chairman, members of the Presidential Commission on the Challenger Accident: I would like to assure you that NASA is continuing to review the facts and circumstances surrounding the accident that occurred with the Challenger.

As NASA continues to analyze the system design and data, as I said at the meeting last Thursday, you can be certain that NASA will provide you with its complete cooperation. In keeping with that, we have implemented several procedures.

One of them is that all NASA testimony that is given to the Commission will be reviewed on a word by word basis by a knowledgeable NASA technical review team. Should any error, partial, or incomplete statement, or potentially misleading statement be found in the testimony, an amendment to the testimony will be filed in order to clarify the issue of concern. That will certainly be called to your attention.

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In addition to that, the NASA policy concerning the release of material is that NASA is making available to the Commission and then to the press and the public all information related to the tests that we have in NASA reviewed and which we find to be reasonably factual and not grossly misleading. We will continue that policy and, while I am sure that there will be concerns on NASA's part and as well as elsewhere over the time it takes to review some of this mass of information, to pull it together, and to provide it in a form that can be distributed to the press, nevertheless we are making very, very substantial efforts to have that information available to you and then subsequently to the public as quickly as we possibly can.

Do you have any questions, Mr. Chairman, concerning that, before I go on to introduce the next speaker?

CHAIRMAN ROGERS: Yes. I would like to have a discussion with you about that first. Are there many confidential or classified documents among the documents in your possession?

DR. GRAHAM: There are very few that we have found to date, Mr. Chairman, that are of a national security nature that are classified.

CHAIRMAN ROGERS: So you will be able to make

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all the documents available, in the final analysis, to the public?

DR. GRAHAM: Within the constraints of the law, either constraints concerning national security issues or constraints concerning technical export or transfer, we may have to exercise some discretion on a very, very small set of the documents. But in general, and certainly the vast bulk of the information, the relevant material and the data will be released to the public. And of course, all data is accessible to the Commission.

CHAIRMAN ROGERS: Now, there is no feeling on the part of NASA that the work of the Commission is in any way interfering with the disclosure of information, I hope?

DR. GRAHAM: No, sir. In fact, the work of the Commission is very much in accord with the work that NASA is undertaking and conducting internally, and we find these to be in general complementary.

CHAIRMAN ROGERS: In fact, you asked me to have this public session today in order to make it clear that NASA was not trying to brush anything under the rug, isn't that right?

DR. GRAHAM: Yes, sir. I suggested to you that you consider having a public session today on

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specific characteristics of the SRB's, the solid rocket boosters, and any other matters you saw fit to question the NASA officials concerning.

CHAIRMAN ROGERS: I assume that there are thousands and thousands of documents that you are now considering for purposes of the investigation and for purposes of this Commission, is that right?

DR. GRAHAM: Yes, sir, a large number of internal documents that we have in review and consideration. We plan to release to the press today at the conclusion of this discussion the material that will be presented to you or is being presented to you today. And then tomorrow NASA will have a press briefing, and at that time we plan to release the entire bulk of the material that was released and presented to the Commission yesterday.

The amount of that material alone is a stack probably close to three inches high, and that is just a small part of the total data that we are reviewing and preparing for transmission to you and to release.

CHAIRMAN ROGERS: In light of the memorandum which has appeared in the press, written by Mr. Cook to Mr. Davis—and incidentally, those gentlemen are here today and will appear and testify—I assume that there are a lot of other documents of that nature, which make

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suggestions about how matters should proceed, pointing out risks that were involved in launches, et cetera, is that correct?

DR. GRAHAM: Yes, sir. In any highly sophisticated technical operation such as the operation of the space shuttle system, there has to be a continuing dialogue within the agency that is responsible for operating it concerning the performance of the system, the characteristics, how

well the design is behaving in comparison with the operational data and the design expectation of the system.

All that is being constantly cross-checked and reviewed and re-analyzed, and you will find that there is a substantial volume of information that documents that process inside NASA, and we will make that available to you as soon as we have a chance to accumulate it and put it together in a form that is comparable.

CHAIRMAN ROGERS: The point I'm making is it's not unusual in an agency like yours to have employees make critical comments, suggest dangers that might be involved in the program. That is the way the system works, isn't it?

DR. GRAHAM: Yes, sir. It is very, very important, in fact, for the system to work to be somewhat

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self-critical, and in the process of operating these systems to constantly review the issues, the engineering decisions, the performance. That internal self-criticism is in fact one of the strongest characteristics of NASA and one of the things that makes it in my view such a high quality technical operation.

CHAIRMAN ROGERS: And it may very well be that there will be a lot of other memoranda that appear in the press that neither you nor we know about? That would not be unusual at all, would it?

DR. GRAHAM: I wouldn't be in the least bit surprised if other issues come forward as we proceed through this review process.

CHAIRMAN ROGERS: And if we focus today on, to some extent on seals and O-rings and memoranda that are written by Mr. Cook and others dealing with that subject, the fact that we focus on it doesn't mean that that is the only area of concern as far as you're concerned or as far as the Commission is concerned, is that right?

DR. GRAHAM: No, sir, it is not the only area where we will find memoranda expressing engineering issues and engineering concerns. And it certainly doesn't mean that the NASA internal analyses has singled out any one area at this point—the O-rings, the seals, the field joints, or any other specific area—

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as a unique source of concern and analysis.

We are still looking across a broad range of issues to try to establish what actually occurred in the Challenger accident.

CHAIRMAN ROGERS: Very well. Then the Commission will try in an orderly way to consider each one of those aspects, so at the end of our deliberations we will have a complete record of all of the documents in your possession and a complete record of the pictures and telemetry and all other aspects of it, that will help us make a judgment.

And I thank you very much for this colloquy. Maybe other members of the Commission might want to ask some questions.

DR. FEYNMAN: I wanted to say that there's as aspect of trying to figure out exactly what happened, in that first something looks obvious. Then, it is the experience of Commissions who have looked into accidents that what looks obvious at first turns out later to have a little flaw, and, when you make a long list of things that are out of the ordinary, that are called anomalies, you discover that there is something that doesn't quite fit, and then the theory has to be completely changed.

So that kind of work we don't want to have to drag the public through. We're thinking of

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possibility A, possibility B, possibility C, and as we go through all of these things all the newspapers are always saying, now they think it is this, now they think it is that.

We don't know what it is, and we would like to investigate that physical question, I should hope, if it is at all possible, without the public directly, and then we can give a complete report. But when we are discussing this particular matter, it doesn't imply that this is what was the cause of the accident, but as an example the kind of thing that we have to investigate.

I don't think you should conclude that we know that this is directly related or whether it is or it isn't directly related. Certainly it is information we have to have to the actual accident.

DR. GRAHAM: Yes, sir.

CHAIRMAN ROGERS: I think there is one other aspect that deserves some comment at this point. Usually in investigations of this kind, you find that the press is not very knowledgeable on the subject and therefore the reporting is not very accurate. It seems to me that in this case the reporting has been quite fair and accurate on the part of the press, partly because they know a lot about it and they have followed it very closely.

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And so I hope that we don't develop any friction between the media and NASA and this Commission, because we are all working to the same end. And as far as I can tell up to date, it has been a very fair process on the part of the media, and I hope we can cooperate with them in all ways to deal with this very difficult and tragic accident, which is of such importance to the nation.

All right, Doctor, you may proceed.

DR. GRAHAM: Thank you. Mr. Chairman. And that is certainly our intention, to proceed in just that manner.

I would like to now introduce the Associate Administrator for Space Flight, Mr. Jesse Moore. Mr. Moore plans, directs, and executes the development, acquisition, testing, and operation of all elements of the space transportation system within NASA and, as I mentioned last week, he has also been named as the director, the new director of the Johnson Space Flight Center at Houston.

Mr. Moore.

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STATEMENT OF JESSE W. MOORE, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT, NASA

MR. MOORE: Mr. Chairman and members of the Presidential Commission:

What I would propose to do today is to give you a short status report of what my task force is doing and the areas we are focusing in on, and then I would like to call on the project manager of the solid rocket booster from the Marshall Space Flight Center to discuss with you and members of the Commission the solid rocket booster aspects and to try to address some of the issues that have been raised before the public here and to let you, as well as the public, know what actions have been taken with the solid rocket booster and what its functions are and so forth.

So that is kind of the agenda that I plan to cover this morning.

(Viewgraph.) [Ref. 2/11-1]

I would like to say at the outset that I think the discussion that you and Dr. Graham had lays a very good foundation for the type of work we're conducting in our task force. We plan to cooperate very fully with the Commission and provide the data to the Commission as required.

As you indicated and Dr. Graham indicated,

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there is an enormous amount of data that is available on all aspects of the space shuttle, and we will pull and are pulling all relevant documents together related to the 51-L tragic accident.

On February 5th, Dr. Graham formed a data analysis design task force on the 51-L mission incident. I am chairing that task force.

(Viewgraph.) [Ref. 2/11-2]

This was a transition from an interim mishap board that I had been chairing previously, and we're in the process now of formally establishing our charter and our membership. We had our first organization meeting at the Kennedy Space Center yesterday. I was obviously unable to attend, but the group is meeting, and we are preparing a list of and setting up a group of panels to address specific areas associated with the space shuttle 51-L mission incident.

We are planning to include on our panels, as well as the overall task force, members not only from NASA, but members from the outside to address specific areas of expertise as far as this overall incident is concerned.

Where we are today is we are continuing our salvage operations to try to find as much physical evidence as we possibly can that would allow us to piece

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together the set of circumstances that caused the 51-L tragedy.

(Viewgraph.) [Ref. 2/11-3]

In the area of data analysis, which is one of the most complex areas to try to look at, our primary concentration is to try to reconstruct a mission events time line, and this time line will tell us in great detail the sequence of events that went on from launch until the incident happened, some 70-plus seconds later.

We are looking at photographic data, and you mentioned that earlier in your opening comments. We will share some of that data with you in Florida on Thursday, and we are also trying to understand what load effects might have been applied to Challenger's launch, meaning there are different forces that we don't understand that happened during the trajectory: Were there any unusual set of circumstances happening prior to launch that we need to know about?

And our efforts are aimed at trying to get what I'd call an integrated load picture of what the flight looked like during pre-launch as well as during its ascent.

And you mentioned weather. Weather is certainly an issue that we're going to be working very

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hard and are working very hard to try to understand what effects, if any, the weather played. We will be looking at temperature effects. We will be looking at wind conditions, not only surface wind conditions but upper atmospheric wind conditions, as well as moisture conditions, rain content, and so forth.

All these will be looked at in very great detail, and we will be happy to discuss weather activities with you and the Commission at your discretion.

CHAIRMAN ROGERS: On that subject, in preparation for hearings it would be useful if we had a scenario worked out of conferences and meetings and discussions, so that we can have a narrative form of what happened, in addition to all of the weather data itself.

MR. MOORE: I presented to you and the Commission, I guess last week when we had the public hearing, that there were a number of mission meetings held in the chronology of this launch. We will go back, and are doing it right now, expanding that chronology, so we will be able to talk to you and the Commission in great detail on the weather aspects associated with this launch.

CHAIRMAN ROGERS: Very good.

MR. MOORE: In addition to things like

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weather, loads, and other kinds of effects, it is critical to understand the pedigree of the hardware, how the particular hardware was processed, who handled the hardware, what kind of safety inspections were performed, and how many times the hardware had been used, and so forth. So these areas are also being looked at in very, very great detail.

There is a very complex procedure set up to apply and reuse hardware from the various shuttle flights and, as you know, there are two major elements that are reuseable from the shuttle flights. The shuttle orbiter certainly is reuseable, as well as the two solid rocket boosters are reuseable. They are launched and deployed, and we bring those boosters back in, refurbish those boosters to a certain set of specifications, and then refly those boosters on subsequent missions.

You asked a question of us the other day that I would like to answer. A new set of solid rocket boosters, flight set, meaning two of them, costs about 65, $66 million, is what a brand new set costs. A refurbished set costs on the order of 22 to 23, $24 million.

So there is about a factor of three in terms of cost relative to buying new flight sets of SRB's for

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each flight versus the reusing of these flight sets. And so I thought I would present that piece of data to you, to answer one of your questions that you talked about.

Another element that is very important in this particular launch is the launch pad. This was the first time we had launched a mission off of pad B. Our previous 24 launches had been on launch pad A at the Cape, and we are clearly spending a lot of time looking at any differences there may be relative to the two launch pads that were used.

We were carrying some cargo on board this flight. We were carrying a Tracking and Data Relay Satellite system, as well as a Spartan-Halley. We had flown the Tracking and Data Relay Satellite system and the inertial upper stage before, but we are trying to find out, were there any unusual circumstances associated with that.

No STS element—and "STS" is "Space Transportation System"—will be left unturned. No stone will be left unturned. We are not exonerating any aspect of this particular mission as far as free from either being a cause or an effect from the tragic incident that happened on 51-L.

We are putting together, as has been

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previously discussed, several of what I would call failure scenarios. These are things that could go wrong, and we are putting those together across all parts of the program. And our job in this task force is to try to go and prove each of these scenarios did not contribute to this accident, and we are doing that by analysis and by tests that are being conducted now.

(Viewgraph.) [Ref. 2/11-4]

And also by test data that has been previously run in the program. And so that will be a process we will be going through to discount and to say conclusively that this particular failure scenario was not a contributor in the 51-L mission incident.

CHAIRMAN ROGERS: Jesse, based upon what you told us before, though, you have been doing that each time, haven't you? I mean, this analysis is not new to 51-L?

MR. MOORE: We have failure modes and effects analysis for all elements of the shuttle, and that has been done and documented, and we're using those failure modes and effects analyses that are in the program as starting points for the kind of analysis that we're doing right now, Mr. Chairman.

CHAIRMAN ROGERS: Good, because we wouldn't want to leave the impression that you're doing it just

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because of this accident. Your records indicate you have been doing this on a regular basis.

MR. MOORE: Yes, sir. It has been done since the program, since the start of the program, as part of the normal analysis of a system like this where you do go through and do detailed failure modes and effects analysis and so forth.

And what we're trying to do is make sure that what has been documented and known and done in the program is consistent with the postulates that we're putting forward now, that may have been a cause relative to the 51-L mission.

We are also trying to be very careful and discriminating between cause and effect, and it is easy to say, here's a picture that shows a piece of information, but that piece of information may have been the result of some other cause. And so we are trying to be very careful and discriminating between cause and effect as far as where we are focusing in on the problems with respect to this accident.

The solid rocket booster is obviously one area that we are focusing very heavily on, and I will say a little bit more about that, and that is the purpose of our agenda here today, is to try to tell you and the Commission what we're doing in

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the solid rocket booster area and some of the potential areas that are of concern relative to the solid rocket booster.

The external tank is also involved in one or more scenarios as far as potential failure modes in this whole program, as well as the other elements, as I said earlier. And I would like to emphasize to the Commission that we have not exonerated any aspect of the 51-L mission as of now.

Finally, with respect to the solid rocket booster, we are looking at things like design specifications, materials that are used, the manufacturing process that was used, how the system was stacked and how it was prepared for launch, who was involved, the safety aspects of it, the quality assurance aspects of it, any photography that we have which closes out the work prior to a launch, and we do that on a routine basis, take photographs—

(Viewgraph.) [Ref. 2/11-5]

—of the flight hardware at various stages during its preparation for launch. And we call those closeout photography. And that data has also been impounded and is being used in our process of trying to understand what happened.

We're also going to discuss seals. That has

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been a very visible topic lately, and Mr. Mulloy of the Marshall Space Flight Center will go into a fair amount of detail on seals.

And we're looking at environmental effects. So there is a range of things that we're focusing on, Mr. Chairman, in our task force, that we will be working and interacting with you and your

Commission to make sure that all the data that is being generated by our task force is available to you and you understand that particular data.

And I think a point that was made earlier that I would like to just close on before I introduce Mr. Mulloy is that, you know, a lot of memos have been written about different concerns and issues in the program, and those memos, there are hundreds and thousands of those kinds of memos throughout the whole program.

Those concerns are looked at by the engineering and the technical analysts in the overall program. They are thoroughly reviewed as a part of our flight preparation process, which starts out at the contractor and then goes to the project office at a particular center that is responsible for this, and then goes up to the center management and then goes to what we call level two management at the Johnson Space

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Center, which we described the other day, and then comes to my level.

So there has been a thorough look at all of those elements that are critical to the launch of a space shuttle. And I would just like to say that on the solid rocket motor we believe that has been done as well, and you will hear that from Mr. Mulloy.

And we're also going back and looking at the engines, the tank, and so forth with that same degree of thoroughness to assure that all of that has been done for those elements as well.

With that, Mr. Chairman, I would like to turn this over, if there are no questions.

CHAIRMAN ROGERS: Let's see. There may be some questions.

(No response.)

MR. MOORE: I would like to introduce Mr. Larry Mulloy, who is the project manager of the solid rocket booster at the Marshall Space Flight Center.

(Witness sworn.)

AGENDA

• INTRODUCTION	WILLIAM R. GRAHAM, ACTING ADMINISTRATOR
• STS 51-L TASK FORCE STATUS	JESSE W. MOORE, ASSOCIATE ADMINISTRATOR OFFICE OF SPACE FLIGHT
• SOLID ROCKET BOOSTER	LAWRENCE B. MULLOY, SRB PROJECT MANAGER MARSHALL SPACE FLIGHT CENTER
• DESIGN DESCRIPTION	
• SRM SEALS	
• CONSIDERATIONS FOR STS '51-L MISSION	
• SUMMARY	JESSE W. MOORE, ASSOCIATE ADMINISTRATOR OFFICE OF SPACE FLIGHT

[Ref. 2/11-1]

STS 51-L TASK FORCE

- TASK FORCE FORMED ON FEBRUARY 5, 1986, BY NASA ADMINISTRATOR
- CURRENTLY ESTABLISHING A FORMAL ORGANIZATION, SPECIFIC CHARTER, AND MEMBERSHIP
- ORGANIZATION WILL INCLUDE PANELS TO ADDRESS ALL AREAS OF STS 51-L ACCIDENT
- MEMBERSHIP WILL INCLUDE PERSONNEL INTERNAL AND EXTERNAL TO NASA
- FIRST MEETING HELD AT KSC ON FEBRUARY 10, 1986

[Ref. [Ref. 2/11-2]

STS 51-L TASK FORCE

STATUS OF INVESTIGATION

- SALVAGE OPERATIONS
  - PHYSICAL EVIDENCE
- DATA ANALYSIS
  - MISSION EVENTS TIMELINE
  - INTEGRATED LOADS ANALYSIS
  - PHOTOGRAPHIC ANALYSIS
  - WEATHER EFFECTS (TEMPERATURE, WINDS, ETC.)
- MANUFACTURING AND ASSEMBLY PROCESSING OF HARDWARE
- LAUNCH PAD
- CARGO
- NO STS ELEMENT EXCLUDED AS A CANDIDATE PRIME CAUSE

[Ref. 2/11-3]

STS 51-L TASK FORCE

STATUS OF INVESTIGATION (CONT'D.)

- SEVERAL FAILURE SCENARIOS REASONABLY CONSISTENT WITH MISSION EVENTS TIMELINE AND OTHER DATA HAVE BEEN ESTABLISHED
- COMPARING EACH SCENARIO AGAINST THE FACTS
- DISCRIMINATING BETWEEN CAUSE AND EFFECT
- SOLID ROCKET BOOSTER IS OBVIOUSLY INVOLVED IN ONE OR MORE SCENARIOS
- EXTERNAL TANK IS INVOLVED IN ONE OR MORE SCENARIOS
- ORBITER, SHUTTLE MAIN ENGINES, AND OTHER ELEMENTS ARE BEING ANALYZED FOR THEIR CONTRIBUTION

[Ref. 2/11-4]

<u>STS 51-L TASK FORCE</u>

STATUS OF INVESTIGATION (CONT'D.)

- SOLID ROCKET BOOSTER
  - DESIGN SPECIFICATIONS
  - MATERIALS
  - MANUFACTURING
  - PROCESSING
  - SAFETY
  - PERFORMANCE
  - QUALITY ASSURANCE
  - CLOSE OUT PHOTOGRAPHY
  - EXPERIENCE
  - SEALS (O-RINGS)
  - ENVIRONMENTAL EFFECTS

[Ref. 2/11-5]

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TESTIMONY OF LAWRENCE B. MULLOY, PROJECT MANAGER, SOLID ROCKET BOOSTERS, MARSHALL SPACE FLIGHT CENTER, NASA

MR. MULLOY: Mr. Chairman, members of the Commission:

As Mr. Moore has stated, I intend to give you a briefing on some aspects of the solid rocket booster assembly, the details of that solid rocket booster, and then concentrate with a bit of information on how the solid rocket motors are assembled, how they were refurbished, and particularly on the seals and the joint.

(Viewgraph.) [Ref. 2/11-6]

The solid rocket booster is made up of a number of assemblies. The forward assemblies here are manufactured and refurbished by the United Space Boosters booster production company, and the forward skirt and forward frustum and the nose cone. That assembly contains the electronic devices and also the recovery system for the solid rocket boosters. It has three main parachutes, a drogue parachute, a pilot parachute, in that assembly, which does return the booster to the ocean, where it is retrieved by retrieval ships, brought back to the Kennedy port for disassembly, and returned to the manufacturer for refurbishment.

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The aft assembly, which is known as the aft skirt, which is the hold-down point for the total shuttle system, is also manufactured and refurbished by the United Space Boosters booster pro-

duction company, and then the center section here is the solid rocket motor, which is the primary area of interest that we have here today.

The solid rocket motor consists of four casting segments. Each of those casting segments is about 24 feet long. The booster is 146 inches in diameter. The casting segment itself is made up of two tank segments and joined by a factory joint.

When the motors are cast at Thiokol, they are then shipped by rail car to the Kennedy Space Center, where they are assembled into the shuttle stack. This center factory joint here is covered with the insulation that is inside the motor. There is an insulation, then a liner, and then the propellant.

The end joints or the field joints are metal joints with a tang and clevis that I will describe in more detail, and sealed with two Viton O-rings. When the boosters are recovered and returned to the Kennedy Space Center and disassembled, there is a very thorough inspection done of those assemblies immediately after retrieval.

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Steps are taken to preserve the hardware, as you will see in this model or this section, out of flight hardware that I will show of an actual section of the joint. The D-6 steel material is very susceptible to corrosion, so immediate steps are taken to wash down that metal and apply grease to protect the joints particularly from any corrosion.

CHAIRMAN ROGERS: Mr. Mulloy, could you as much as possible relate what you are saying to 51-L?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: How many retrievals were made?

MR. MULLOY: The aft segments on 51-L, the right hand booster, the aft segment had been used twice, once in a test motor and once in a flight motor. The aft center segment had been used once. We have used segments up to four times.

CHAIRMAN ROGERS: And is there data on how each segment was handled, both in retrieval and in Utah?

MR. MULLOY: Yes, sir. There is a complete record of the inspection results immediately after retrieval. Then when the booster segments are returned by rail car to the Wasatch Division of Morton Thiokol, there is—the process that goes through is a washout, initial washout

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of the remaining insulation material, then a grit blast, and the amount of material removed with that grit blast to take the paint and remaining insulation off is measured and recorded.

There is a detailed inspection then made for cracks, surface blemishes, and dimensional tolerances, and a structural analysis made to assure that that is acceptable. Then that segment is put back into what we call a hydroproof test, where we apply 112 percent of the maximum operating pressure that that segment has to sustain in the next flight.

Subsequent to that hydroproof test then there is what is called a magnetic particle inspection made of the case segment, to assure that there aren't any flaws that are not visible to the naked eye. And then that segment is put back through processing.

One of the critical things in the acceptance of that segment is a dimensional check to assure that particularly the mating joint, the tang on one end and the clevis on the other, is within the dimensional tolerances for new hardware. Reused hardware has no different tolerances than new hardware.

We do have a procedure whereas a hardware that doesn't precisely meet the drawing specification can be dispositioned by material review boards or a waiver can

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be issued. One of the things that is being investigated, of course, for 51-L is if any of those things applied to any of the segments on the 51-L vehicle.

To my knowledge, all of the hardware in the solid rocket motor on 51-L met all of the requirements for new hardware.

DR. RIDE: Have you ever recovered any of the solid segments that didn't meet the criteria for reuse?

MR. MULLOY: I believe in STS-1, either 1 or 2—and perhaps Bill Lucas may remember—there was one segment, due to the splashdown loads and cavity collapse loads—I would point out that there are stiffeners. These rings that you see back here on the aft segment are stiffener stubs; I believe my recollection is that we added an additional stiffener into the segment to preclude the loss of the aft segment.

And I do believe that in the early flights one or two segments had gotten outside of dimensional tolerances and could not be reused.

Since STS-5, which is when I took over the program, we have had no loss of segments due to flight loads or splashdown loads.

DR. RIDE: Do you X-ray any of those segments?

MR. MULLOY: Yes. The segments are X-rayed in

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Utah. In the initial stages of the program, there was 100 percent X-ray, 100 percent to the extent that you can get to 100 percent X-ray of a large structure like this. The maximum possible X-ray was done on all of the development qualification motors, and through a period up through the first six flight sets, to assure that our process that we had in place was producing a repeatable product.

We never found any problems as a result of X-ray, and what we are doing right now is a periodic X-ray to assure that the process controls that we have in place for the propellant, liner, and insulation, that we're not getting outside our experience on that.

And so essentially, what we're doing right now is X-raying about one segment a month on a sampling basis.

VICE CHAIRMAN ARMSTRONG: Anything other than X-ray? Ultrasound or NMR or other approaches used?

MR. MULLOY: Yes, ultrasound is used, and visual inspections, particularly visual inspections of the end of the segments, where you can determine whether there is any de-bond of the insulation to the liner or the insulation to the case wall.

DR. RIDE: What percentage of the segments is

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one segment per month?

MR. MULLOY: Well, of course, at the rate we've been going now, at a rate of 12 per year, we are casting eight segments per month, and so it is one of eight, essentially.

MR. WALKER: Have you X-rayed any recovered segments?

MR. MULLOY: No, there has been no X-ray of the recovered segments. There is nothing there but the steel. We do not do an X-ray of the steel. We do a surface dye penetrant inspection of that steel segment and a proof test.

CHAIRMAN ROGERS: So you will have a history, a full history of these two booster rockets that were on 51-L?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: Has there been anything that's come to your attention that was unusual about the two boosters on 51-L?

MR. MULLOY: No, sir. In going through—and I will cover the readiness review process that we went through—there is nothing that is in any of the records that I have reviewed that is unique about the boosters on 51-L.

DR. RIDE: What kind of effect would you

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expect from the corrosion on particularly the O-rings and the putty? I guess what I'm interested in is, if you recover the solids from 51-L do you expect to learn anything about the O-rings and the putty and the joint, or do you expect corrosion?

MR. MULLOY: What we are particularly interested in—and perhaps I should get into the details on the joint here. From a corrosion aspect, the primary concern for corrosion—and let me turn this in the flight direction here. The primary concern for corrosion is in the sealing surfaces of where the O-rings, which are these two black 280 thousandths diameter devices you see in these grooves here.

We are particularly concerned for pitting that may be inside of that sealing surface, because a pit obviously can provide a path for hot gas to get by the O-ring.

CHAIRMAN ROGERS: Could you describe what you're holding?

MR. MULLOY: Yes, sir. What we have here is a section from an actual solid rocket motor. The bottom section is the upper portion of one of the casting segments. The black on the inside is the propellant insulation.

The piece on the top is what is called the

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tang end of a motor segment. This is the field joint. It also is the factory joint, case to case. As I mentioned, two of these 12-foot segments go together to make one casting segment, and that is what goes to Kennedy for assembly.

On the factory joint, there is no discontinuity in this insulation, because the insulation is applied after the joint is made. So you have insulation over this joint.

On the field joint, however, there is this discontinuity in the insulation, since you have to put it together at KSC, at the Kennedy Space Center. And this gap between the insulation is filled with a zinc chromate asbestos-filled putty. That putty is laid up in strips prior to assembly.

We use strips of putty that are eighth inch and quarter inch thick and an inch to an inch and a half wide, to lay that putty up in a precise drawing pattern such that we are sure that the putty, when laid into this joint, does not extrude outward, but you have a good fill of the putty between the insulation surfaces, but that it does not extrude down into the O-ring gap such that it would tend to unseat the O-ring.

These O-rings in here are Viton rubber, provided by Parker Seals, and they are, as I say, about

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280 thousandths in diameter, and there are two of these at each one of the joints on the vehicle. The assembly is done in this position, with the tang end up and—excuse me, with the clevis end up and the tang end down.

Looking on this side, you see a pin that is a one-inch pin that is a high strength steel, that there are 177 of those pins in a joint. That provides the structural integrity of the joint.

You also see what looks like a little clip here on the outboard leg of the clevis. That is a 32 to 36 thousandths of an inch shim. The purpose of that shim is to assure that we have a con-

trolled dimension on the outer leg of the tang to—the outer leg of the clevis to the tang, to maximize the O-ring compression or the squeeze on these O-rings between this inboard tang, the inboard leg of the clevis and the tang.

MR. HOTZ: When do you get that squeeze?

MR. MULLOY: On assembly.

MR. HOTZ: You don't get it during launch?

MR. MULLOY: Sir?

MR. HOTZ: Do you get it during launch?

MR. MULLOY: Yes, sir. The design is to assure that you maintain that compression during launch, such that, the way these seals operate is it is a

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pressure-actuated seal, that you want compression on the seal such that when the motor pressure is applied to the seal that the seal will extrude into the gap downstream of the pressure and provide the pressure seal.

MR. HOTZ: Is there any particular phase of launch when that pressure is the strongest?

MR. MULLOY: Yes, sir. We go from zero up to the maximum pressure in about 900 milliseconds, and so it is instantaneous. And then we hold that max pressure for 20 seconds. And I will show you a pressure profile later in the briefing.

And then that pressure drops down to about 600 psi, and then it ramps back up slightly, and then you go into the thrust tailoff at approximately two minutes into the flight. And I do have a profile of that in the briefing.

DR. RIDE: Could you describe the corrosion on the joint?

MR. MULLOY: Yes. You can see the corrosion. What it amounts to is pitting in the metal, and so you see the corrosion that is on the outside of this piece here, is what we don't want to have inside the O-ring groove. That is why we take extra precaution to assure that we immediately preserve that hardware, because when we get it back it has been in the sea water

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for perhaps 30 hours longer with rough sea.

DR. RIDE: What about corrosion of the putty and O-ring? Is that a problem?

MR. MULLOY: Corrosion? Extrusion of the putty?

DR. RIDE: No, I think what I really want to know is how does the sea water affect the O-ring and the putty. "Corrosion" is the wrong word, but do you expect to find the O-ring intact when it has been in the sea water for a long time?

MR. MULLOY: Oh, yes. And we also find the putty intact. And as we have shown you in the data that we have presented to the Commission, where we have all of the data about our experience with this joint post-flight, you can clearly see the putty is still there in the joint. You can clearly see hot gas paths through the putty, and you can see very clearly any erosion that has occurred to the primary O-ring, and that is definitely attributed to the flight motor operation and not any effects of sea water.

DR. FEYNMAN: Can I ask a few questions in succession to help explain how this thing works?

MR. MULLOY: Yes, sir.

DR. FEYNMAN: This rubber thing that is put in, the so-called O-ring, that is supposed to expand to

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make contact with the metal underneath so that it makes a seal, is that the idea?

MR. MULLOY: Yes, sir. In the static condition it should be sealed to—it should be in direct contact with the tang and the clevis of the joint, and be squeezed 20 thousandths of an inch.

DR. FEYNMAN: And if it weren't there, if it weren't in contact at all and there was no seal at all, that would be a leak. Why don't we take the O-rings out?

MR. MULLOY: Because you would have hot gas expanding through the joint and destroy——

DR. FEYNMAN: Pushing the putty through, and so on?

MR. MULLOY: Yes. You will always push the putty through, because the motor pressure is 900 psi nominally, 1,000 psi at max, and that putty will sustain about 200 psi.

DR. FEYNMAN: Now, we couldn't put instead of this some sort of material like lead, that when you squash it it stays? It has to be that it expands back, because there is a little bit of play in this joint and it has to be able to come back. I mean, it is a rubber material, so that it comes back when you move a little, and it stays in contact, is that right?

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MR. MULLOY: Yes, sir, that is the purpose of the putty, as a thermal barrier, a thermal barrier. In the data that we have presented to the Commission, as you noted yesterday, we have looked at other alternatives, some of those alternatives are things like——

DR. FEYNMAN: I'm talking about the rubber on the seal?

MR. MULLOY: I'm sorry?

DR. FEYNMAN: In the seal, in order to work correctly, it must be rubber, not something like lead?

MR. MULLOY: Yes, sir.

DR. FEYNMAN: Because when the seal moves a little bit when there is vibration and pressures, it would lift the lead away, which the rubber expands in place?

MR. MULLOY: Yes, sir.

DR. FEYNMAN: So it is important that it have this property of expansion and not be plastic, like lead. And I think you call that resilience, right?

MR. MULLOY: That is correct. It has to have resiliency, and that is why we use an elastomer.

DR. FEYNMAN: If this material weren't resilient for say a second or two, that would be enough to be a very dangerous situation.

MR. MULLOY: Yes, sir.

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DR. FEYNMAN: Thank you.

MR. MULLOY: If it was rigid.

MR. HOTZ: Mr. Mulloy, could you tell us whether the shim that you have put in here to damp out some of the vibration is an original design consideration, or is that something you added as a result of experience?

MR. MULLOY: That was added. It has been on since the first flight vehicle. It was added as a result of experience during the early development testing on the motor. It is not for the purpose of damping vibration. It is for the purpose of assuring a uniform gap on the outside and maximum squeeze on the O-ring on the inside.

DR. COVERT: Mr. Mulloy, for purposes of my own understanding, I would like to have you go through the ignition process. And I find that I understand things best if I can feed them back to you so I want to ask a series of questions, and I will use this as an example.

This gap here is filled with the zinc chromate asbestos putty-like material.

MR. MULLOY: Yes, sir.

DR. COVERT: And it's designed to more or less be plastic?

MR. MULLOY: Yes, sir.

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DR. COVERT: Now, when you pressurize this side of it there is a little volume in here between the termination of the putty and where the O-ring lives, is that correct?

MR. MULLOY: Yes, sir.

DR. COVERT: And when you pressurize it, then, because the plastic is able to flow, it flows into this gap and compresses the air in that gap until the pressure is equal to the pressure in the combustion chamber, is that correct?

MR. MULLOY: That is one thing that could happen.

DR. COVERT: Don't confuse me with a lot of alternatives at this point.

[Laughter.]

DR. FEYNMAN: Why don't you put up the picture two from now, the one called "SRM No. 3."

MR. MULLOY: Would you put chart number three on, please.

(Viewgraph.) [Ref. 2/11-7]

DR. COVERT: Now, the point I want to get to at this point is that this O-ring then is subjected to the pressure that is caused by the plastic deforming and helping to fill this little cavity, and that in turn drives the O-ring into this crack in back of it. That

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is called extrusion, I believe?

MR. MULLOY: Yes, sir.

DR. COVERT: And so that is the mechanical joint that carries the pressure seal, is that correct?

MR. MULLOY: That is correct.

DR. COVERT: Now, if there was a flaw of some kind, then what would happen would be, instead of the plastic deforming and coming into this, then there would be hot gas flowing in a narrow jet into that cavity, is that right?

MR. MULLOY: A flaw in the putty?

DR. COVERT: Yes, sir.

MR. MULLOY: Yes, sir, a flaw in the putty would cause a hot gas jet to impinge on the primary O-ring.

DR. COVERT: So that would in turn then, that hot gas, would be what would drive the O-ring and cause it to extrude and carry the pressure load?

MR. MULLOY: The hot gas jet erodes the O-ring, and the pressure rising in the cavity tends to seat the O-ring.

DR. COVERT: So you have sort of a redundant system, then. The way the design works out is that there is—if the putty holds, the gas compresses the O-ring and extrudes it into the gap; and if the putty

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for one reason or another has a flaw in it and a little jet of gas comes in there, there is still a pressurization in there, and that causes this to be sealed?

MR. MULLOY: Yes, sir.

DR. COVERT: And the second O-ring then is a backup just for safety purposes?

MR. MULLOY: It was a backup to make the—to provide a redundant sealing capability.

DR. COVERT: Thank you very much.

MR. MULLOY: Yes, sir.

MR. WALKER: I have a question about the O-ring. The manufacturer generally specifies the amount of compression on the O-ring by specifying the depth of the O-ring groove. Is the compression that you get here equal to the amount of compression recommended for O-rings of this diameter?

MR. MULLOY: Yes, sir. The minimum O-ring compression that we have here is 7.54 percent, and that is within the recommended levels.

MR. WALKER: What was the impact of adding the metal plates which you put at each place where you have a steel pin? Was that to increase the compression, or what was the exact purpose of that?

MR. MULLOY: The primary purpose is to assure

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a uniformity of gap, and also then to assure that we would achieve the minimum compression by pre-shimming that to the 32 thousandths.

MR. WALKER: So are the shims placed on all 177 of the pin locations?

MR. MULLOY: Yes, sir.

MR. HOTZ: Mr. Mulloy, how are these materials, this putty and the rubber, affected by extremes of temperature, both hot and cold? Do they change their characteristics at all?

MR. MULLOY: Yes, sir, there is a change in the characteristic. As elastomers get colder, the resiliency decreases, and the ability to respond——

MR. HOTZ: Now, the elastomers are what?

MR. MULLOY: That is the Viton O-ring.

MR. HOTZ: The rubber?

MR. MULLOY: Yes, sir.

Now, the putties are not as sensitive as the elastomers are to the temperature over the range of temperatures we operate. Of course, temperature——

MR. HOTZ: How about moisture? Are the putties affected by moisture?

MR. MULLOY: Yes, sir. And in order to control that, we maintain the putty in a refrigerator and have limits on the time that it can be outside of

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the refrigerator before the joint is mated. What we have found, especially at the Kennedy Space Center, with the putties, that they do tend to take on moisture, and as the putty gets more moisture it becomes extremely tacky and sticky, which makes it very difficult to lay into the joint and to work with.

And it can take on enough moisture such that the putty loses its ability to hold together. So we control the humidity that that putty sees prior to installation into the joint until we have the joint made up.

CHAIRMAN ROGERS: Was the putty on flight 51-L the same quality putty you used on other flights?

MR. MULLOY: Yes, sir, it is the same putty we have been using since STS-8. It is a Randolph type two putty, zinc chromate with an asbestos filler.

CHAIRMAN ROGERS: The same manufacturer?

MR. MULLOY: Yes, that is the manufacturer, Randolph. We did have a change of putty in the program because the original supplier of the putty, which was Fuller-O'Brien, went out of making this particular putty because of its asbestos content.

CHAIRMAN ROGERS: When was the change made?

MR. MULLOY: STS-8. And somebody could help me with the date on that.

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CHAIRMAN ROGERS: How far back in terms of number of flights?

MR. MULLOY: This was the twenty-fourth, so 16 flights.

MR. HOTZ: Were you considering any further changes in the brand or the type of putty?

MR. MULLOY: Yes, sir, because asbestos products, of course, people are going out of the business of making asbestos every day. We were evaluating other putties. We were looking at a non-asbestos putty, as well as an Inmont Canada putty, which is asbestos-filled, and we have done some testing on that in some of our development motors that we have currently in test in the filament wound case program as an alternative to the Randolph putty.

But none of that has been implemented into the program yet.

CHAIRMAN ROGERS: Is there any reason why you were thinking of changing the putty, except for the asbestos problem?

MR. MULLOY: That's the only reason, sir, to have another source, not because of any concerns for the performance of the putty.

CHAIRMAN ROGERS: Then there is nothing unusual about the putty that was used in 51-L that you

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want to call to the Commission's attention?

MR. MULLOY: No, sir, not that I'm aware of. As I say, we're looking at all of the records and the paper to assure that the handling of the putty was as it was supposed to be, that the joint was mated within 12 hours of the time that the putty was first removed from the storage.

MR. HOTZ: But you did have some very high moisture conditions on the pad just before launch.

MR. MULLOY: Yes, sir. But we haven't seen any indication that, with the exposure of the putty, as Sally mentioned, even to sea water, we don't see that kind of breakdown in the putty when we get the hardware back for evaluation, just even after 30 hours in the ocean.

VICE CHAIRMAN ARMSTRONG: When we go to Kennedy, will we be able to see how this putty is applied?

MR. MULLOY: Yes, sir.

DR. COVERT: Do you throw away the O-rings after each use and put new ones in?

MR. MULLOY: Yes, the O-rings are single use items. You fly new O-rings on each flight.

MR. WALKER: A fairly detailed question. On your diagram, there is a gap at the end of the inside

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leg of the clevis—I mean, of the tang. The U-shaped device is—I mean at the other end, right there. Is that gap filled with putty or is that gap air, and the putty extrudes into that gap during launch, is that correct?

MR. MULLOY: That is air. And as I say, we take precautions to be sure that we hold the putty back off of here, such that during assembly the putty doesn't extrude down into the O-ring gap and unseat the O-ring. And yes, under pressure the putty tends to extrude into the gap.

It does not extrude totally into the gap, because, as I say, the putty won't sustain 1,000 psi, and in almost all instances, rather than the situation that Mr. Covert described, rather than a uniform decompression of putty, there is usually a breakthrough of the putty up at the 1,000 psi.

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We don't see when we get it back. We see putty getting further down than it was on assembly, but we don't see it extruded all the way into the O-ring gap.

MR. ACHESON: Have you experimented with material as a substitute or alternative to putty which would tend to fill that groove under high pressure and temperature?

MR. MULLOY: Fill this groove?

MR. ACHESON: Yes, sir.

MR. MULLOY: No, sir, because that is not a desirable situation to have anything that would fill and get into the O-ring gap at all. We have experimented with materials that are alternatives to putty which is in the data that I presented to the Commission yesterday, looking at carbon mesh, wire mesh, and channels that would allow uniform pressurization of the cavity to eliminate the hot jet impingement that goes through the putty and other alternatives.

At this point, with the testing that we have done over the last year, we have concluded there is no better alternative than the putty that we are using based on the testing that we have done.

DR. RIDE: What methods do you have to verify that the putty has been laid properly? Do you have any

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way of examining it after it has been laid to make sure that there are no air gaps?

MR. MULLOY: It is examined after it has been laid on, and I wish I had a diagram, but you will see it Thursday at Kennedy as to how that is laid up. But literally, what you do is you just lay these putty strips directly on the surface, and we use quarter inch strips here and then eighth inch strips, and they are laid up in a prescribed pattern. It is not an operator option to put enough putty on there to be sure you fill the gap. It is a drawing, it is controlled and it is inspected and signed off by quality inspection that the putty strips are installed in accordance with the procedure, and that procedure is to assure that the putty is laid in tightly onto the insulation and that you don't have air gaps in there.

And we have shown by tests that that provides the best thermal barrier. We have also shown when you deviate from that that the thermal barrier is compromised. So we are very, very careful about how that putty is laid up.

DR. RIDE: Have you had a chance to go back and look at the quality assurance records on 51-L and verify that those were signed off properly?

MR. MULLOY: That is in process now under Mr.

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Moore's Design and Data Analysis Task Force, and that is in process, and hopefully Thursday at KSC you will be able to see some of the certification of the rereview of those records. All those records are at KSC where the assembly is made. All of them are under the control of the NASA investigative board, the interim board, and now the task force. But nothing during the assembly of 51-L where I get involved in that assembly process, if there is some requirement to deviate from the requirements that we have for the assembly, then I would get involved in that because it would require a waiver to deviate from that.

I have checked with my managers and with the contractor at Morton-Thiokol and the manager of the solid rocket motor project who works with me at NASA, and they assure me that they have no recollection of any deviations being worked in the assembly of 51-L.

GENERAL KUTYNA: Mr. Mulloy, we have had a history of some problems with these O-rings since about 1980.

Could you summarize the history of the erosion problems and the blow-by and when they occurred, the conditions under which they occurred?

MR. MULLOY: Yes, I can. I think it would be useful, if I may, to proceed through the next diagram

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and then move into what causes the erosion, and review—it is not in this presentation today, but to review with you the numbers of instances that we have had in a summary fashion that summarizes the detailed data that I presented to the Commission yesterday.

Next chart, please.

(Viewgraph.) [Ref. 2/11-8]

DR. WALKER: Before you leave that chart, I have one more question.

MR. MULLOY: Go back to Chart 3.

(Viewgraph.) [Ref. 2/11-7]

DR. WALKER: How wide is the gap between the insulation pieces of the two different sections where the putty goes?

MR. MULLOY: Let me get those dimensions out of the presentation from yesterday.

The gap size on the field joint varies from .01 down at this narrow section here up to .4 inches at the top, and the length of this channel right here is 3.3 inches.

DR. WALKER: So is the putty just laid into that gap, or is it worked into the gap?

MR. MULLOY: No, the putty is laid on before the joint is assembled. It is laid on to the surface here and then assembled in a very precise, precisely

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controlled pattern to assure that we don't get any voids, or minimize the voids that we have in the putty.

DR. WALKER: And then it is visually inspected after the joint is made?

MR. MULLOY: Well, you can't inspect it after the joint is made. All you can see is that you have extruded the putty out of the joint which you would expect to do under the configuration that we have it in prior to the lay-up. You would expect to see this kind of a bead here. The inspection is, if you didn't see the putty coming up to here, obviously it wasn't laid up properly. But the inspection is made prior to pushing the joint together, and we have many, many tests that assure that if you lay the putty in that way and then assemble the joint, you will get a fill with minimum voids.

Let me back up one chart to talk more generally about joints.

Let me have Chart 2, please.

(Viewgraph.) [Ref. 2/11-8]

MR. MULLOY: The joint we have been talking about is represented here for these three field joints. These are the three joints where the four segments are tied together. As I mentioned, there is an identical joint in each one of these segments which is covered

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with the rubber in the casting process at Thiokol. There is another joint which has erosion history on it that is not the case-to-case joint but it is the—where the nozzle is attached to the aft end of the solid rocket motor. That configuration is significantly different than the case-to-case joint.

And in the case joints we have two O-rings in series on the same bore, if you will. On the nozzle joint it has this right angle sealing surface here, and when the nozzle is inserted, and I will show you on a bigger diagram, there is an O-ring that is a face seal as well as a bore seal.

So let me go to Chart 5, please.

(Viewgraph.) [Ref. 2/11-9]

MR. MULLOY: Chart 5 is a larger diagram of that nozzle-to-case joint where we have also experienced some O-ring erosion, and this shows the two O-rings, this being the face seal and this being the bore seal. Because the tolerances are somewhat tighter on this joint than we have on the case-to-case joint, this O-ring groove is somewhat wider than the O-ring groove on the case joint to assure that we can assemble this nozzle to the case without damaging that O-ring.

Now, let me move forward.

MR. ACHESON: Let me ask why the tolerances

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are less tight on the field joints?

MR. MULLOY: Well, because the 146 inch diameter that has to be mated to assure that we don't have any gathering of the material when we mate the joint or puckering, there has to be somewhat more tolerance in that. This is 103 inch diameter versus 146 inch diameter.

Okay, let me go to the next chart, please, which would be Chart 5.

(Viewgraph.) [Ref. 2/11-10]

MR. MULLOY: Or 6.

CHAIRMAN ROGERS: Excuse me.

Would the television people, is it necessary to have the lights on so bright? It is really intolerably hot here.

Is there any way to turn them down a little bit?

MR. MULLOY: General Kutyna, I am going to get to your summary, if you will allow me. I would like to give a little precursor to that that I think leads in to how we dealt with those data relative to 51-L.

And as Mr. Moore has mentioned, we do have a very thorough preflight review process for the solid rocket booster. That preflight review process starts with the recovery of the hardware from the last flight,

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because we are very sensitive to anything that we see on the last flight that might pose a consideration that we should have for the flight readiness of the next one. So we have had that opportunity to go back and do a detailed examination of the hardware from the previous flight prior to committing to the next one.

The key thing in our flight readiness review is that previous flight performance. We look at the ballistic performance of the motor, and then particularly any problems from the previous flight.

Now, we have not had in the solid rocket motor in terms of ascent performance, we have had no anomalies related to ascent performance in the motor. What we have seen on recovering the hardware are some things that would indicate that there are some improvements that could be made in the design to provide more margin, and the particular point of interest here is the case joints and the nozzle joints, and particularly the erosion of the O-ring seals. So we have dealt with that finding on all previous flights in the flight readiness for all subsequent flights, and 51-L was no different.

Sometimes as the flight frequency increases we are in a situation where we have something from two flights back, maybe, that is still under analysis that we, even though we were able to continue with the

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previous flight, if that isn't closed out, we look at it even for the second flight downstream. There are several things in the SRB world that have fit in that category. One of them has been some damage that we have been getting to rate gyro assemblies just due to the splashdown and the tow back and the porpoising as we tow the boosters back. We have been trying to work that problem. That is a reuse issue.

The thing of interest here is what have we seen in the O-rings. Now, the fact is, before 51-L we hadn't seen any anomalous erosion for about a year. The O-rings had been performing very well. The last time we had seen any erosion on O-rings was the January launch the year before. But we were very sensitive to, mainly because of the activity that we've had going on in the last year to try and improve the margin in that joint, we had been very sensitive to how that was going on, and we were continuing to look very carefully at the previous flights to assure that nothing had changed in that area that would change our rationale that we had developed for continuing to fly in light of the erosion we were seeing on the O-rings.

We considered that in 51-L, and concluded, particularly since we had not seen any significant erosion in the last year, and we had no test data that

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changed our rationale, the same rationale then applied for 51-L as it applied to the last year in the flight readiness review.

Then we looked very carefully at the flight performance requirements for our next flight. In the case of the solid rocket booster, those performance requirements are in terms of the ballistic performance of the motor. And we review the small motor testing that was done at Thiokol to characterize the ballistic performance of the propellant that is in this particular motor. That was done in this case.

And then we go through our complete certification and verification status, and this is where I gained my confidence that there wasn't any kind of a waiver or deviation in the assembly process of 51-L because we review all of those at that point in the flight readiness process, and none are in the record, and I am confident none were brought to my attention.

Next chart, please.

(Viewgraph.) [Ref. 2/11-11]

CHAIRMAN ROGERS: Could I say on that that the only thing you say that you have had a history of one year's success with the O-rings previous to flight 51-L.

MR. MULLOY: Yes, sir.

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CHAIRMAN ROGERS: The only thing that might be different or that might affect the O-rings differently was the weather then?

MR. MULLOY: Yes, sir, and I am addressing at this point the flight readiness review process, and at that point the weather was not a factor, and I will get into the one day prior to launch consideration of the weather.

CHAIRMAN ROGERS: Okay. I think we prefer, and I don't want to disrupt your presentation, but I think we should have a full session just on weather so we can focus on it.

MR. MULLOY: Okay. There are no charts in here on that.

Okay. The levels of review. In the case of the SRB, we do require that our contractors have a flight readiness review process that covers all of that information, and that is documented. That is chaired by a Senior Vice President at Thiokol above the level of the program manager.

And he uses other people at the Wasatch Division who are not on the SRM project to do that flight readiness review.

Then my element managers, I pointed out that I have essentially two contractors on this.

MR. HOTZ: Could we have his name, please?

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MR. MULLOY: Yes. That would be Calvin Wiggins.

MR. HOTZ: How do you spell that?

MR. MULLOY: W-i-g-g-i-n-s. He is the Senior Vice President in charge of the Space Division at Thiokol. Thiokol is organized into three divisions, the Space Division, Tactical and Strategic. The SRM program manager works for Mr. Wiggins. The SRM program manager is Mr. Kilminster.

MR. HOTZ: How do you spell that?

MR. MULLOY: K-i-l-m-i-n-s-t-e-r.

MR. HOTZ: First name?

MR. MULLOY: Joseph.

Then my element managers then go through that same review process at a minute level of detail, and I think when the flight readiness review proceedings are presented to the Commission, you will find that there is a great deal of detail reviewed relative to the configuration and the performance predicted for the particular solid rocket motor that is going to fly.

And then I chair a review with senior managers at the Marshall Space Flight Center in the Science and Engineering Directorate there where my element managers and contractors present that to me. I am then required to review that with the manager of the Shuttle Projects

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Office at Marshall, and then we have a center review with Dr. Lucas which includes all of the elements of the Marshall Space Flight Center, and that is a very thorough review. And then the level 2 National Space Transportation System manager reviews the flight readiness, still using the same agenda, the same agenda items in every one of these.

And then Mr. Moore, who is the Associate Administrator for Manned Space Flight has the level 1 review. And then one day before launch what we call now L minus 1, we used to do it L minus 2, but lately it is L minus 1, the level 1 has a review to assure launch readiness, and the purpose of that review is to assure that nothing has changed in the two weeks since they had—Mr. Moore had his level 1 flight readiness review. Any deltas that occur are then presented to that board.

I can give you one example of where nothing was presented relative to the solid rocket booster until we got to the L minus 2 day review, and that was because we were at a frequency of flight that we did not get a look at the joint between the nozzle and the case joint until after the level 1 review, and you will find in the record that at the L minus 2 day review I presented the details of our observation there and the rationale for

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flight for that.

And then in the case of 51-L, up through the L minus 1 day review, no concerns regarding SRM joint O-ring erosion were expressed during any of that process.

Now, I would like to get into what the basis for that was at that time. If you will go to, I guess it is Chart 7—next chart, or Chart 8——

(Viewgraph.) [Ref. 2/11-12]

MR. MULLOY:—our experience when we were looking at 51-L that we were looking at—and this is a summary of the detailed information that was provided to the Commission yesterday—our experience was that prior to 51-L we had eleven static test motors and 48 flight motors. The field and nozzle joints of those 57 motors had been examined which represent some 288 joints with 456 O-rings, and this is a summary again of the detailed data. Six of the 171 field joints exhibited some erosion of the primary O-ring.

Now, if I may, I would like to go to Chart 10 and then come back to this one.

(Viewgraph.) [Ref. 2/11-13]

MR. MULLOY: I think it is helpful when we say some erosion, what kind of mechanism we are talking about there. This is the nominal configuration I have

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already shown you where we have zero pressure in the motor and we are ready for ignition. We have these two O-rings in here which are specified to have a 20/1000 inch minimum compression such that when they are called upon to do so, the primary O-ring can be extruded into this gap and form a seal.

If it did not have the compression on it, the gas—and I will show you a scenario there that does that—the gas can blow by the primary O-ring, and it will never seat. So you have to have that compression, and we are sure we have that from the dimensions of the clevis and the tang in the steel, and assuming a minimum O-ring with—and some compression set in that O-ring, to account for the resiliencies that it has to follow the metal as it expands.

Now, as the motor is pressurized, and if I may take that down for a moment and bring up Chart 9——

(Viewgraph.) [Ref. 2/11-14]

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MR. MULLOY:—this is a typical pressure time trace. Full pressurization of the motor from time zero occurs in 600 milliseconds, .6 second. And then we are up here at the maximum pressure, operating pressure, nominally 900. It is qualified for 1004, but 900 is the nominal operating pressure, and we stay there for about 20 seconds, and then we have a thrust tail-off to limit the G forces on the Shuttle vehicle to 3 Gs, and this thrust profile is designed to do that in conjunction with the throttling of the engines to such that we don't exceed a 3 G load on the vehicle.

And then at about 50 to 60 seconds, we start ramping back up again, and that is what this bar indicates. And so there are two times in the motor operation when the motor is increasing in pressure, and that is in the first 600 milliseconds, and in the 50 to 75 second timeframe.

So what is going on in this first 600 milliseconds, if I may go back now to the previous chart, Chart 10——

(Viewgraph.) [Ref. 2/11-13]

MR. MULLOY:—the nominal situation, and I pointed out to you that only six of 171 have exhibited any erosion, and so the other 165 of them performed as you see in this diagram here, which is the intended function

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of this joint.

Two things happen with that motor pressurization. The additional tension loads are put into the case due to the pressure. The 1000, getting up to that 1000 psi tends to want to pull this joint apart. That pulling on these pins then tends to rotate this clevis outward as you pull with tension load on the pins. The other thing that is happening is the outward pressure on the motor is tending to want to expand the motor more out here in the membrane area of the case than it is

in the stiffer joint section, which further tends to cause a rotation of this clevis upward, which tends to reduce the initial squeeze on the O-ring.

Well, what has happened in 165 of the 171 cases, when this O-ring was called upon to exercise itself, it did so by extruding into this primary groove. You see no erosion on the O-ring. You don't see any soot blow by the O-ring, and the secondary has not even been energized because the primary has done what it was designed to do.

Any questions about that?

MR. HOTZ: Could you describe the rotation again? I am not quite clear on that.

MR. MULLOY: Okay, sir. The motor is at zero

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psi, zero pressure. We come up in 600 milliseconds to 900 psi. So there are two things that that case is wanting to do. It is wanting to expand outward due to that pressure, and it is also wanting to expand longitudinally. The longitudinal load pulling on that pin tends to want to rotate that clevis. In other words, if you can visualize, if you pulled on that long enough to fail it, the clevis would open up until the tang end pulled out of there.

The other thing that is happening is that that joint is much stiffer. It is like having a belly band, if you will, around a balloon, a belt around a balloon. Now you blow up the balloon, or say an elastic belt around the balloon. Now you blow up the balloon, the balloon will expand more where the elastic belt is than where it isn't. And so you get this exaggerated shape like that which further tends to rotate the joint.

VICE CHAIRMAN ARMSTRONG: What holds the pin in place?

MR. MULLOY: The pin is held in place by a metal strap. After all the pins are put in, there is a metal strap that is put around the pins and cinched down just like a container strap mechanism, and then that is closed out with cork. A quarter inch of cork is put around that just over, just over this section of the

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clevis, and that cork is there to assure that during ascent, since the heat sink of that band is much less than the heat sink of this whole mass here, if we got any aerodynamics under that band, that band could heat up and fail, and we could lose the pins during ascent. So the cork is a thermal protection for the retaining band on the pins.

DR. WALKER: I have a couple of questions on the O-ring. There are some tolerances on the diameter of those O-rings.

Do you inspect each O-ring to see that it is within tolerance?

MR. MULLOY: Yes. The tolerance is plus 5 and minus 3, and the O-ring is inspected with a micrometer on receipt to assure that it is within tolerance.

DR. WALKER: Do you inspect it at many places along its length?

MR. MULLOY: Yes. I believe it is every two feet, and relative to 51-L, all of the O-rings that are in the inventory are being re-inspected to get a statistical data base to try and understand if possibly there could have been an undersized O-ring, for instance, in 51-L.

VICE CHAIRMAN ARMSTRONG: Is there a tolerance on this pin and these holes that isn't shown in that

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diagram?

MR. MULLOY: I am sure there is, Mr. Armstrong. I am not sure exactly what that is. It is not an interference fit. There is a tolerance. I can get that for you.

CHAIRMAN ROGERS: Mr. Mulloy, on your O-ring history, which is a very useful review, I think, for our purposes, would you mind taking each one of these observations and just making some comment on them? For example, you say that you examined 228 joints with 456 O-rings. The first observation is 6 out of 171 exhibit some erosion of the primary O-ring.

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: That did not disturb you, I suppose? You would like to correct it, but it wasn't, in and of itself, it didn't disturb you too much?

MR. MULLOY: It wasn't disturbing from a standpoint of safety because the O-ring, even though it was eroded, had done what it was designed to do. It was disturbing from the standpoint that we were looking for ways to increase the margin such that we wouldn't even have that incidence of erosion.

CHAIRMAN ROGERS: Going to the second, you say that two of those joints, there was some soot behind the primary O-ring.

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Was that a serious problem, and if so, what was the problem?

MR. MULLOY: That is more disturbing than just having the O-ring erosion and then the joints or the O-ring then seated, although this, too, is in the population of the six. The six with erosion, two of the six with erosion showed soot. The concern there is that there is some blow by the primary which says that we are concerned that we have adequate squeeze on that primary such that we won't get that blow-by, and it will energize and go into, extrude into the gap without blow-by.

And yes, that is where we started looking at things like how can we decrease the joint rotation.

CHAIRMAN ROGERS: And were those instances just prior to 51-L or a long while back?

MR. MULLOY: No, sir. As I said, there were no instances of that for a year before 51-L.

CHAIRMAN ROGERS: On the next one, 16 of the 57 nozzle joints exhibited some erosion of the primary O-rings.

How did that relate in terms of concern to the other two? Was that more serious or less serious or about the same?

MR. MULLOY: It is about the same. There is

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more concern for the case-to-case joints because of that rotation. In the case of or in the instance of the nozzle-to-case joint, we don't have that same rotation of the joint. So we deemed that fixing, improving the margin in the case joints by reducing the rotation to put it in the same population as the nozzle joint would be a desirable improvement.

CHAIRMAN ROGERS: The next one refers to soot in the nozzle joints, eight out of 57, and would the answer be the same?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: And then you have one nozzle joint secondary O-ring which has been eroded. Was that of particular significance?

MR. MULLOY: Yes, sir, it was because until we saw that, we were always assured that even though we were causing some initial damage to the primary O-ring, that the primary O-ring was functioning. In the instance of that nozzle joint, we were now seeing a violation of the secondary seal, and we did after that, and before we would commit to another flight, we went and did some extensive testing of the tolerance to that, and did an analysis that matched that testing so we could determine what the limiting mechanism was, in other words, how long, if you—since the size of that cavity behind the

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primary O-ring is of limited volume and you are pressurizing that cavity with a very large volume of gas, which is inside the solid rocket motor, there is a limit of time that that gas can flow into that cavity.

Once the gas flows into that cavity and the pressure becomes the same as in the motor, flow stops. So our rationale was through testing, can we get enough damage to the secondary O-ring before the flow stops such that we would have a failure of the secondary O-ring? And our analysis and our tests which the analysis correlated very well with, said that we had a margin of three—we could take three times what the maximum amount of erosion that we had observed and have a margin of two on what was theoretically probable under the limited time that that flow could occur. And thus, we concluded that that was an acceptable situation.

We have not had any other secondary O-ring erosion on any joints since that instance.

CHAIRMAN ROGERS: So you were satisfied based on that experience that you did not have a problem with 51-L in that connection?

MR. MULLOY: In that connection, yes, sir.

MR. ACHESON: If soot blows by the primary seal, will it lodge between the secondary seal and the wall of the chamber to prevent a tight squeeze?

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MR. MULLOY: No, sir. We haven't seen that. What we tend to see is that that soot is very, very fine. It is the products of grease, the pyrolysis of the grease and some pyrolysis probably of the O-ring itself, the primary O-ring, and it is a powder, and blowing—it would have to blow by the secondary before it could compromise that, and we have seen no evidence of that at all.

You see a kind of a fan-shaped sooty spot in the putty, if you will, impinging but not into the groove generally. You will see that in the primary groove, but you do not see it in the secondary groove.

DR. WALKER: A question on the six incidences of erosion of the primary O-ring in the field joints.

Was each of those associated with some channel or damage to the putty?

MR. MULLOY: Yes, sir. There is a track through the putty to that erosion.

DR. COVERT: Mr. Mulloy, on the six rings that exhibited some erosion, do you have numbers comparable with those on the nozzle joint erosion? In other words, you eroded half of those needed or what?

MR. MULLOY: Yes, sir. Theoretically we have a factor of two, based upon tests and analysis, over the maximum observed.

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MR. FEYNMAN: Sir, you suggested that if the primary O-ring were to fail, it is still no big problem because the secondary O-ring might hold.

MR. MULLOY: Yes, sir, it should energize.

MR. FEYNMAN: But there is a way for the gas to come out, at least possibly, and that is the leak test port that you put in to make a pressure to test the O-rings, and I wonder how good we can expect—how was it sealed? How was it closed, and how good is it? Can we guarantee that that might not fail, that is, the gas come out through the hole that you used to put the pressure on to test the O-rings to see if they were okay earlier on?

MR. MULLOY: Yes, sir, that is installed to a torque requirement and inspected and signed off. It is installed to that torque requirement, and then there is extensive test data that indicate

with that test plug, and it has the O-ring sealing surface also at that torque, will not leak at 1000 psi.

But if a human error was made and the test port plug was left out, obviously—and you went by the primary seal, that would be a leak source, or if it wasn't properly torqued, there could be a leak source through there which could lead to failure of the secondary O-ring.

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CHAIRMAN ROGERS: How many checks of human failures do you make? In other words, if one person—do you have to rely on the one person's activity, or does somebody supervise that one person? How many checks do you make to avoid that kind of human failure?

MR. MULLOY: Well, the first check is the technician who makes the installation. The second check is by the contractor, quality inspection, of whoever is making that installation, and then there is a government inspection check on that.

CHAIRMAN ROGERS: Who makes that? Who is the government?

MR. MULLOY: In the instance of the leak check port, for instance, that is Air Force Quality at KSC.

CHAIRMAN ROGERS: Yesterday we talked about the orientation of the leak ports on the two solid boosters. Since we have a model here, could you indicate where those were?

MR. MULLOY: Yes, sir. On 51-L, on the right hand booster, it is located on this axis, and on the left hand booster it is located on the other axis.

MR. HOTZ: Mr. Mulloy, while you are at the model, could you indicate and describe for us the pressures on the solid rocket boosters that are caused by the so-called Twang maneuver just before launch?

MR. MULLOY: Yes, sir.

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The main engines ignite approximately 6 seconds before the solids ignite. That timing is set such that when the main engines ignite, and the Shuttle stack is bent over, the hold-down point for the whole stack is here on four points at the bottom of the aft skirt on the solid rocket booster. That is restrained. So there is a bending in this direction.

So the stresses that are put on are bent—it is a bending moment.

MR. HOTZ: They are bending forward in the same direction as the Shuttle?

MR. MULLOY: Yes, in this direction because the only thing that is applying force are the main engines over here, which is an eccentric kind of a load, which tends to rotate it this way. And then the timing of the ignition of the boosters is timed such that you are back in the vertical position at ignition.

MR. HOTZ: Have you any measurement of the quantity of the force that is put on the SRB?

MR. MULLOY: Yes, sir. That was done in the facility verification vehicle early. There was a stack made, and then there was what was called a Twang test and a deflection test that was run on the boosters where they were deflected for the maximum predicted amount and the strains measured, and that is accounted for in the

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design.

In other words, they were actually pulled over from this forward——

MR. HOTZ: What was that maximum predicted amount, do you recall?

MR. MULLOY: The deflection?

MR. HOTZ: Yes, sir.

MR. MULLOY: No, sir, I don't recall what that is at the tip. It is very visible in watching launches.

MR. HOTZ: Thank you.

DR. RIDE: Have you—when you have gone back and inspected the O-rings that have experienced erosion, have you seen the erosion occur at the same point circumferentially on the different O-rings?

MR. MULLOY: No. In the data that I presented to you yesterday, you will see on the field joint that there is no preferred location on the case field joints, and those six occasions, you find that scattered over all 360 degrees of the circumference, and the same is true in the nozzle-to-case joint. There is no preferred location. It seems to be random in circumference, more related to the point, I think, where the pressure breaks through the putty as opposed to any loads or gap dimensions in the joint.

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CHAIRMAN ROGERS: Mr. Mulloy, just to clarify for the record, the material you presented to us yesterday is not any different than the material you are presenting today, is it?

MR. MULLOY: No, sir. What I have done today is on that one chart that we are dealing with here is summarize the details of all of those detailed observations by flight number, degree of erosion, location of the soot around the azimuth, etc.

DR. WALKER: A couple of times you referred to the vacuum grease that you use on the O-ring.

Is that silicone grease, and could you explain the purpose?

MR. MULLOY: That is an HD-2 grease, and it is not silicone.

DR. WALKER: What is the purpose of putting grease on the O-ring?

MR. MULLOY: The purpose of putting grease on the O-ring is to ease the installation of the O-ring into the joint and assure that you don't damage it. The purpose of having the grease in the joint is to keep the D-6 from getting into the condition that that sample is in.

Are there any further questions on this particular chart?

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CHAIRMAN ROGERS: I guess not.

MR. MULLOY: Okay, sir.

Let me move forward to some other conditions of primary O-ring erosion.

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MR. ACHESON: In the examinations of either the test segments or the flight segments, have you ever found test port damage?

MR. MULLOY: No, sir, no evidence of any heat damage through a test port.

DR. RIDE: What does the plug on the test port look like?

MR. MULLOY: It is a cad-plated steel, oh, about a quarter inch in diameter, and then it has—it looks like a screw, and it has—on the head it has an O-ring. Let's go to chart 11.

(Viewgraph.) [Ref. 2/11-15]

Continuing with the physical explanation of what we have seen, this is a case where we see two of the six that did erode on the primary. We show soot passed the primary O-ring with no damage to the secondary O-ring, and that soot we're talking about is in this area here, between where you see the two O-rings on this diagram.

What is happening there is the pressure rises from about zero to 200 psi, on its way to 900 psi. We have a concentrated hot gas jet through the putty that impinges on that primary O-ring

and begins to erode it while it is being transported to its seated position. While it is being transported, then there is some

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blow-by, and that is where you see the soot deposit, and then the O-ring extrudes into the gap and the erosion continues for a short time and then stops, and the pressure rises to operating pressure and the primary O-ring remains fully seated, and erosion on the primary O-ring stops at that time.

And that has been observed. We have had the erosion on the six of the 171, and that is the mechanism by which you can have erosion on the primary, the primary seats, but you still see soot behind the primary, because that soot is being blown by in the seating process and extruding into the gap.

(Viewgraph.) [Ref. 2/11-16]

Chart number 12 is a case where we see soot without any erosion. There is a deposit of soot behind the primary O-ring, but there is no observable erosion on the O-ring, as we reported. And that happens very early in the ignition transient from zero to 50 psi, where you have the blow-by.

But it is of such short duration, in that first few milliseconds, that no erosion occurs to the primary O-ring, and it then extrudes into the gap and seals. So that is a configuration that goes with that observation.

And so all of these are what we expect the

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joint to do, primary O-ring sealing. Now, what we have not seen on any of the 171 joints examined is what I will show on chart 13.

(Viewgraph.) [Ref. 2/11-17]

Which, as we're going from zero to 200 psi on the way to 900 psi, the primary O-ring does not seal. The secondary seal is energized and seals, because all of the pressure now is on the secondary seal, and it does what it is supposed to do.

It extrudes into the gap. The pressure rises on up to operating pressure, and during that blow-by of the primary seal, primary erosion occurs, and erosion of the secondary occurs, but it does not compromise the integrity of the seal. On any of the hardware we have examined, we have never seen that condition.

VICE CHAIRMAN ARMSTRONG: How can you tell that, because the primary seal is essentially intact?

MR. MULLOY: No, sir. Because the secondary seal is not eroded. That is a condition where the secondary O-ring erosion occurs, and we have never seen that on a case to case joint.

MR. ACHESON: Does erosion depend upon blow-by, or can you have erosion just because it is in the presence of the heat and the pressure without

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blow-by?

MR. MULLOY: That first case that I showed, six of them, that is that case. There is no blow-by, there is erosion, but no indication of blow-by of the secondary seal or the primary seal. That says that you've got compression on the O-ring. As the pressure hits it, nothing gets by it, just due to the compression on the O-ring.

The O-ring is—there is a period of time, though, that a jet is impinging on that O-ring, until such time as the cavity between the putty and the O-ring reaches motor pressure, flow stagnates and there is no further flow and no further erosion.

That is the highest incidence of what we have seen, erosion without blow-by. This is just something, Mr. Armstrong, that I'm saying can happen with this design, but it hasn't.

Okay, if we could go to chart 14.

(Viewgraph.) [Ref. 2/11-18]

Now, this is a case of a failure of the joint, which on any of the 171 joints we have examined we have not seen. The motor pressure rises from zero to 200 psi, the primary O-ring does not seal, it doesn't have sufficient compression to seal. The joint rotates during the pressurization cycle and the O-ring squeeze

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is lost on both of these O-rings.

As we go up to the maximum predicted operating pressure or, in the case of 51-L, 900 psi, the secondary O-ring seal does not seal, and we have hot gas blow-by and we have eroded the seals where they are no longer capable of sealing in the rotated condition, the hot gas blows by and exits through the joint.

As I say, we have seen certainly no evidence of that on anything we have examined to date.

Okay, if I might go to the next chart, please.

(Viewgraph.) [Ref. 2/11-19]

I have taken you through kind of our flight readiness review process and our rationale for accepting the conditions and tried to explain with some sketches the configuration that matches the observations that we have had. We have understood that condition for some time, and our rationale for accepting that is, as shown here, the jet impingement erosion has been shown by test and analysis to be within acceptable limits.

The analysis that is done is gas flow, heat transfer analysis, and structural analysis, by tests of full-scale motors, sub-scale motors, and a test that we did of O-rings that were damaged to the extent that it was two to three times, between two and three times the

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maximum observed erosion, and two times greater than is what is predicted can occur with the time that flow can exist.

And we subjected that to 3,000 psi, which is three times the maximum expected operating pressure, and we had successful sealing with the O-rings in numerous tests. That is not a single data point.

And the other thing that leads us to accept that is that the detailed post-flight inspection that we have been doing, the very detailed analysis of those results and the tests run, to understand the limiting mechanism that does exist in that, assures understanding of those observations and a logical engineering judgment that the safety is not compromised by what we have observed.

The next chart, please.

(Viewgraph.) [Ref. 2/11-20]

In summary, I have discussed for you primarily the case joint, because that is the expressed area of interest, and the data and design analysis task force has not concluded that the failure that we have discussed here or the scenarios that I have discussed here is the cause of the 51-L incident.

However, the joint also has not been eliminated as a cause or as a factor in the accident,

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and of course the media and all of us have seen the film that shows the plume coming from the side of the solid rocket motor. But we have not yet determined whether this is a cause or an effect.

GENERAL KUTYNA: Mr. Mulloy, might I ask, what further tests will you do in this area to determine whether it is a cause or an effect?

MR. MULLOY: The testing that is going on now is primarily aimed at understanding the behavior of this joint and the O-rings under the specific conditions of the 51-L launch, particularly the temperature, the humidity environment, the loads, and all other factors associated with 51-L.

And there is extensive testing and analysis going on now in this area, as well as other, as Mr. Moore mentioned, in the other areas of the investigation as a potential cause.

GENERAL KUTYNA: When might we see some results of these tests?

MR. MULLOY: I think you will see some of this on Thursday.

MR. HOTZ: Have you been able to analyze any further exactly where that flame first started on the casing?

MR. MULLOY: No, sir. I have looked at the

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film many times, and we know that the flame is in the vicinity of this attach point in this quadrant here. But in what I have seen we have not been able to pinpoint the location of that at this time.

MR. WALKER: How far away is the attach point from the nearest joint?

MR. MULLOY: About 12 inches, I think. Yes, about 12 inches above this ring that you see here, which is the ET attach ring, and the joint is approximately 12 inches.

MR. WALKER: The joint is above the ring?

MR. MULLOY: Yes, sir. The ET attach is part of the aft segment.

MR. ACHESON: Can you show us on the model where the test port would be located?

MR. MULLOY: Yes, sir. In the joint on the right-hand side—is this the right-hand? No.

On the left-hand side, it is on the plus Z axis, which is the crew heads-up position of the orbiter; on the right-hand side, it is at the zero degree position on the minus Z, and they all line up.

CHAIRMAN ROGERS: Any other questions for Mr. Mulloy?

(No response.)

CHAIRMAN ROGERS: If not, I would like to ask

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one or two questions.

Have—as you know, the press has reported a letter, the contents of a letter dated July 23rd, 1985, written by Mr. Cook to Mr. Mann. Prior to the accident, had you seen that letter?

MR. MULLOY: No, sir.

CHAIRMAN ROGERS: You have seen it now, I presume?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: Is your assessment that you've given us this morning affected in any way by the letter? Has it changed your views at all?

MR. MULLOY: No, sir.

CHAIRMAN ROGERS: Mr. Cook is here and we are going to ask him to testify and give his comments, and he can refer to anything he'd like to. I would like to suggest that you listen to

what he has to say and then we, if you would like, after that some time this afternoon, to make further comments about that letter and anything he may say.

We want to give him the opportunity to appear and for the Commission to consider his thoughts, particularly because of the visibility that resulted from the New York Times story, and to have you and anybody else that you want comment upon the contents of

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that letter.

Is that okay with you?

MR. MULLOY: Yes, sir.

CHAIRMAN ROGERS: Then why don't we have a ten minute recess and reconvene after the recess.

(Recess.)

CHAIRMAN ROGERS: Could we ask the Commission to reconvene, please.

Before we start with Mr. Cook, Dr. Feynman has one or two comments he would like to make. Dr. Feynman.

DR. FEYNMAN: This is a comment for Mr. Mulloy. I took this stuff that I got out of your seal and I put it in ice water, and I discovered that when you put some pressure on it for a while and then undo it it doesn't stretch back. It stays the same dimension. In other words, for a few seconds at least and more seconds than that, there is no resilience in this particular material when it is at a temperature of 32 degrees.

I believe that has some significance for our problem.

CHAIRMAN ROGERS: That is a matter we will consider, of course, at length in the session that we will hold on the weather, and I think it is an important

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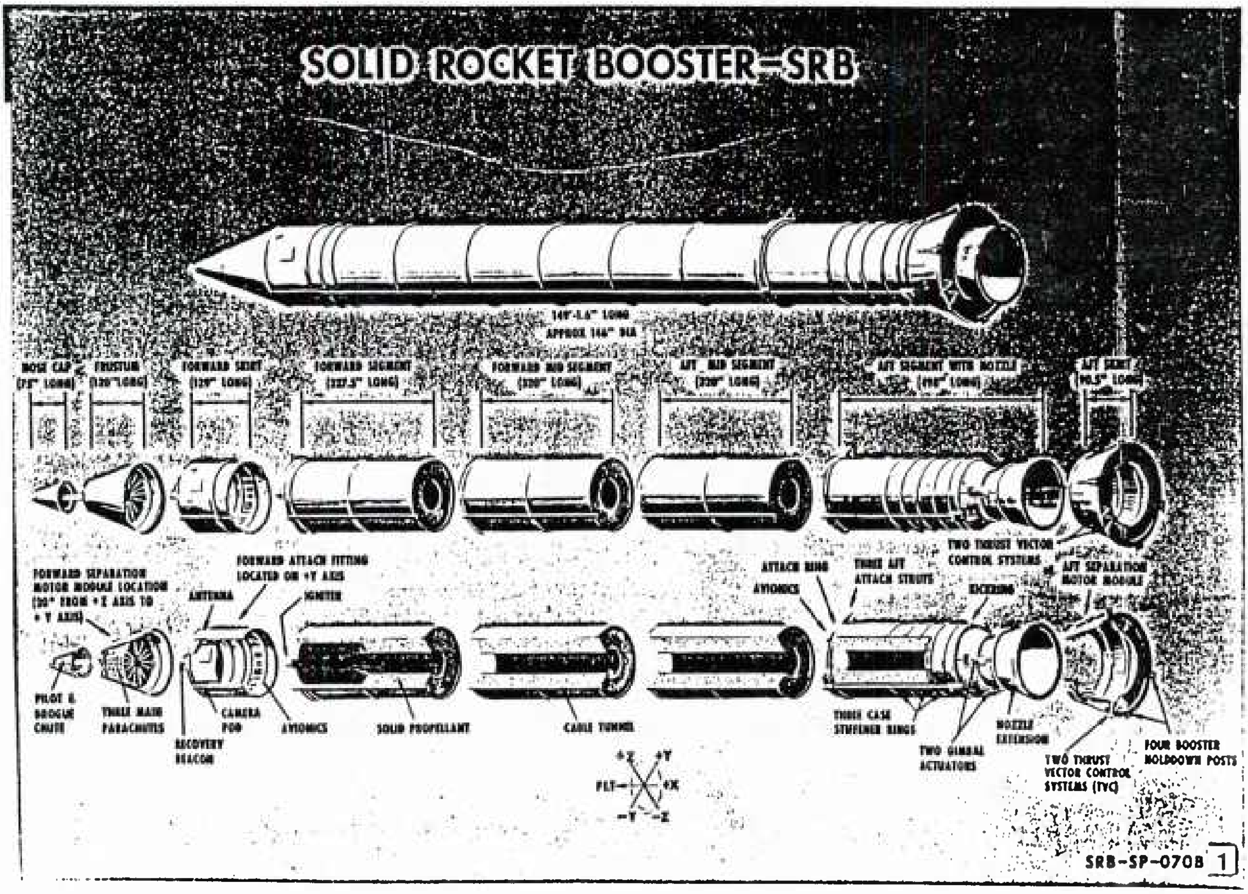
point, which I'm sure Mr. Mulloy acknowledges and will comment on in a further session.

Now, if I may and if there are no further comments, I would like to ask Mr. Cook to come forward.

(Witness sworn.)


SOLID ROCKET BOOSTER—SRB
NOSE CAP
FRUSTUM
FORWARD SKIRT
FORWARD SEGMENT
FORWARD MID SEGMENT
AFT MID SEGMENT
AFT SEGMENT WITH NOZZLE
AFT SKIRT
FORWARD SEPARATION MOTOR MODULE LOCATION
FORWARD ATTACH FITTING LOCATED ON +Y AXIS
ANTENNA
IGNITER
PILOT & DROGUE CHUTE
THREE MAIN PARACHUTES
RECOVERY BEACON
CAMERA POD
AVIONICS
SOLID PROPELLANT
CABLE TUNNEL
ATTACH RING
AVIONICS
THREE AFT ATTACH STRUTS
TWO THRUST VECTOR CONTROL SYSTEMS
AFT SEPARATION MOTOR MODULE
THREE CASE STIFFENER RINGS
TWO GIMBAL ACTUATORS
NOZZLE EXTENSION
TWO THRUST VECTOR CONTROL SYSTEMS (TVC)
FOUR BOOSTER HOLDDOWN POSTS
+Z
+Y
FLT
+X
−Y
−Z
SRB-SP-070B
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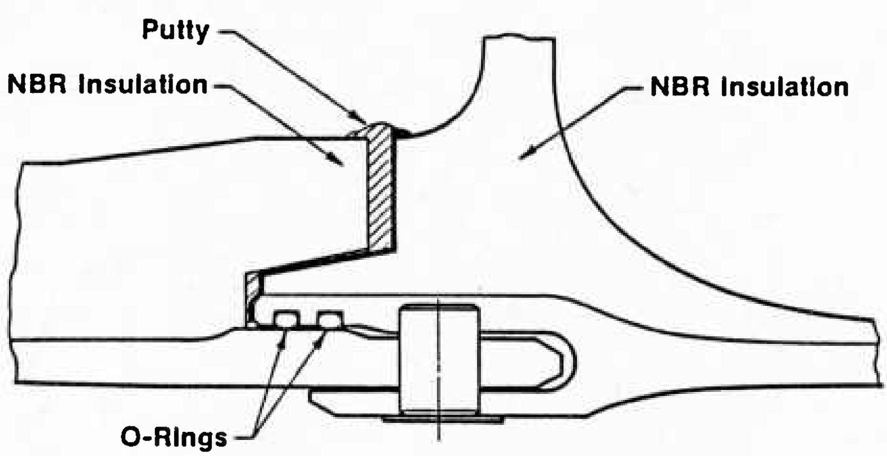
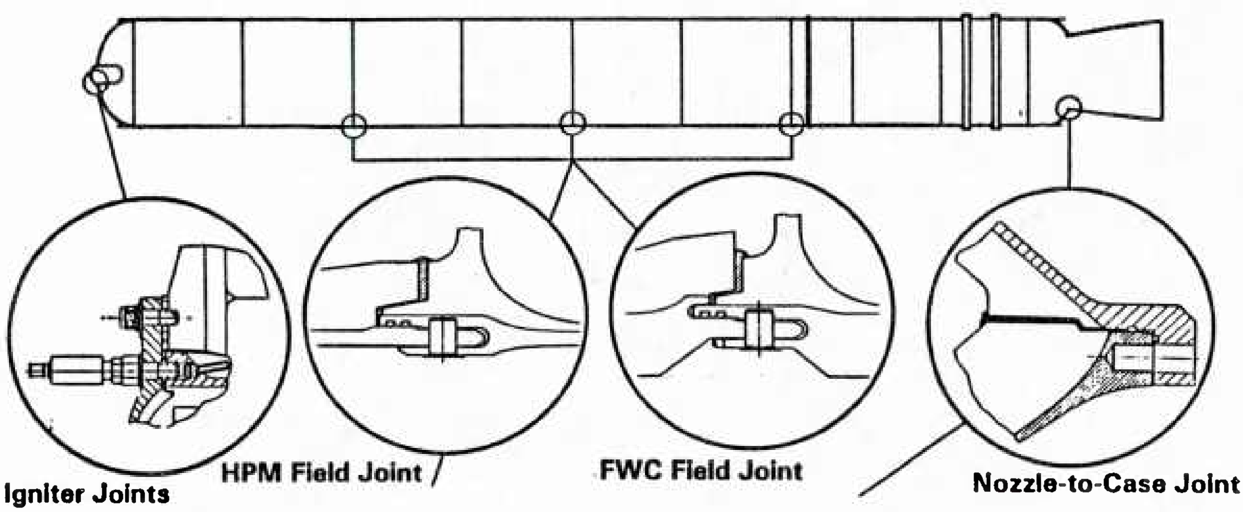
[Ref. 2/11-6]

SRM-HPM FIELD JOINT



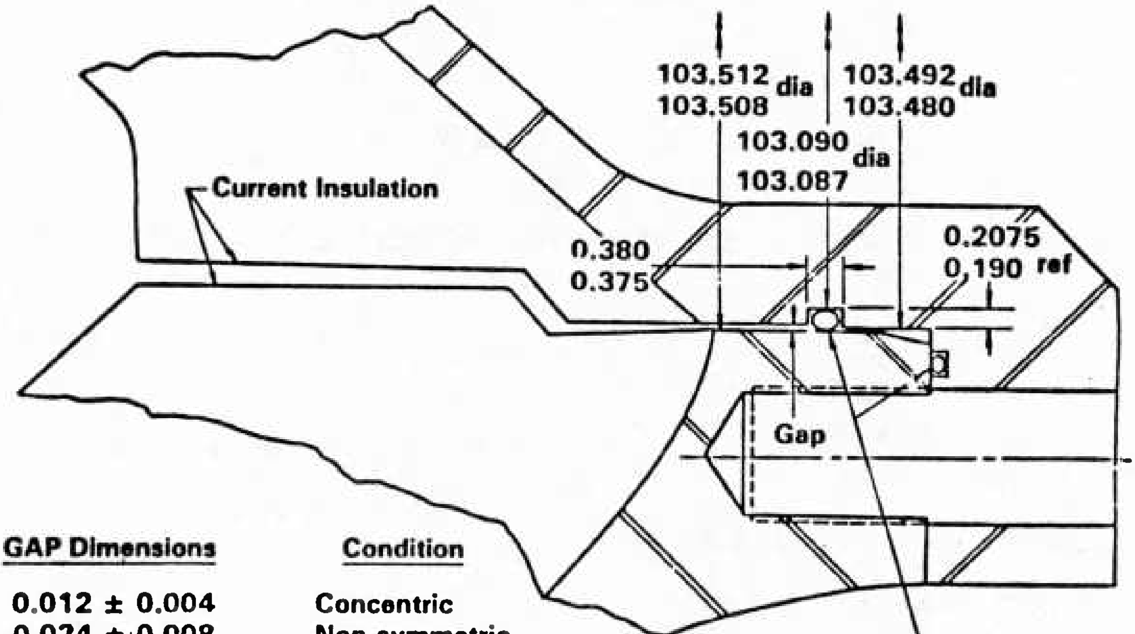
[Ref. 2/11-7]

Space Shuttle SRM Joints



[Ref. 2/11-8]

Space Shuttle SRM Segment Joint



GAP Dimensions	Condition
0.012 ± 0.004	Concentric
0.024 ± 0.008	Non-symmetric

- GAP opens additional 0.015 in. at 230 ms into ignition transient

[Ref. 2/11-9]

STS 51-L SRB FLIGHT READINESS CONSIDERATIONS

- IN PREPARATION FOR ANY FLIGHT, A SERIES OF REVIEWS ARE HELD. AGENDA INCLUDES:
  - PREVIOUS FLIGHT PERFORMANCE
  - ANY PROBLEMS FROM PREVIOUS FLIGHT
  - ITEMS OF CONTINUING ANALYSIS TO ASSURE NO CHANGE IN PREVIOUS FLIGHT READINESS RATIONALE
  - UPCOMING FLIGHT PERFORMANCE ASSESSMENT
  - CERTIFICATION/VERIFICATION STATUS

[Ref. 2/11-10]

STS 51-L SRB FLIGHT READINESS CONSIDERATIONS

- LEVELS OF REVIEW
  - PRIME CONTRACTOR (CHAIRED BY SENIOR VP)
  - SRM AND BOOSTER ASSEMBLY PROJECTS (CHAIRED BY PROJECT MANAGER)
  - SRB PROJECT (CHAIRED BY PROJECT MANAGER)
  - SHUTTLE PROJECT (CHAIRED BY PROJECT MANAGER)
  - MSFC CENTER BOARD (CHAIRED BY CENTER DIRECTOR)
  - LEVEL II BOARD (CHAIRED BY NSTS MANAGER)
  - LEVEL I BOARD (CHAIRED BY ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT)
- ONE DAY BEFORE LAUNCH, LEVEL I HAS A REVIEW TO ASSURE LAUNCH READINESS
- NO CONCERNS REGARDING SRM JOINT O-RING EROSION WERE EXPRESSED DURING THE STS 51-L FRR PROCESS

[Ref. 2/11-11]

O-RING HISTORY

- PRIOR TO STS 51-L . . . . .11 STATIC TEST MOTORS
  48 FLIGHT MOTORS
- FIELD AND NOZZLE JOINTS OF 57 MOTORS HAVE BEEN EXAMINED REPRESENTING 228 JOINTS WITH 456 O-RINGS
- OBSERVATIONS
  - 6 OF THE 171 FIELD JOINTS EXHIBITED SOME EROSION OF THE PRIMARY O-RING
  - 2 OF THE 171 FIELD JOINTS EXHIBITED SOME SOOT BEHIND THE PRIMARY O-RING
  - 16 OF THE 57 NOZZLE JOINTS EXHIBITED SOME EROSION OF THE PRIMARY O-RING
  - 8 OF THE 57 NOZZLE JOINTS EXHIBITED SOME SOOT BEHIND THE PRIMARY O-RING
  - NO FIELD-JOINT SECONDARY O-RINGS HAVE BEEN ERODED
  - 1 NOZZLE-JOINT SECONDARY O-RING HAS BEEN ERODED (ONLY 1/4 OF THE ESTIMATED SAFE DEPTH)
- CONCLUSIONS
  - IN ALL OF THE FIELD JOINTS EXAMINED, THE PRIMARY O-RING SEALED EFFECTIVELY EVEN WITH EROSION
  - IN ONE NOZZLE JOINT, THE PRIMARY O-RING FAILED TO SEAL, BUT THE SECONDARY O-RING SEALED EFFECTIVELY EVEN WITH EROSION

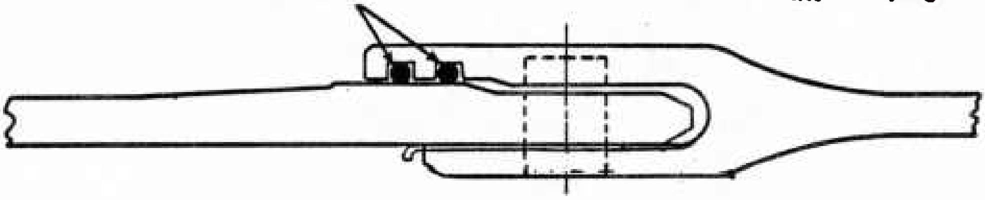
[Ref. 2/11-12]

SRM FIELD JOINT

Segment Canterline

0.020" Min. O-Ring Squeeze

P_{int} = 0 psig

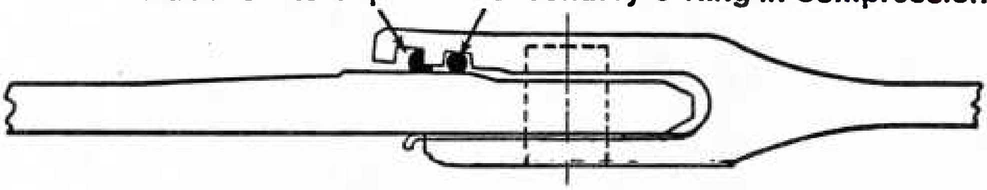


Unpressurized Joint - No Rotation

Segment Canterline

Primary O-Ring Extrudes Into Gap

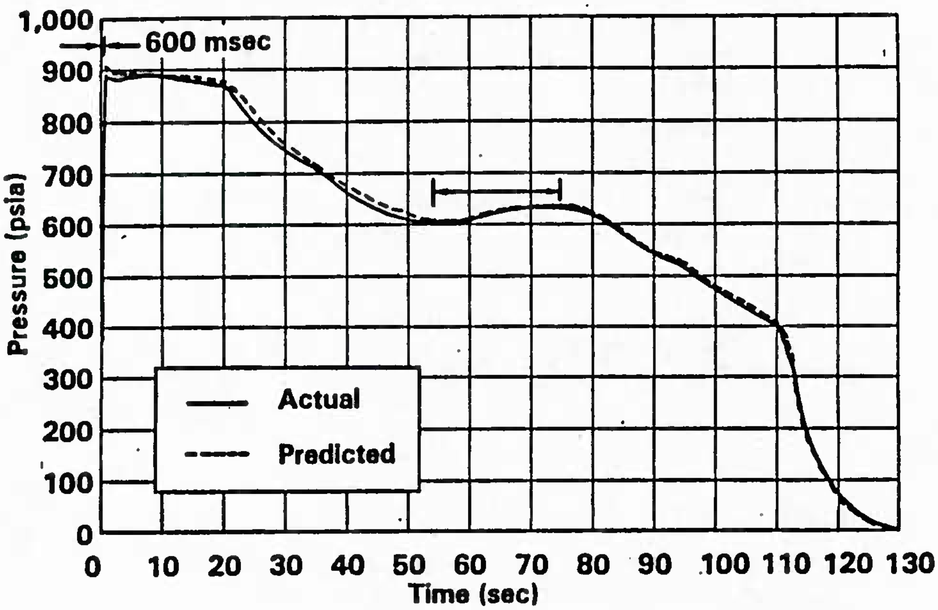
Secondary O-Ring In Compression



Normal Pressurized Joint (Unaffected Joint)
(Joint Has Rotated)
Observed On 163 of 171 Field Joints (95.3%)

[Ref. 2/11-13]

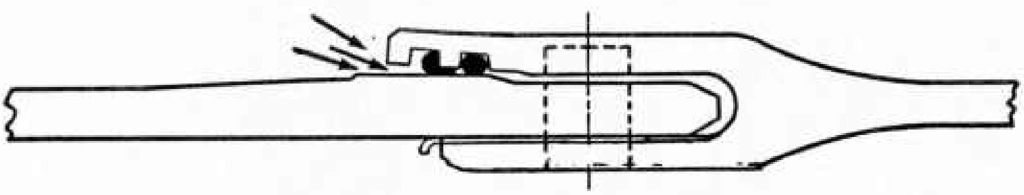
SRM-15A Headend Pressure (Actual Versus Predicted) at 60°F–1 Sample Per Second Data



[Ref. 2/11-14]

PRIMARY O-RING EROSION

- MOTOR PRESSURE RISES FROM 0 TO 200 PSIG
- CONCENTRATED HOT GAS JET THROUGH PUTTY IMPINGES ON PRIMARY O-RING AND ERODES IT WHILE THE O-RING IS BEING SEATED. (NO BLOW-BY)
- PRIMARY O-RING SEATS AND BEGINS TO EXTRUDE INTO GAP AND EROSION CONTINUES
- PRESSURE RISES TO OPERATING PRESSURE AND PRIMARY O-RING REMAINS FULLY SEALED - EROSION ON PRIMARY O-RING STOPS

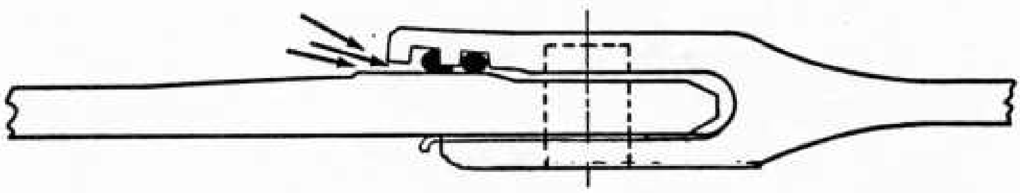


- OBSERVED ON 6 OF 171 JOINTS (3.5%)
- 2 OF THE 6 EXHIBITED SOOT PAST PRIMARY O-RING WITH NO DAMAGE TO SECONDARY O-RING (1.2%)

[Ref. 2/11-15]

SOOT BLOW-BY PRIMARY O-RING WITHOUT EROSION

- MOTOR PRESSURE RISES FROM 0-50 PSI
- PRIMARY O-RING DOES NOT SEAL (BLOW-BY)
- COMBUSTION PRODUCTS OF PUTTY AND GREASE BLOW-BY PRIMARY SEAL BEFORE PRIMARY O-RING EXTRUDES INTO GAP - DEPOSITS SOOT BETWEEN THE TWO O-RINGS
- PRIMARY O-RING EXTRUDES INTO GAP AND SEALS WITH NO EROSION TO PRIMARY OR SECONDARY O-RING

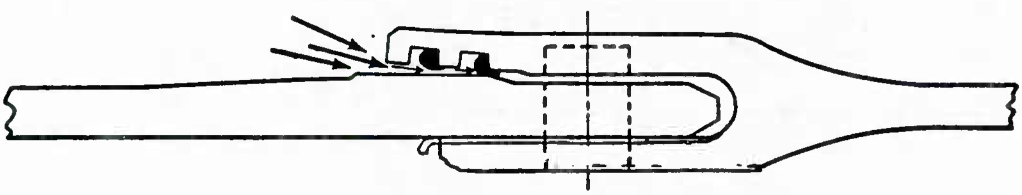


- OBSERVED ON 2 OF 171 JOINTS EXAMINED (1.2%)

[Ref. 2/11-16]

PRIMARY O-RING EROSION WITH BLOW-BY AT IGNITION

- MOTOR PRESSURE RISES FROM 0 TO 200 PSI
- PRIMARY O-RING DOES NOT SEAL (BLOW-BY)
- SECONDARY SEAL IS ENERGIZED AND SEALS
- PRESSURE RISES TO OPERATING PRESSURE
- DURING BLOW-BY OF PRIMARY SEAL, PRIMARY EROSION OCCURS AND EROSION OF SECONDARY O-RING OCCURS BUT DOES NOT COMPROMISE INTEGRITY OF SEAL

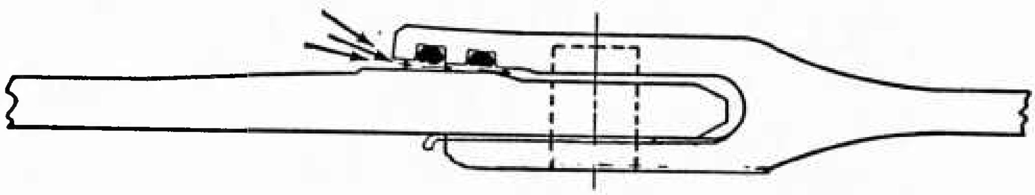


- NEVER OBSERVED ON ANY OF 171 JOINTS EXAMINED

[Ref. 2/11-17]

BLOW-BY PRIMARY O-RING CONTINUES PAST 200 PSI

- MOTOR PRESSURE RISES FROM 0 TO 200 PSI
- PRIMARY O-RING DOES NOT SEAL
- JOINT ROTATES AND SECONDARY O-RING SQUEEZES IS LOST WITH WORST CASE JOINT TOLERANCES (200-1004 PSI)
- SECONDARY O-RING DOES NOT SEAL
- BLOW-BY CAUSES PRIMARY AND SECONDARY SEAL FAILURE



- NEVER OBSERVED ON ANY OF 171 JOINTS EXAMINED

[Ref. 2/11-18]

RATIONALE FOR ACCEPTING OBSERVED O-RING EROSION

- JET IMPINGMENT EROSION HAS BEEN SHOWN BY TEST AND ANALYSIS TO BE WITHIN ACCEPTABLE LIMITS
  - ANALYSIS
    - o GAS FLOW
    - o HEAT TRANSFER
    - o STRUCTURAL
  - TEST
    - o FULL SCALE MOTORS
    - o SUBSCALE MOTORS
    - o TEST OF O-RINGS AT 3000 PSI (3 TIMES MAXIMUM OPERATING PRESSURE) WITH 2 TIMES OBSERVED EROSION
- DETAILED POSTFLIGHT INSPECTION, ANALYSIS OF ANOMALIES, AND RIGOROUS REVIEW PROCESS ASSURES UNDERSTANDING OF OBSERVATIONS

[Ref. 2/11-19]

SUMMARY

- HAVE DISCUSSED JOINT SEAL BECAUSE OF EXPRESSED INTEREST.
- DATA AND DESIGN ANALYSIS TASK FORCE HAS NOT CONCLUDED THAT THE FAILURE OF 51-L WAS CAUSED BY A JOINT ANOMALY.
- HOWEVER, JOINT HAS NOT BEEN EXCLUDED AS A FACTOR IN THE ACCIDENT.
- FILM SHOWS FLAME COMING FROM SRM.
- **HAVE NOT YET DETERMINED IF THIS IS CAUSE OR EFFECT.**

[Ref. 2/11-20]

TESTIMONY OF RICHARD C. COOK

CHAIRMAN ROGERS: Mr. Cook, the Commission asked you to appear today because of recent stories concerning particularly a memorandum which you wrote on the 23rd of July, 1985, and we will let you make whatever comments you would like to make on that memorandum. And we will of course make the memorandum available to the press. [Ref. 2/11-21]

You told me before the meeting was reconvened that you would like to make some preliminary comments, and of course you may go right ahead and say anything you would like to.

MR. COOK: Thank you, sir.

All I wanted to do really was just give a little bit of background about my own background, what I do at NASA, and set kind of a context for this particular memo that we have here. By background, I have been a program policy analyst for the federal government.

I started working for the government in 1970. I worked at the Civil Service Commission and the Food and Drug Administration. I worked at the White House Consumer Affairs Council for both the Carter and Reagan Administrations.

Then I went out of the government. I got my

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introduction to this kind of high tech hardware when I was working on a project at TRW, on a defense intelligence hardware project, immediately before I came to NASA.

And I was brought to NASA as the resource analyst in the Comptroller's Office, and it is divided up by hardware and the hardware I was given to work on was the external tank, the solid rocket boosters, and the Centaur upper stage, which is now part of the shuttle configuration.

The memorandum in question I wrote very soon after my arrival at NASA. It was one of the first assignments that I was given, to do a little background on this. And the reason that we do things like this is because we have to prepare the budget for NASA. We have to cost out what things are going to require, particularly if we have engineering questions that come up that are going to require some kind of additional funding or some kind of change in the funding profile, to be able to pay for it.

So for this reason, we have to keep pretty much in touch with the project people in the Office of Space Flight, and we also go on field trips down to Marshall or Kennedy or other places, to try to keep as informed as we can. And then when issues arise that

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look like they might be budget threats, we have got to report back on it and try to come up with some kind of estimate with the program office of what it's going to cost to repair this type of thing.

When I first got the assignment, now, I did go over it and I did talk to the people over there, the engineers over in the Office of Space Flight, who are an extremely cooperative group of people and very helpful in this type of thing.

And it became apparent to me that there were some real concerns with the O-ring problem at that time, concerns from an engineering standpoint which, as I understood it from what I discussed with people had flight safety implications and potentially major budgetary concerns, because with something like this, as I understood it, if you fix something like this you've got quite a range of cost implications.

If it is something simple, you might be able to absorb it in your budget. If it is a major redesign or re-engineering, where you've got to retrofit or you've got to go out to the contractor and have him cast new SRB segments or something like that, you might have a major budgetary hit and you have got to come up with the money.

And at that time the SRB budget on

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development, the development side was pretty much winding up. SRB development was coming to a close. We had a few things that were still in the budget and had to be covered, but as far as major budgetary concerns, if something came along we would have to think real hard, work with the Office of Space Flight on figuring out how to cover something like that.

And we felt that the O-ring problem—and I think it was our impression from the Office of Space Flight—was a potentially major budget hit. When we went through the monthly briefings over at the Office of Space Flight, the O-ring problem was one of the—there was a list of budget threats that was printed each month.

Every month the O-ring problem or O-ring charring problem was on that list. Sometimes it was first on the list, sometimes it wasn't. But I don't think that necessarily reflected priority. But there was no question that the O-ring problem was considered a potential budget threat month after month from the summer and on into the fall.

And I understand it was, even when it went up to the Administrator in August with the annual budget review to the Administrator, it was also listed on that presentation as a budget threat.

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I reported back to my management monthly on the assessment I was getting from across the street on what the O-ring situation was. And I know, I'm not an engineer. I can't talk in engineering detail. I think I understand the basics of the joint configuration and all of that stuff, although I certainly couldn't comment on an engineer's analysis of it. But there were some things that were being factored into my analyses that I think have some bearing on this.

For instance, it was mentioned that an effort was going to be made to keep the secondary O-ring from unseating in flight or at least from breaking the seal in flight. Now, the secondary O-ring, as I understand it, we went back a couple, three years, when they first started to discover that this rotation, this joint rotation, was unseating the secondary O-ring, and that was reducing or eliminating in some cases the redundancy feature on that joint.

And so there was a lot of concern about how we could get redundancy back in that joint without having to throw away half a million dollar SRB segments and starting all over again with redesigning and recasting them. There was a 13 month lead time if you wanted to order a new segment from the manufacturer, and so if you had to throw this stuff out you had a problem.

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And so we were looking for ways to get the thing taken care of in a reasonable manner. And as I understand it, the capture feature was going to be on the QM-5 firing, which had been scheduled this week, but I think has been postponed. And it was my understanding—and this is just what I gather; I have it in my documentation, I think, somewhere, but you would have to check with the program people. As I understand it, if the O-ring—the capture feature got through the review, the QM–5 firing, and whatever other reviews Marshall and the Office of Space Flight were going to do, we were going to try to get that on the booster segments around January of 1987.

But again, that was my understanding, and that would be something that you would have to check on.

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There was another issue that came up on the leak test port. It was my understanding that the leak test port had been mentioned as a problem back when the redundancy requirement was reviewed for the unseating of the secondary O-ring, because at that time, as I understand it, there was no good test for checking the pressure on it once you plugged that thing back in down at Kennedy.

Now that may be something you just have to check. I cannot vouch for that talking to anybody recently. It was just another related problem they were trying to solve, the redundancy issue on that field joint.

Now I do have one question, and I'm certainly not competent at all to comment on Mr. Mulloy's presentation this morning. I won't even attempt to do that. But from my own perspective as the guy who is supposed to be watching this issue for the Comptroller's office, it was my understanding that there was at least some erosion going on in 1985 in the O-rings.

I understand, I have been told there is a Thiokol document from August 19 that documents it. I know I have seen in my own files a report from Code M indicating some erosion on a flight, I believe in August. Now the document I believe I'm referring to is

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in September, and I would have to go back and check that.

But I would urge the Commission to work from—if you want to analyze my report, the thing to work from is probably the monthly reports of the Propulsion Division to the Administrator made every month, where it lists the propulsion issues and it lists sometimes the specific findings from the various flights. To me that was a very helpful document in kind of keeping track of what was going on.

And since I'm working from essentially secondary documents—you know, I was never in on the primary observation of this stuff or the analysis—to really analyze my document I would say you should go to the Propulsion Division, look at their documentation and their engineering staff.

But I must say that I take full responsibility for what I said in my memo of last summer. I realize that what I was saying in there was of some concern. I felt it my obligation to report to my management, and I understand my reports went all the way up to Mr. Newman, as they should have, and I felt I was being as fair as I could to present a balanced view based upon what I had been told.

So that is just kind of my introduction. Now

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I would be glad to answer any questions about it.

CHAIRMAN ROGERS: Thank you, Mr. Cook.

I would like to ask a few questions. First, your memorandum was sent to Mr. Mann. He was your superior?

MR. COOK: Yes, sir.

CHAIRMAN ROGERS: And your focus of attention was primarily budgetary; is that correct?

MR. COOK: Yes, sir.

CHAIRMAN ROGERS: And, to summarize it, you were, I gather, thinking about whether if changes were required for safety reasons or any other reason you had to think about how much it would cost?

MR. COOK: Yes.

CHAIRMAN ROGERS: And therefore your questioning of people in NASA was in connection with that budgetary matter?

MR. COOK: Exactly.

CHAIRMAN ROGERS: You didn't, I assume, make any attempt to weigh budgetary considerations and safety considerations, did you?

MR. COOK: Not at all.

CHAIRMAN ROGERS: You weren't qualified for that?

MR. COOK: No, sir.

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CHAIRMAN ROGERS: And you assumed other people were doing that?

MR. COOK: Yes.

CHAIRMAN ROGERS: You had no reason to think the other people were not qualified to do that, or you had no reason to think that people who were weighing those considerations were not qualified to do it?

MR. COOK: I had no reason, except I would have to qualify that not from the standpoint of criticizing anybody, but it relates to something that I said earlier—and I know that I have the

highest regard for the professionalism in NASA; I worked in several agencies, and it is the most professional agency I have worked for—as far as the depth into which these things are analyzed.

And so I wouldn't want to reflect at all on that, and particularly in the Comptroller's office. But going back to what I said earlier, we had a developmental budget for the SRBs that was coming to an end. We have a budget, a developmental budget, that is divided into three parts. We have filament wound case, which is, as you know, is the substitute lightweight solid rocket booster that is being developed right now.

We have tooling, which is mainly based upon an effort to get the tools in place at the factories, so

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that when the flight rate got up to 24 per year we could support that flight rate.

And the third thing is residual development, and that was special studies that came up and other things. I'm sure others could explain it more fully than I could. But it was special studies that came up, either anomalies or improvements. I think we had a nozzle erosion study going on that was funded under residual development.

Plus, we were trying to figure out how we could qualify a second source to build the SRBs. That was in residual development. But that budget was coming down very steeply, and, at the same time, the flight rate was going up very rapidly. And, to me, that creates a very difficult situation where a lot of judgment is required to figure out what you're going to spend your developmental money on.

I had no reason at all to question anybody's—the way that was weighed. All I am saying is that it puts the Agency, it put me, having to analyze it and make recommendations, in a spot where you just had to use real good judgment to say what was needed and what you were going to do if a surprise came up and you had to come up with a whole lot of money to cover one of these things.

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CHAIRMAN ROGERS: But still what you've just said relates to the budgetary considerations. My question was did you have any reason not to rely on the recommendations of the people who were highly qualified to make recommendations insofar as safety was concerned?

MR. COOK: No, sir.

CHAIRMAN ROGERS: So you don't now and you never have said that you distrusted or were unable to rely on those people who were primarily responsible for safety features?

MR. COOK: Not at all, no. I didn't.

CHAIRMAN ROGERS: And so you felt that to perform your job in terms of the Comptroller's office you were anxious to find out, if you could, what plans were being made that might impact on the budget?

MR. COOK: That's right.

CHAIRMAN ROGERS: And you didn't feel that you were in a position or should you make those decisions about what should be done with the space program?

MR. COOK: That's right.

CHAIRMAN ROGERS: And so that the memo, which has been given a great deal of attention, sort of suggests that you were taking issue with the people who

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were highly qualified to make those judgments, when in fact you weren't at all. You were looking to see how much it might cost if certain changes had to be made; is that right?

MR. COOK: That is right. In fact, I have made a point on the second page here. "It should be pointed out that Code M management is viewing the situation with the utmost seriousness." And I had no reason to doubt that, and I didn't as time went on. In fact, in the fall the reports I was getting, because every time I had to make my report I would ask the program people about the situation and about the O-rings, what was happening with the O-rings, and in fact we were seeing, as I understood it, there was erosion, according to the records I was given in the summer of 1985.

But, as I understood it, what was reported to me was there was a flight in the fall of 1985 where there was no erosion at all, and I reported that back to my management. In fact, I was reporting at that time that it looked to be as though the fix for the O-rings might be less serious than was earlier indicated. At the same time I reported or at least I was told—I have to look at my own files, my own documentation—that because they weren't exactly sure of what was causing the

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erosion, for instance, the putty was an issue and it was mentioned, and we had somewhere around a $50 million estimate to requalify new putty if we had to do that.

In fact, I think we were going to have to do that anyway because the asbestos was going to have to be gotten rid of. And so there was that. There was the unseating of the secondary O-ring that the capture feature was supposed to take care of.

So we still weren't sure, but at the time I was reporting that it looked as though we might not have a major budgetary hit during fiscal year 1986 from the O-rings. They were also, as I understand it, combining the test of the capture feature with another test to save money.

CHAIRMAN ROGERS: I think the Commission and I certainly understand what you are saying. And you were reporting back to the Comptroller's office, and to perform your job on budgetary considerations, and you were picking up information from different sources as to what they might be thinking about. You were not passing judgment, though, yourself on what they should or should not be doing, were you?

MR. COOK: Not at all. In fact, I was new to the program and I felt I was hearing things that I expected. I wasn't the only one that was hearing in the

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Comptroller's office. I had no reason to believe that. But I felt for my own education and for my own professional judgment that I would write it up as I heard it, and that is what I did. I was not passing judgment.

CHAIRMAN ROGERS: Since the time of the accident—well, let me withdraw that.

Well, since the accident occurred have you had discussions with people about your memorandum of July 23, 1985? [Ref. 2/11-21]

MR. COOK: Yes, particularly since it showed up in the newspapers.

CHAIRMAN ROGERS: Did you have discussions with people before it showed up in the newspaper?

MR. COOK: I had given it to my boss just as a matter of giving him documentation.

CHAIRMAN ROGERS: But no one else?

MR. COOK: Someone else? Well, my boss and the other, the former SRB analyst that I worked with very closely.

CHAIRMAN ROGERS: Anyone out of the office?

MR. COOK: No.

CHAIRMAN ROGERS: And so you were not involved in the publication of the document yourself?

MR. COOK: No, I was not.

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CHAIRMAN ROGERS: Since the accident have you written another memorandum in connection with the accident?

MR. COOK: Yes, I did.

CHAIRMAN ROGERS: What prompted that?

MR. COOK: The heat of the moment. I did write another memorandum and what I was saying essentially was that, given all of the information that was coming in, I felt at that time that, number one, it had not been demonstrated what caused the accident. The evidence that points to the SRB is either circumstantial or interpretations of photographs.

But I felt that the problems that I had been apprised of during the course of, now it wasn't just the first weeks on the job, it was several months, ought to be looked at seriously enough so that whatever happened this needed to be taken care of. The O-ring problem needed to be taken care of before we could look at the shuttle program as being completely resolved.

And we had some other major budgetary issues that came up in that connection.

CHAIRMAN ROGERS: You still were doing it in terms of the budget? I mean, was that the purpose of writing the memorandum after the accident?

MR. COOK: Yes.

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CHAIRMAN ROGERS: Did you have reason to think that efforts would not be made after the accident to investigate it thoroughly?

MR. COOK: No. In fact, I knew that across the street they were doing the same analyses we were doing.

CHAIRMAN ROGERS: Then will you explain again why you wrote the memorandum?

MR. COOK: To document what I felt were all of the budgetary implications of the situation.

CHAIRMAN ROGERS: Only budgetary? Was there any other purpose? Well, if you'd rather not answer, that's all right. I'm just curious about why you wrote the memorandum. I mean, it doesn't sound as if you had budgetary considerations in mind. It sounds differently. But I just wanted you to have an opportunity to tell the Commission why you wrote it.

MR. COOK: I wrote it because I felt that it was a serious enough situation that I didn't think that until these various issues were resolved with the SRBs that I had been involved with, that they had to be taken care of before the shuttle could safely continue.

CHAIRMAN ROGERS: Did you think your engineering experience, based on the short time you had been with NASA, improved your ability to pass

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judgment on what others had decided? Well, here again I really don't want to press you.

Do you have anything else to tell the Commission?

(No response.)

CHAIRMAN ROGERS: Any other questions?

DR. WALKER: When you referred to "across the street" did you mean Code M?

MR. COOK: Yes, sir.

MR. HOTZ: As far as you know, in the preparation of the fiscal 1987 NASA budget were there any provisions made for an O-ring or a seal improvement program?

MR. COOK: For fiscal year 1987?

MR. HOTZ: Yes, the current budget.

MR. COOK: Not to my knowledge. Now there was to be, as I said, a firing of the QM-5 test article this week, which is a budgetary item, but to my knowledge there was not a separate budgetary proposal going into the 1987 budget at that time for this problem.

MR. HOTZ: But in your last paragraph here you expressed an opinion or a recommendation that there should be some sort of a provision in the fiscal 1987 budget.

MR. COOK: Yes, sir. I thought there should

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be.

MR. HOTZ: And to your knowledge there was none?

MR. COOK: Well, but this is just beginning, really, the thinking about what—to my knowledge, NASA has not yet put together a complete—well, how could it—analysis of what will be needed to remedy any requirements that grow out of the investigation of the accident.

MR. HOTZ: Yes, we understand that. We're talking about their judgment before the accident, just as an ongoing program improvement.

MR. COOK: There was no separate item that I know of now, at the level I was dealing with, the composite numbers that I was dealing with. I'm sure there were studies and analyses that were part of a total project support number, for instance, that dealt with it, but there was nothing on the order of, say, the nozzle erosion study, which was a $3 million or $4 million study.

MR. HOTZ: Thank you.

CHAIRMAN ROGERS: Mr. Cook, I just received a copy of the memorandum that I referred to previously, which is dated February 3, 1986, and the other Commission members have not had a chance to look at it. [Ref. 2/11-22]

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I would propose to let them take a look at it during the lunch hour and then we can resume and they may have some questions to ask and you may have some comments to make.

Okay. Let's have a recess for lunch.

(Whereupon, at 12:50 o'clock p.m., the Commission recessed, to reconvene at 2:30 o'clock p.m. the same day.)

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AFTERNOON SESSION

(2:25 p.m.)

TESTIMONY OF RICHARD C. COOK—(Resumed)

CHAIRMAN ROGERS: Mr. Cook, is that chair convenient for you or would you rather have a different kind of chair?

MR. COOK: I am much more comfortable just to stand up.

CHAIRMAN ROGERS: Well, let me begin by saying that I had a chance to talk a little bit with Mr. Cook during lunch and learned that he had in fact been asked to prepare a memorandum subsequent to the accident on budgetary matters, and Mr. Cook pointed out, and we discussed the fact that in the initiation of the memo he included some material in there that did not strictly come within the budgetary request that was made.

Nonetheless he said in his testimony this morning that he did it in the heat of the moment, and I thought maybe, Mr. Cook, you would want to follow up on the discussions we had about

how you relate to the engineers in NASA and the others in terms of their ability and their qualifications.

MR. COOK: Kind of to paraphrase what we covered this morning, it is a requirement of my job and of everybody in the office, each of which has its own

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hardware, to handle, to try to understand as much as possible about the engineering side of things as we can. There are plenty of guys there who do it a lot better than I do, and even some of the more complicated parts of the program, such as the space shuttle main engines, and we rely on the program office in the Office of Space Flight and we rely on the conversations we have with the Centers, with Marshall and Kennedy and other places, and we try to come as best as we can to an expression of what the engineering issues appear to be, because we want to give accurate cost estimates when they go up to the Congress.

And the kind of support we get from the program office is, by and large, superb. I mean, the professionalism and the cooperation that we get from these people is excellent. And we have kind of a middle man role of trying to translate these things back into language that can be used for the cost and price analysis and also to give our budget presentations to the Congress.

One of the things that, immediately after the accident—and it is still going on—is that our office was required to come up with some very fast estimates of what the implications were of some of the possibilities that were being talked about at that

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time. And, of course, the external tank and the boosters fell into my area for analysis.

And so essentially what I did was to try to pull together all of the possible budget implications that I could think of. Again, I spent a lot of time across the street, a lot of time talking to people, trying to put into my own words what I felt would be needed for NASA to address the SRB question, if it happened to be an SRB problem or perhaps even if it didn't, because, even if it wasn't an SRB problem in the accident there were still things that we had been concerned about that had been well documented during the previous year, including the putty, including the O-rings, that we wanted to take a look at.

And I felt—and I think the people across the street felt also—that this was a particularly crucial time to kind of lay it all out, all of the problems we saw, all of the things that would need to be examined and really understand if we were talking about a four-month delay, an eight-month delay, a year delay or exactly how long it was going to take.

And right now I believe we are running those options in more detail—different length of delays depending on in large measure the outcome of what you folks are deliberating.

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I don't know if it would be useful to run down the issues that I had included. I don't think there is anything new in there that has not either been before the Commission or shortly will be. But I would be glad to do that.

CHAIRMAN ROGERS: Well, why don't you do that, Mr. Cook? I think that—you are speaking now about the memorandum subsequent to the accident.

MR. COOK: Yes, sir.

CHAIRMAN ROGERS: Our plan is to, because we don't have a machine we can do it here, to ask NASA to release this memorandum that we're talking about a little later on when we finish today. But I do think it might be useful to have you summarize what is contained in the memo and any comments you care to make about it.

MR. COOK: Let me just briefly run through those things, then. There are just a few items. One is that the capture feature that was to be tested on the QM-5, I said, is something that has not yet been accepted for use on the steel SRB cases, so if this is going to be a solution to the unseating problem, then that would be something that during the next few months would have to be tested and qualified.

The second thing was it is clear that the field joint putty plays a significant role in O-ring

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erosion. It is also an asbestos problem. And so flight qualification of new putty is recognized to be a major unbudgeted cost item, meaning up until that time, the time of the incident, it had not yet been costed out and put into the budget we were submitting to Congress.

Any effects of environmental and weather factors on the putty and O-rings may have design implications which require further investigation. I noted that eight SRB segment sets are in manufacturing flow at that moment, and that meant simply that from the time Thiokol pulls them off the shelf and starts loading them up with propellant until they get in to the Cape to be assembled for flight we have eight SRB flight sets that are moving through that process. And depending on any action that we have to take to retrofit those cases with either a capture feature or any other new design feature, something has to be done with those.

We have either got to scrape that stuff out, that propellant out, or they have got to static fire it, or, I suppose, conceivably think about doing a retrofit with the case segments in place. Now again what I am doing is reporting on the resource implications. I have no idea what the detailed engineering implications are here.

Again, what we're trying to do is come up with

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a cost estimate of what it's going to take to retrofit case segments. Five flight sets of hardware remain on the shelf at Thiokol which should not be loaded until the O-rings are repaired, and so there is a possibility of a shutdown or a slowdown at Thiokol while they wait to see what can be done with those flight sets.

I noted here the problem with the leak test ports, because this is not something that I had put in any previous analysis that I had given to the Comptroller's office, that something needs to be looked at as far as those leak test ports, and if there is anything that needs to be done in regard to assuring that those are put in place properly or tested properly.

And another question had come up at some point whether we had any case cracking or case rupture, and I simply made a note that the procedures for checking case cracks, I think there was a test at Thiokol where there was a burst of a case because of a crack in a stiffener ring bolt that raised some question about whether we have got to take another look at that. And that relates to our budgetary issue because we have been watching very carefully the attrition rate for solid rocket motor hardware.

We have got in our budget an assumption that somewhere around five percent of all of our flown case

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segments will be lost to use either because they are damaged when they hit the water after the SRB comes down from flight. We lost a couple of flight sets when they were sunk. We lost some in an accident out at Thiokol, and we have got some segments under repair.

So we are trying to come up with the best estimate we can of how many flight segments over the lifetime of the 20 per use configuration have to be replaced by new hardware. We have been working a 5.1 percent projection. It has actually been higher than that. And so we're won-

dering at this point whether the possibility of a crack occurring might lead us to have either a higher attrition rate or additional expenditure for detecting these.

Another issue was this thing that has been debated in the news—and again this is just something that came up in this context as a potential cost implication—as to whether anything was going to have to be assessed to see if instrumentation, software or launch and training procedures would be implicated, if the agency thought there was anything we could implement to have an abort mode during SRB ignition.

Now, as I understand it, particularly from Mr. Aldrich's testimony a few days ago, at this time I'm not aware of any possible abort mode that is being

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discussed—and again that is something that would have to come from the engineers—but I cited this in here as something that from a budgetary standpoint we might have to take into account if the agency made a decision to install an abort mode during SRB firing.

The next item is that if the capture feature cannot be qualified for the steel segments and we have to reengineer the field joints, then there are three SRB segments for each booster, six per flight set, that one way or another will have to be reengineered.

And if that is the case, the lead time to tell the manufacturer to make a new segment that we have been working with is about 13 months, and so that has serious budgetary implications if we have got to remanufacture case segments and we're looking at that kind of lead time. We either have to have a crash program to manufacture them or we have a delay of that length of time. So that was kind of a crucial variable and that is another reason why we needed to cost out the test and cost out the capture feature as soon as we could.

The other thing is, I guess Number 9 is the attrition rate on the SRB segments which I just mentioned has been higher than the planning projections, and we just lost 22 more segments because of the accident. That throws us behind considerably in our

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manufacturing flow because we're only producing, I believe, one of these segments a month, or maybe two a month. And so now we have to take a look at that, and even if we suspend flights for a period of months we're going to have to come up with a program where we can make up for those lost segments and get them back into the flow.

Again, that is—that depends on the capture feature or any other fix that is made to the SRBs to accommodate whatever the results of the investigation is or, as I guess some people felt ought to be done anyway and was going to be done anyway—I mean even before the incident—the capture feature was to be tested. It was to be tested this week and installed. So the presumption is that that was going to happen and would be going on anyway.

The other item is that the filament-wound case project needs to be examined because it, too, had safety issues. There was a test where under pressure the case showed a rupture. The case was to be retested and there was a difference of opinion as to what the likelihood was going to be of the case passing muster as far as its overall flight safety posture was concerned.

And that too had to be factored in because our ability to meet the Vandenberg launch schedule after

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July depended very much on having those filament-wound cases and the filament-wound cases were also a part of the third production contract that was about to be negotiated between the agency and Morton-Thiokol.

So based on all of those considerations, if we went through with this program I was projecting—and again this is just a budgetary analyst's estimate—I was projecting a nine-month or longer suspension of flights to deal with all of these things.

CHAIRMAN ROGERS: Thank you very much, Mr. Cook. I appreciate that.

Summarizing your first memo, of July 23, 1985, as you have said several times, it was done for budgetary considerations and you did not intend to pass judgment on any of the engineering features as such. You were looking at it for budgetary purposes?

MR. COOK: Absolutely. That was all I was, that was all I was able to do.

CHAIRMAN ROGERS: And you were satisfied with the performance and ability of the engineers that you dealt with at NASA?

MR. COOK: Yes, and I would say particularly the fellows in the Propulsion Division at headquarters. I thought it was an extremely professional group. It was a great deal of help to me particularly getting on

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board and getting up to speed on all of these things. And I thought they were an excellent team that had the interest of NASA and the program entirely in mind, and I had complete confidence in the information that I was receiving.

CHAIRMAN ROGERS: Thank you very much.

Now I asked you this morning about the reason for the memorandum subsequent to the accident, which is dated February 3, 1986. And it subsequently turns out that you were asked to provide a memorandum to the effect along the lines of your summary just now. In other words, you were asked to do that by Mr. Mann?

MR. COOK: Yes, sir.

CHAIRMAN ROGERS: And the memorandum that is dated February 3 that you have just summarized was in response to that request, and I think your summary of the memorandum is very good and it explains to the Commission your motivation.

I would like just to ask one question about the memo. You say at one point when you are referring to the engineers, I believe you say—well, let me read the whole thing. "It is also my opinion that the Marshall Space Flight Center has not been adequately responsive to headquarters concerns about flight safety,

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that the Office of Space Flight has not given enough time and attention to the assessment of problems with SRB safety raised by senior engineers in the Propulsion Division."

Now this is the part I want to ask about. "And that these engineers have been improperly excluded from investigation of the Challenger disaster." In light of the work of this Commission and the investigations that are being conducted now at Kennedy, are you still of that view?

MR. COOK: Well, let me just comment very briefly on that paragraph. I editorialized a bit at the beginning and the end of this, and I did so on the basis of my general point of view in retrospect on some of these issues, and since I wasn't prepared to comment on this memo at all today I'm not going to try to go into a lot of detail about the first two items.

CHAIRMAN ROGERS: Well, it is really not necessary. I think the thing that concerns me most is whether you have confidence now that the investigations are being properly conducted.

MR. COOK: Well, if I had access to my files and time to write, I would try to be more specific. That is all. But let me say this. The last item, frankly I was amazed that when this incident occurred the engineers in Washington were over there in their

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offices getting the data on the investigations from the newspaper and the media, and now and then phone calls from guys down at Kennedy about what was being found.

These were the top propulsion engineers who prepared reports for the Office of Space Flight and for the Administrator and for us. I just couldn't understand why that group wasn't down there going through the data and looking at the photos and everything else. Frankly, and I will be honest with you—and I'm not intending to explain why that was or criticize anybody—I was just, in a way I was glad because I could go over and talk to them and get my information from them.

But I just couldn't understand why the headquarters propulsion office didn't have their guys down taking part in that. I have no question whatsoever about the investigation or the Commission's work. I don't feel I'm really competent to make much of a comment on that, although I must say I am glad that you all are having public sessions and that it is a presidential level group. I think that is absolutely in order and really needed.

The only thing that I would urge would be that as much as you can to get just the ordinary working guys, such as me and the engineers and the guys from the

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Marshall S&E Lab, and if you can get them in from Thiokol, just the ordinary engineers who break these things down, who look at them, who call each other on the phone and say hey, look what I found here. You've got to take a look at this. And that is what I hope will be included.

And I think that if everybody who has firsthand knowledge and experience and feels they can come up and talk freely, I think that you will have a good investigation.

CHAIRMAN ROGERS: Well, I assure you, Mr. Cook, we do plan to do that, and we have plans made.

Secondly, you told me during the lunch hour that you had great confidence in Jess Moore and the people who work for him—that is a fact, isn't it?

MR. COOK: Yes, sir.

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CHAIRMAN ROGERS: When you wrote the second memo of February 3, you intended that to be used for internal purposes only?

MR. COOK: Absolutely.

CHAIRMAN ROGERS: You have no objection now, in view of what has transpired, if it is made public, because we want to be sure as a Commission that we don't appear to be holding anything back, and President Reagan has wanted us to be sure to lay all of the facts on the table and to let all be known, and so you have no objection if we make that memorandum public?

MR. COOK: No, sir, I don't, although I must say that I think everybody who really works on this needs to understand, and I am not addressing the Commission, I am addressing I guess people who would be reading that, the complexities of the situation and the difficulties that any individual has in coming to conclusions.

CHAIRMAN ROGERS: Yes. Thank you.

You also mentioned this morning that you wrote the memo in the heat of the moment, and I assume you were, like everybody else in the country was, terribly disturbed and upset by the accident, and it was in that spirit or at that time when you wrote the memorandum. You didn't really mean to adversely criticize for public

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consumption your associates or people around you, did you?

MR. COOK: No, I didn't, but I must say, I didn't say anything in there that I didn't feel I had—that I couldn't back up and wouldn't stand by.

CHAIRMAN ROGERS: No, I understand. I am really not asking for you to back down at all. I think to understand the contents of the memo, it is helpful to have you say what you have said, I just mean that it was done in the heat of the moment, and I think that has helped us understand the memo.

One other thing, and then I will stop.

Sometime subsequent to this, I understand that you decided to change jobs in the government.

Could you tell us about that?

MR. COOK: Yes, sir. This had been going on for some time. As a matter of fact, I had worked before at the Treasury Department, and it just so happened by coincidence that I got an offer from them to return to the Treasury Department last week. This had been something that had been in the works for several weeks, and I will be reporting to work at the Treasury Department next week.

But it is a coincidence, and it doesn't have anything to do with this.

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CHAIRMAN ROGERS: It was voluntary on your part?

MR. COOK: Absolutely.

CHAIRMAN ROGERS: Thank you very much.

Are there any other questions?

(No response.)

CHAIRMAN ROGERS: Thank you very much, Mr. Cook. We appreciate your appearance and your frankness.

Mr. Mann?

(Witness sworn.)

CHAIRMAN ROGERS: Mr. Mann, I want to ask you a few questions about the memorandum that was from Mr. Cook to you dated July 23, 1985.

What were the circumstances surrounding that memorandum, do you remember?

MEMORANDUM 7/23/85

TO: BRC/M.Mann

FROM: BRC/R.Cook RC

SUBJECT: Problem with SRB Seals

Earlier this week you asked me to investigate reported problems with the charring of seals between SRB motor segments during flight operations. Discussions with program engineers show this to be a potentially major problem affecting both flight safety and program costs.

Presently three seals between SRB segments use double O-rings sealed with putty. In recent Shuttle flights, charring of these rings has occurred. The O-rings are designed so that if one fails, the other will hold against the pressure of firing. However, at least in the joint between the nozzle and the aft segment, not only has the first O-ring been destroyed, but the second has been partially eaten away.

Engineers have not yet determined the cause of the problem. Candidates include the use of a new type of putty (the putty formerly in use was removed from the market by EPA because it contained asbestos), failure of the second ring to slip into the groove which must engage it for it to work properly, or new, and as yet unidentified, assembly procedures at Thiokol. MSC is trying to identify the cause of the problem, including on-site investigation at Thiokol, and OSF hopes to have some results from their analysis within 30 days. There is little question, however, that flight safety has been and is still being compromised by potential failure of the seals, and it is acknowledged that failure during launch would certainly be catastrophic. There is also indication that staff personnel knew of this problem sometime in advance of management's becoming apprised of what was going on.

The potential impact of the problem depends on the as yet undiscovered cause. If the cause is minor, there should be little or no impact on budget or flight rate. A worse case [Ref. 2/11-21 1 of 4]

scenario, however, would lead to the suspension of Shuttle flights, redesign of the SRB, and scrapping of existing stockpiled hardware. The impact on the FY 1987-8 budget could be immense.

It should be pointed out that Code M management is viewing the situation with the utmost seriousness. From a budgetary standpoint, I would think that any NASA budget submitted this year for FY 1987 and beyond should certainly be based on a reliable judgment as to the cause of the SRB seal problem and a corresponding decision as to budgetary action needed to provide for its solution. [Ref. 2/11-21 2 of 4]

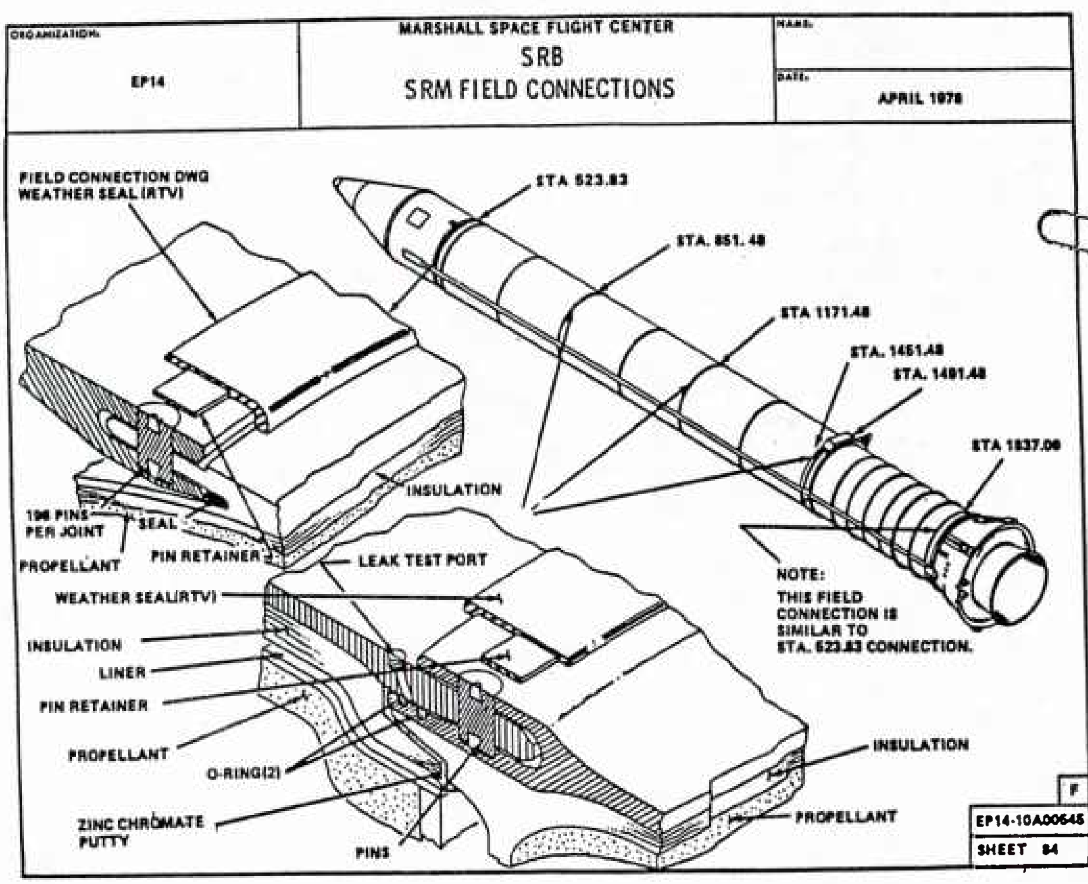
Richard C. Cook
Program Analyst

Michael B. Mann
Chief, STS Resources Analysis Branch

Gary B. Allison
Director, Resources Analysis Division

Tom Newman
Comptroller

[Ref. 2/11-21 3 of 4]



[Ref. 2/11-21 4 of 4]

NASA

National Aeronautics and
Space Administration

Washington, D.C.
20546

February 3, 1986

Reply to Attn of:

TO: BRC/M.Mann

FROM: BRC/R.Cook RC

SUBJECT: Required Solid Rocket Booster Improvements

There is a growing consensus that the cause of the Challenger explosion was a burnthrough in a Solid Rocket Booster at or near a field joint. It is also the consensus of engineers in the Propulsion Division, Office of Space Flight, that if such a burnthrough occurred, it was probably preventable and that for well over a year the Solid Rocket Boosters have been flying in an unsafe condition. This has been due to the problem of O-ring erosion and loss of redundancy caused by unseating of the secondary O-ring in flight.

The technical details of the O-ring problem were described in the attached memorandum from Irv Davids to the Associate Administrator for Space Flight in June 1985. Also attached is a copy of the memorandum I wrote on the subject on July 23, 1985, in which I stated: 'There is little question...that flight safety has been and is still being compromised by potential failure of the seals, and it is acknowledged that failure during launch would certainly be catastrophic.'

Even if it cannot be ascertained with absolute certainty that a burnthrough precipitated the explosion, it is clear that the O-ring problem must be repaired before the Shuttle can fly again.

It is not clear, however, how long this will take or what it will cost. The facts are:

1) The capture feature to be tested further on the Filament Wound Case is not yet a demonstrated fix to the problem and has not been accepted by Marshall Space Flight Center for use on steel SRB cases.

2) It is clear that the field joint putty plays a significant role in O-ring erosion. It must be replaced in any case, because it contains asbestos and is a potential health hazard to workers. Flight qualification of new putty is recognized to be a major unbudgeted cost item.

[Ref. 2/11-22 1 of 3]

3) The effects of environmental and weather factors on the putty and O-rings may have design implications which require further investigation.

4) Currently eight SRB flight sets are in manufacturing flow. Segments of these flight sets must either be retrofitted with the capture feature or field joint segments must be re-engineered and replaced. In either case, the propellant must be removed, either by hand-scraping under carefully controlled conditions or by static firing. One million pounds of propellant per flight case must be removed, at a cost for propellant alone of $2 million per flight set.

5) Five flight sets of hardware remain on the shelf at Thiokol, but should not be loaded until the O-rings are repaired. Consequently, we are looking at a probable factory shutdown.

6) A further potential safety hazard has been identified which can take away O-ring redundancy. If the leak test ports between O-rings are not properly capped before flight, a small leak in the field joint can result if the primary O-ring is eroded. Consequently, procedures to assure proper capping must be reassessed. Further, the instance of case rupture during test following failure to detect a crack at the stiffener ring bolt hole must call into question Thiokol's safety procedures and orientation. It should be determined whether this situation has any implications for the current negotiations for the third production buy regarding failure penalties.

7) Given the known O-ring problem, it could be construed as negligent not to have installed sensors on the surface of the SRB which could have detected a burnthrough or to provide a system to jettison the SRBs when loss of control is detected such as was apparently the case with 51-L. Videotapes show the burnthrough commencing twelve to twenty-five seconds before the explosion, and I understand that loss of SRB control was evident eight seconds before the explosion. Correcting this situation will require a considerable investment to revise instrumentation, software, and launch and training procedures.

8) It is possible that the capture feature will fail to be flight-qualified for steel segments and that re-engineering of the field joints will be required. In this case, three of every eleven SRB segments will have to be discarded, and re-engineered segments manufactured by Rohr. Manufacturing lead time would be thirteen months after successful acceptance testing.

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9) The attrition rate of SRB segments following flight has been far higher than the planning projections and has been made even worse by the loss of both boosters in flight 51-L. More intensive inspection for cracks and other anomalies will probably lead to further unanticipated attrition. It seems clear that the planning assumptions are unrealistic and must be revised. This will have major budgetary implications.

10) The Filament Wound Case project now needs to be reassessed, as there is an opinion among staff engineers that it should be discontinued for safety reasons. It is essential that such misgivings now be taken seriously.

Given these facts, it is my considered opinion that NASA is facing a suspension of Shuttle flights due to SRB problems of a minimum of nine months and possibly as long as two years or more. This assumes that the agency makes a rapid decision to proceed with the required SRB improvement program, along with improvements in Thiokol's safety management. The financial planning assumptions also need to be re-done. The commencement of this program does not depend on the outcome and final conclusions of the Challenger investigation,. since it would have to be done even if the explosion were due to some other cause. The delay could be longer, of course, if additional findings require re-engineering of the external tank or other hardware.

It is also my opinion that the Marshall Space Flight Center has not been adequately responsive to headquarters concerns about flight safety, that the Office of Space Flight has not given enough time and attention to the assessment of problems with SRB safety raised by senior engineers in the Propulsion Division, and that these engineers have been improperly excluded from investigation of the Challenger disaster. For these reasons I recommend that the SRB reassessment be led, tightly controlled, and adequately staffed at the headquarters level. I also recommend that the Acting Administrator be informed of the history of the SRB problems as outlined in this report.

cc: J.Brier
G.Allison
M.Peterson

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TESTIMONY OF MICHAEL B. MANN, CHIEF, STS RESOURCES ANALYSIS BRANCH, OFFICE OF THE COMPTROLLER, NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

MR. MANN: Yes, sir.

Rick Cook was a newly assigned employee, and the issue of the charring of the O-rings had come up in the normal course in the Office of Space Flight discussions. It was a perfectly open issue. It was discussed in an activities meeting, and as part of his training, I asked him to go talk to the engineers and to give back to me a report on what was exactly involved in the charring and what the budget implications were, and that was pretty much a normal part of the way we work. We try and keep, as Rick said, keep abreast of all the technical issues that have a potential budget impact.

CHAIRMAN ROGERS: What is your title, and what are your responsibilities?

MR. MANN: I am the Chief of the STS Resources Analysis Branch. It is within the Office of the Comptroller. We do the—we prepare the budgets. We work with the program office to prepare the budget for the Office of Space Flight and the Space Station. We also do independent reviews periodically of the issues

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that have a cost or a budget implication such as pricing policy for the Shuttle or particular cost issues.

CHAIRMAN ROGERS: And how long have you worked for NASA in that capacity?

MR. MANN: I have been in the Comptroller's office since 1980.

CHAIRMAN ROGERS: And you are Mr. Cook's superior officer?

MR. MANN: Yes, sir.

CHAIRMAN ROGERS: Now, when you received the copy of this memorandum, do you remember what you did with it or what consideration you gave to it and what actions you took?

MR. MANN: I did several things. One, I talked to Rick about it. One of the things I wanted him to develop was the ability to do a cost estimate, to actually take an issue like this and talk to the engineers and be able to come back with alternatives, programmatic actions, and the costs of each, and some judgment as to the relationship between the alternatives. And his memo hadn't done that, but it was kind of early, frankly, in his training to expect him to be able to do that.

I also took the memo to my boss, the Deputy Comptroller, and the Comptroller. I asked Rick to take

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it back to the engineering staff in the program office and discuss it with them, one, so that they knew what he had written, and two, so that if they had any—if they wanted to comment on it, that they would have that opportunity.

So I discussed it with my immediate supervisors, and I had it sent back to the Office of Space Flight, and then I also went to discuss with the engineers if the memorandum really reflected the situation as they saw it. And in those discussions which I got the feeling that maybe the memo overstated the concerns, that there were quite a few actions being taken within the program office to resolve the issue, that there were extensive reviews going on, as there are in almost any technical type of review.

So I was satisfied that it was in the proper technical channel, that it was being handled properly, that it was being handled rigorously and thoroughly, and over time, over the next couple of months, as I saw the report come out, the reports and the statuses within the program office, the immediate concern died down.

CHAIRMAN ROGERS: So you thought the points that had been raised by Mr. Cook in that memorandum were being taken care of, and you thought adequately as far

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as you could tell?

MR. MANN: Yes, sir.

CHAIRMAN ROGERS: Are there any questions for Mr. Mann on this particular letter?

DR. WALKER: I have a question, Mr. Chairman. I know that developmental programs within NASA have a contingency as a part of the budget to take care of unanticipated costs.

Now, with the Solid Rocket Booster Program, which I presume was operational at this point, was there still a contingency in the budget so that some of these problems identified with the

seals, for example, could be handled without additional monies being authorized or appropriated?

MR. MANN: Sir, there are three levels of reserve within the budgets. There is a project level reserve which would be within the Solid Rocket Booster Project. That particular level of reserve would not be sized to handle any kind of significant redesign effort. It is more an operational type of reserve.

There are then level 1 and level 2 reserves that are held at the headquarters or the Johnson level, and those reserves are really available to handle and to rebalance the program for particular problems, and those

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kind of reserves would largely be used for any kind of significant effort.

There is a third reserve that is called the Changes and Systems Upgrading Line, and it appears specifically in the NASA budget, and that is a reserve that the Administrator reviews, that he has available for improvements or significant upgrades to the program. And there was no—although in the Solid Rocket Booster, there was no specific reserve for this particular situation, there were reserves within the program that could be applied to it, and there was no budget determination made in terms of was there adequate funding? There was no impact on the engineering process as a result of the budget.

DR. WALKER: I understand that the July letter of Mr. Cook's was essentially a training letter. I was a little surprised to see that it didn't discuss whether or not these problems would be within the contingency or would require additional appropriations.

Would you have expected that this letter would have discussed that point?

MR. MANN: Yes, sir. What we really do is we try and size a problem. It is one thing to understand that there is a problem. It is an altogether different issue to be able to put a cost on it and then to

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determine where the Agency can come up with the funds to resolve it.

And so I wasn't particularly dismayed that in this particular instance it was, I believe, the first one that Rick had done, the first issue he really reviewed, that he had not been able to do a thorough cost analysis of it. But typically, that is exactly what we would expect. We would expect that here are the alternatives, here is the cost of each alternative, and this is where we think we can find the money within the overall budget to support it.

MR. ACHESON: For the record, Mr. Mann, does the Budget Division have its own engineering staff?

MR. MANN: We have some engineers on the staff, but they are not on the staff to be engineers. It just so happens that that is their technical background before they got involved in cost work. Our office doesn't try and do any kind of engineering evaluation or safety evaluation, which is why when I was assured that the issue on the charring was being resolved in the proper engineering channels, we pretty much stopped working on it.

MR. ACHESON: So on engineering and safety questions you are entirely dependent on the other engineering divisions of the Agency, is that not so?

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MR. MANN: Yes, sir.

DR. RIDE: I think it might also be appropriate to point out that Mr. Cook referred to some detailed engineering analyses earlier and recommended that the Commission review those, and

we were given those Marshall and Thiokol engineering reports yesterday, and have access to all of the data by the engineers on the program.

CHAIRMAN ROGERS: There is no doubt about it. Mr. Cook referred to these documents, and somebody is suggesting maybe that we have not had access to them, and in fact we have them, as Dr. Ride has said. We have all of the documents we have asked for, and they are included.

On the subsequent memo that I talked about, February 3 from Mr. Cook to you, I gather that you requested a memorandum of sorts from him after the accident?

MR. MANN: Yes, sir. We were trying to frankly size the potential impacts to the NASA budget as a result of the incident. I asked Rick to specifically do a cost estimate for the SRB project in the event that we had to make some change to the cases, and I specifically asked him to do that particular type of evaluation to try and size the budget impact without

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really knowing what it would be, that we were doing the same kind of analysis on the main engines, on the external tank, as a part of trying to see what the size of the budget problem would be.

CHAIRMAN ROGERS: He testified that he responded to that request in the heat of the moment and wrote the memorandum which we have referred to.

Were you surprised at the contents of the February 3 memorandum?

MR. MANN: Frankly, I was. I had specifically asked for a cost estimate, and I specifically needed a number, this is a $20 million problem, this is a $100 million problem, to integrate that with other estimates that we were doing on the rest of the program, and frankly, the memorandum didn't provide that kind of information. It had some useful insights into the particulars of the program, such as how many motors we had in inventory, but it didn't really come down to what I was trying to accomplish.

CHAIRMAN ROGERS: Any other questions?

(No response.)

CHAIRMAN ROGERS: Thank you very much, Mr. Mann.

Mr. Moore?

Mr. Moore, you have heard Mr. Cook's

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testimony, and his testimony specifically about the two memoranda that we talked about.

Do you care to make any comment on this memo?

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TESTIMONY OF JESSE W. MOORE, ASSOCIATE ADMINISTRATOR FOR SPACE FLIGHT, NATIONAL AERONAUTICS AND SPACE ADMINISTRATION—Resumed

MR. MOORE: Yes. I would like to make a few general comments, Mr. Chairman, and then I would like to introduce the division director, if I can, of my Propulsion Division who manages all of the NASA headquarters engineers on this, to also make some comments.

The two memos that were in question, I guess I saw the first memo that was dated on July 23, I believe, was that date. I guess I saw that for the first time on Sunday, and I think that there are some statements that in general are fairly correct in that memo. I think there were some areas that were somewhat out of context or a little bit exaggerated, but in terms of the activities that were going on in the program, I believe that our Propulsion Division, and I believe the Marshall Space Flight Center were taking all of the prudent actions to try to lay out a

program that would help us get a better handle on what we had been seeing in some of the previous flight experiences.

With respect to the memo that you just talked about on February 3, I just saw it last night, I guess,

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for the first time, and although there is some good information in there regarding the number of flight cases in inventory and so forth, at this point I didn't know the intent of that memo was the budget, and I was very surprised at the last paragraph in there where it seemed to say that we were not paying enough attention to flight safety. And under no conditions had I heard any of my people come up to me and say any of the flights were not safe for launching. So I was a little bit surprised by that particular comment in the memo.

And I also think we have had extraordinary cooperation out of the Marshall Space Flight Center in terms of working with us on this problem. The Thiokol test program has been under way for a number of months now, since back early in 1985, and in fact, back in the July time-frame some money was expended on long lead hardware in the event some of the tests that we had aimed for the QM-5 program, which was a test program that we were planning to run coming up this week, as a matter of fact, in the event it showed different kinds of data, we would have a leg up on this problem so we could go and fix it.

I would also like to emphasize to the Commission that we were simply trying in this program, in this demonstration program and in the test programs, to try to get more margin into a situation that we felt

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was safe for flight even though we had seen some of these O-rings and so forth. And so our programs focused on trying to get more margin in this thing.

And as was indicated in the previous testimony, we had planned to fly the filament wound cases on the upcoming Vandenberg launch. They were lighter weight cases, and we did have some different concerns. But that QM-5 test, which was a full scale test of the filament wound case, was an important article for us to test some of the analysis that had been done and also some of the laboratory tests that had been done to try to get a better handle on exactly the behavior of this overall joint.

And our plan was to take those tests and then make a decision in terms of what our next steps were relative to the continuation of this program.

And I would like to ask Mr. David Winterhalter, with the Commission's permission, who is my Division Director for Propulsion, to come up and give you a little additional in-depth insight into this whole area, if that pleases the Commission.

CHAIRMAN ROGERS: That is fine.

Let's see if there are any questions for you.

DR. RIDE: I have one. I think Mr. Cook implied that there had been a decision made to

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incorporate a capture device in the field joints.

Had that decision been made?

MR. MOORE: That has not been made to my knowledge, no, Sally. I was not aware—we had not made that as far as the overall program was concerned.

MR. ACHESON: One more question.

Are you aware, Mr. Moore, of any personnel at the Marshall Center or elsewhere who have been excluded from the accident investigation who have anything to offer?

MR. MOORE: I am glad you raised that. I was thinking about commenting on it, but I did not. The Marshall Space Flight Center has hundreds of people at the Huntsville Operations Center, which is a support center to every Shuttle launch involved in the detailed analysis of this accident. They are propulsion people. They are worried about the Shuttle main engines. They are worried about the external tank. They are worried about the solid rocket booster. In addition, we have contractor personnel there from all the supporting contractors on this program.

I think what Mr. Cook was referring to was that at the outset, when we set up the interim board, immediately after the accident, we basically impounded all the data associated with this tragic incident so

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that we could get a handle on the control policy on this particular data. We are now in the process of setting up formally our task force, and we plan to involve the Marshall Space Flight Center and other NASA centers, headquarters personnel in that analysis.

And so I think Mr. Cook's observation was probably correct at the outset because everybody was not allowed to be brought into the picture right up front, and that was because we impounded data, and we were trying to put very tight controls on access to any data associated with this so that we could preserve it for our accident investigation.

MR. ACHESON: Thank you.

CHAIRMAN ROGERS: Thank you very much, Mr. Moore.

MR. MOORE: I would like to introduce Mr. David Winterhalter.

(Witness sworn.)

CHAIRMAN ROGERS: Welcome, Mr. Winterhalter.

Please proceed.

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TESTIMONY OF MR. DAVID WINTERHALTER, ACTING DIRECTOR, SHUTTLE PROPULSION GROUP, NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

MR. WINTERHALTER: Mr. Chairman, members of the Board, first let me just tell a little bit about the division that I have. My title is Acting Director of the Propulsion Group at headquarters. It is a division. Within the division we have the overall policy type generation and overall direction of the major projects dealing mostly with the people at the Huntsville Marshall Space Flight Center. That includes the external tank, the solid rocket boosters, and the Space Shuttle main engines, and the various testing programs that go along with those.

I have a group that is broken up pretty much into those projects. They are small groups, one to three people per project, very senior people. I would say that they average over 20 years service in engineering and in manned space flight in the propulsion area, most of those years spent by these people at NASA, very well respected throughout the Agency, called upon many times for their outstanding judgment.

I don't want to address Mr. Cook's memo paragraph by paragraph at all, but I would like to mention that to my knowledge, the things that he said about the engineers not taking part in the

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investigation—I mean, I knew that the time would come, as Mr. Moore mentioned, that they would have their opportunity, and about the noncooperation with the Huntsville folks, as Jesse mentioned, I feel like we are a real team. We interact well, we work well together. Like any

team, we sometimes have our differences, but we are usually able to work them out and carry a coordinated recommendation forward.

And that would go pretty much to any type of situation in what might come up within the program.

If we see something that is somewhat out of the ordinary, either in our test programs or in our flight programs, obviously there is an immediate evaluation to try to find the magnitude of the situation and see if it might have some type of effect on upcoming flights or upcoming tests, etc., to try to understand if maybe we should slow down whatever we are doing and take another look.

Usually what we find is that in the initial evaluation it doesn't take drastic action to stop a program and stop a test program. What we have been doing is we have some low level analysis go on with prudent funding, and as we find out more about the situation, we decide that we don't need to work on it anymore, or in fact, we may need to step up the effort.

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Now, to get the specifics here on this particular area, in the nozzle joint and the field joints on the solid rocket boosters, as soon as we started seeing any indication out of the ordinary of what we had expected of the situation, somewhat small nozzle O-ring erosion, there was immediate, an immediate effort set up at the Marshall Space Flight Center to start looking into that area.

Now, as the situation evolved, in 51-D the secondary nozzle joint O-ring erosion, the effort was stepped up somewhat, and that brings us up into the late spring, early summer of last year when, as it was discussed with you yesterday, the test program at Thiokol, their analysis programs, etc., were beefed up and put into a regular plan, and that has progressed through the summer, through the fall timeframe of a more and more intensive analysis and investigation, etc., culminating in a November-December timeframe where they picked some of the better candidates for joint improvement to be tested in the next resource in the test program. That happened to be a filament wound case, but a full-up solid propulsion motor.

Now, it is very expensive to do all of the testing in solid propulsion system so you try to make use of whatever resources you have at hand. So they picked some of the

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better candidates to put into this test that has been described to you, and it had been planned for Thursday of this week, but it has since been postponed.

And that is sort of a summary of how we go about it in general, in that specific example.

I would rather defer to Mr. Mulloy for any more detailed discussion of that, but I just thought I would give you an overall feel for how we at headquarters look at things on an overall basis. My people work on a day to day basis with the Huntsville people. I know right after the accident everybody really wanted to get in and try to find out what happened, etc., but at our level we have other things to do. We try to take a look at the overall system and try to understand long term impacts, etc., and think through some various scenarios about how we might have to react to the total situation, not just on an individual system basis but on an overall basis, how do our systems interact with other systems. For instance, if there turned out to be a problem on the tank, what do we do in the engine program, what do we do in the solid rocket booster program to maybe free up some money to work on the other to make a more balanced program?

That is our job. As much as being an engineer, we like to jump in and really dig into the

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details and roll up our sleeves and look at all of the data and that sort of thing that is not our job at this time. However, I expect, as Jesse mentioned, as he forms his teams, we have some particular technical expertise within my group, and I am sure that he's going to call on some specific people for help.

So that is what I am anticipating now.

CHAIRMAN ROGERS: Well, thank you very much.

It is fair then to say that after, or at about the time Mr. Cook's memorandum was written in July 1985, that you and your team were—had been and were at that time conducting a lot of investigations and doing a lot of work about the O-rings, and that you continued to do that all during this period up until the 51-L flight so that intensive work was being done by engineers who were highly qualified to do it, is that correct?

MR. WINTERHALTER: That is correct, sir, and I think some of the material that we presented to you yesterday will show that our own internal division meetings and our discussions with Jesse Moore, and our monthly meetings, etc., we talked about this area, we talked about what we were doing about it. We tried to talk about potential impacts if, in fact, various scenarios might work out, depending upon how much redesign might be required or desired, this is how it

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would affect the budget, how it might affect schedules, when we could get it incorporated, etc.

And so we were doing that pretty much for the last year. And as I mentioned, it sort of intensified as the year went on. But that is just sort of the normal way we do business.

CHAIRMAN ROGERS: And it is a very open organization, I gather, in terms of exchange of views and ideas, and everyone is permitted to write memos and make comments as he sees fit? But in the final analysis, the qualified people, the engineers and others who are assigned the responsibility make the decisions, have to make the decisions.

MR. WINTERHALTER: That's true.

CHAIRMAN ROGERS: And the decisions in this case were made pursuant to that policy.

MR. WINTERHALTER: That's true. I pride myself in our division to be particularly good team workers. We have our differences. We work them out. We can work them out with the centers. We do it all the time. And we carry a unified front forward, or we say, okay, this is the way we feel and we also give the other side, if that be the case.

CHAIRMAN ROGERS: And I suppose you have some confidence in your judgment because in 24 previous

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flights you have made good judgments.

MR. WINTERHALTER: That's true.

Let me mention, at no time during that period did any of my people come to me and give any indication that they felt like there was any safety of flight problem in their area. We all worked—we were all working on the situation, but we were all involved in the flight readiness reviews that have been described here today. We take an active role in that, we can make inputs.

Mr. Moore is particularly good, and his deputy, Mike Weeks, involving everybody in these reviews, in these monthly reviews, and everybody is encouraged to feel free to speak out in any area. And let me tell you, sir, it happens. People aren't afraid to speak up. They are not con-

strained by saying someone will take some action against them if they bring up something that is unpleasant.

VICE CHAIRMAN ARMSTRONG: Was it the view of your division, the Propulsion Group, that the seal design, as it was installed and operating in the Shuttle System, was safe and adequate?

MR. WINTERHALTER: It was.

VICE CHAIRMAN ARMSTRONG: But it was further your group's view that the margins on the seal design

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were not such that it would not benefit from improvement, is that correct?

MR. WINTERHALTER: That is correct. We felt like it could be improved. We weren't happy with the situation the way it was. We understood that the testing had been done, the margins, etc., but as you are well aware, we are always striving to make things perfect.

MR. FEYNMAN: If the matter of Mr. Cook is more or less covered, I would like to ask some detailed questions about seals either from you or anyone else you wish to call, like Mr. Mulloy.

MR. WINTERHALTER: I would just as soon that Larry came up and talk about seals since he did such an adequate job this morning.

If you have some general questions—

MR. FEYNMAN: No, it is a specific, detailed question.

CHAIRMAN ROGERS: Thank you very much.

MR. FEYNMAN: We spoke this morning about the resiliency of the seal, and if the material weren't resilient, it wouldn't work in the appropriate mode, or it would be less satisfactory, in fact, it might not work well.

I did a little experiment here, and this is not the way to do such experiments, indicating that the

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stuff looked as if it was less resilient at lower temperatures, in ice.

Does your data agree with this feature, that the immediate resilience, that is, within the first few seconds, is very, very much reduced when the temperature is reduced?

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TESTIMONY OF LAWRENCE MULLOY, SOLID ROCKET BOOSTERS PROJECT MANAGER, MARSHALL SPACE FLIGHT CENTER

MR. MULLOY: Yes, sir, in a qualitative sense. I just can't quantify that at this time.

MR. FEYNMAN: Then you would say that I would conclude from that and the various things that you told me about the need for resilience and the lack of resilience within the first few seconds, and of course, it comes back very slowly, isn't it true, then, that the temperature at a low temperature increases the chance of a joint failure?

MR. MULLOY: The low temperature increases the time that would be required for the O-ring to extrude into the gap, and that would allow greater erosion on the O-ring, yes, sir.

MR. FEYNMAN: Did you have available—there was some kind of a temperature limit, then, on when you would say a seal was safe? Was there some kind of criterion that if the seal was lower than a certain temperature we cannot consider it safe enough?

MR. MULLOY: Yes, sir. The data that we had—as I stated, we are running extensive testing to understand the response of the seal under the specific conditions of 51-L, but the data available to us was the

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procurement specification for the material that says that it will operate from minus 30 to 500 degrees F, and also, a great deal of test data in motors down at temperatures where you can see a difference in the resilience of the seal, for instance, going from 75 degrees down to 50 degrees, you can see that. We had those data available.

We had data that was presented on the 27th by Morton-Thiokol, some test data, that indicated a further reduction in the resilience of that if the temperature was down to 20 or 25 degrees. It was the judgment that with the resilience that we saw there, the shrinkage of the O-ring, and under the circumstances of the leak check that is done, that at that temperature the O-ring would perform its function.

Now, the data that we did see is refuted by some other test data, and that is why we are carefully running controlled tests under the specific conditions of 51-L to understand the response of that O-ring seal, and will it perform its function in that environment?

MR. FEYNMAN: Of course, after the event is a different circumstance, and I am not really considering that. That is not a fair question. It could be that you didn't notice the circumstances.

I just want to know, before the event, from

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information that was available and the understanding, was it fully appreciated everywhere that this seal would become unsatisfactory at some temperature, and was there some sort of a suggestion of a temperature at which the SRB shouldn't be run?

MR. MULLOY: Yes, sir. There was a suggestion of that, to answer the first question. First, the data that was presented, it was the judgment that under the conditions that we would see on launch day, given the configuration that we were in, that the seal would function at that temperature. That was the final judgment.

There were data presented, as we have discussed, by some—by Thiokol engineering that there was a suggestion that possibly the seals shouldn't be operated below any temperature that it had been operated on previous flights.

CHAIRMAN ROGERS: Are there any further questions?

MR. ACHESON: One or two questions.

What is the pressure at which the tests have been run postflight to simulate this condition?

MR. MULLOY: That is the hydrostatic proof test, which is 112 percent of 1000 psi, or 1120 psi. But that does not simulate the rise rate that you have

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with the motor ignition. That is a static pressure test.

MR. ACHESON: I assume that it would just take a very short time for the O-ring to become the same temperature of the surrounding ambient air since there is no insulation between the outside and the O-ring, is that correct?

MR. MULLOY: That is correct. There is a lag, and that is another part of the thermal analysis, to look at specifically, with transients in ambient temperature, how does the steel, the propellant and the O-ring track that ambient temperature. But your statement is true, it tends to track the ambient with some few degrees of lag.

DR. COVERT: What was the temperature at the time of the launch, please?

MR. MULLOY: The ambient temperature?

DR. COVERT: Yes, sir.

MR. MULLOY: I believe about 38 degrees.

Jess, is that correct?

MR. MOORE: That is the number I have heard, 38 degrees.

CHAIRMAN ROGERS: Unless everybody is really eager to press this on, I think it would be helpful if we considered this in detail and carefully at the next session, and I would hope that we could ask individual

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questions when we are at Kennedy, and we would prepare very carefully to have answers for all of these questions in a full session.

I would think that in order to do it with care and deliberation, that it would take probably a full day, and unless the members really want to press it now, I would prefer to do it at that time.

MR. MULLOY: Mr. Chairman, I would like to make a clarification of testimony.

CHAIRMAN ROGERS: Can I say that at that time we will consider the weather, temperature effects, environmental effects and all of those considerations which I think deserve a full day of testimony.

MR. MULLOY: May I clarify some testimony that I made this morning relative to the events on the 51-L and our observations, the rationale that we used given the O-ring erosion?

Some of the questions I have had from the media indicate that it wasn't perhaps as precise a statement as it could have been. As I stated, what I was concentrating on this morning was the case joint since that was the primary area of interest. All of the data that I presented, the 6 of 171, the 2 of the 6 and so forth was related to the case joints. I stated that in leading up to the 51-L flight readiness review, that we had had no significant erosion or no

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erosion in the past year.

That is a true statement for the case joints. It is not a true statement for the nozzle-to-case joints. The April 1985 is when we had that secondary O-ring erosion on the nozzle joints, and we were watching that very closely, and we did have some subsequent cases of erosion on the nozzle-to-case joints which were well, well below the threshold of what our limits were on that.

So I wanted to clarify that in my statement, that was related to the case field joints as opposed to the nozzle factory joints.

MR. ACHESON: But was that erosion on the primary ring or the secondary ring?

MR. MULLOY: In April of 1985 is the one case where we saw erosion of the secondary ring, and that was on the nozzle-to-case joint. And that is when we went back and did some further extensive testing on that particular configuration, and we did have some after that. We did have some subsequent erosion on the primary ring, but never again on the secondary ring. And it was a very low level of erosion and deemed to be within the rationale for the flight after we had the secondary O-ring. We didn't have anything like that magnitude of problem as we continued up through the 51-L

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on that particular joint.

CHAIRMAN ROGERS: What do you mean by erosion? How do you describe erosion?

MR. MULLOY: Erosion is absolute removal of the material. It is like an erosion of a ditch by water. The material is totally removed, and it is a divot in the surface.

MR. ACHESON: By absolute, you don't mean removal of all the material.

MR. MULLOY: No, sir. I mean, when I speak of 38 mills of erosion, I mean that the erosion of the 280/1000 inch O-ring, that there is a bite in that O-ring, and it is a bite shaped elliptical type of erosion that is 32 or 38 mills deep, and it tends to be anywhere from 10 to 50 times the

length of the depth of the erosion is what I am speaking of. But the material is totally removed, and the eroded surface, the sheen is gone, but it is a very smooth surface.

VICE CHAIRMAN ARMSTRONG: Is the band that surrounds the pins on the critical items list?

MR. MULLOY: The steel? No, sir. That is a steel band, and it is not, because the—we have the cork over that to retain that. And on our analysis—and the reason again it is not is because the cork is a first failure point, and then when the motor is

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pressurized, the band is so taut that analysis shows that it can't lift off, and therefore its heating is the same as the basic structure, and no thermal protection is really required in that area. The steel itself is sufficient to take that.

VICE CHAIRMAN ARMSTRONG: The cork isn't a structural member?

MR. MULLOY: The cork is not a structural member. Initially it was there for thermal protection, based upon the early flights. It also served the function of holding the band as we got the ascent thermal environments, and concluded that really, that the cork wasn't required there, we left it in that position because we still had to have something as a secondary restraint on that band, which provides a second protection device that keeps that band from being a critical item.

VICE CHAIRMAN ARMSTRONG: If the band which is under tension, as I understand it, were to snap, what would the effect be?

MR. MULLOY: Nothing as long as the cork is there. The cork would retain the pins.

VICE CHAIRMAN ARMSTRONG: It wouldn't destroy the cork?

MR. MULLOY: I guess to get enough—I would

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have to look at analysis. I suspect to get enough strain to snap the band, you would probably crack the cork, but to get enough strain to crack the band, you have also overpressurized the motor. I don't know what—I don't know what the margin is between 1000 psi on the motor and how much stretch you could put into that band before it would break. But you would have to overpressurize the motor in order for that to happen.

MR. FEYNMAN: You spoke of erosion. There is also a phenomenon called blow-by, and I would like the answer to two questions, and the first is, during the last year where you said there was no erosion, was there any blow-by in the field joints? And second, are you more worried by blow-by or by erosion?

MR. MULLOY: Well, I am more worried by erosion with blow-by, and the reason for that is the erosion reduces the cross sectional area of the O-ring, and the erosion can be caused by two things, which is the direct impingement, and then the blow-by erodes it in a different area which further damages the O-ring and reduces its possibility of sealing.

Let me look at the data that I have provided you yesterday, and again, to be precise about my statements, the statements were leading up to the 51-L FRR, and I did state that after the 51-L FRR, we

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disassembled the 61-C where we found some almost immeasurable erosion on the primary O-ring, almost immeasurable, like .004 inches of the 280/1000s. On the 61-A, which flew in October of 1985, we did record soot behind the primary O-ring with no evidence of erosion, and so what we were seeing there, again, the primary O-ring was displaced during the initial pressurization. There was some gas blow-by for probably from zero to 50 psi, probably very, very low, and then

the O-ring was seated, but that was a hot gas which pyrolysed the grease and putty, which is what deposited the soot. None of that came from the O-ring, because there was absolutely no erosion, the sheen wasn't even gone on the O-ring. There was no indication of heat effect on the primary O-ring.

CHAIRMAN ROGERS: I just want to add to what I said before, that at the hearings that we have that will deal with temperature effects, and weather and environmental effects and so forth we will want the Thiokol people there.

Yesterday we had a representative from Thiokol there, but he did not have as much firsthand information as we would like, and so we want to be sure at that hearing to have firsthand information and a chronology of all of the events preceding the launch that dealt

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with weather and environmental problems.

DR. WALKER: Mr. Chairman, I had an additional question on O-rings, if I might.

The O-rings are not one continuous piece of material. They are bonded in several places.

MR. MULLOY: They are vulcanized together.

DR. WALKER: If the erosion occurred at the location of one of the splices or bonds, would that do you think pose a more serious problem to the seal?

MR. MULLOY: I don't know the answer to that, and I don't know that we have any correlation with erosion at that joint, and I would like to take that as a question and look at that. I really don't know whether erosion at that point would be more critical or whether it erodes more or less in the vulcanized joint.

DR. WALKER: Presumably the erosion is occurring where there are penetrations through the putty, so if the penetration would happen to occur at a joint, then presumably the problem would occur there.

MR. MULLOY: Yes, sir.

Let me take that as a question and look at our test data, whether we have even tested it at a splice joint, and whether that is better or worse in terms of the erosion rate of the O-ring.

CHAIRMAN ROGERS: Thank you, Mr. Mulloy. You

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have been very helpful. We appreciate it.

I would like to close the session now today, and as I have said, we will plan to go to Kennedy on Thursday.

I would also like to remind particularly those from the media that the Commission was asked to conduct a calm and deliberate investigation, which I think so far we have managed to do reasonably well. I would, though, suggest that we, because of the press attention, we are not going to continue to be available individually for individual press conferences, and I hope the media will understand that. We are going to provide as much information on a regular basis as we can, probably principally through a spokesman, and therefore we would ask your indulgence and not to intercept our progress in and out of the building.

(Laughter.)

CHAIRMAN ROGERS: I realize it is a laughing matter. I realize nobody will pay any attention to it.

(Laughter.)

CHAIRMAN ROGERS: Anyway, it's a good try.

Thank you.

(Whereupon, at 3:40 o'clock p.m., the Commission recessed subject to the call of the Chair.)

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PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—THURSDAY, FEBRUARY 13, 1986

Kennedy Space Center
Cape Canaveral, Florida
The Commission met, pursuant to recess, at 11:05 a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
ROBERT RUMMEL
DR. ARTHUR WALKER
DAVID C. ACHESON
DR. RICHARD FEYNMAN
MAJOR GENERAL DONALD KUTYNA
ROBERT HOTZ
DR. EUGENE COVERT
ALSO PRESENT:
AL KEEL, Commission Executive Director

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PROCEEDINGS

CHAIRMAN ROGERS: I would like to say that we're very pleased that you made provisions for us to be here so quickly and so thoroughly, and how busy all of you have been, and thank all of you for making it possible.

Secondly, I would like to suggest that, in view of the story this morning in the New York Times resulting from yesterday's briefing, that you get the appropriate people to start thinking about this criticality one problem, because it came through in the newspapers as if the waiver was of tremendous significance, and it appears as if the waiver only applied to this particular flight and this particular problem.

And so I think we may want to, after we've had a chance to talk to you about it, pointing out that this is not all that unusual, that it is not a waiver as such, that you've isolated a criticality problem and then you've thoroughly considered whether that should result in the stopping of flights or not or whether it was something that was an important factor and you had done all you could about it, but you decided to proceed.

Now, if you can show that that is also not an

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unusual circumstance, the same papers and the same documentation would show a lot of other aspects of the shuttle program, that would help you.

If you saw the paper this morning, it sounds very serious, just in the case of the O-rings, and you waived something that was dangerous. So give some thought to how we might handle that possibly this afternoon.

MR. MOORE: Yes, sir. Let me just comment on that. We started an action yesterday going back through the entire program looking at category one items, and there are a number of them in the program, and I think we will put that particular situation in context.

Mr. Chairman, let me also make a couple of other comments quickly. We have people here from three NASA centers—the Kennedy Space Center, the Marshall Space Flight Center, and the Johnson Space Flight Center, to try to support you and your Commission here today.

You will see reports from our various teams that have been formed in terms of where they are in their analysis and the results of the work that they have done to date on this thing. And we have tried to sketch the agenda out so we can give you reports, and tomorrow, I would like to close tomorrow, if I could,

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with telling you what our kind of forecast, being a schedule of activities to encompass the additional analysis that we plan to undertake, as well as the additional tests that we plan to undertake trying to validate various phase errors.

So with that, I would like to turn the meeting over to Mr. Dick Kohrs of Johnson Space Center. And Dick is prepared to cover in detail the environment and the events time line, which I think has been of high interest on your list.

So with that, let me turn it over to Dick Kohrs.

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STATEMENT OF DICK KOHRS, DEPUTY MANAGER, NST PROGRAM OFFICE, JOHNSON SPACE CENTER

MR. KOHRS: I'm Dick Kohrs from Johnson Space Center, Deputy Manager of the NST Program Office, and I work for Arnie Aldrich.

Jess mentioned I was going to talk about environments today. I'm not going to talk too much about the environment. I'm going to give you the weather that we had on launch day during our final mission management teams, and we're building a more detailed environment and discussion of the weather that we hope to have ready later on this week.

(Viewgraph.) [Ref. 2/13-1]

Could I have the next chart, please, which is the outline.

(Viewgraph.) [Ref. 2/13-2]

I'm going to go over a few charts on the pre-launch time line and the weather summary I'm going to give you is the weather summary as we dealt with it in our mission management teams that we had the last day, starting with the L minus one review, and just highlight that for you. And then I'm going to go through the ascent time line, and then I have some detail of the data base that we used to build the ascent time line, and show you those data

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points.

It is both telemetry data and data that we recreated from the photos which you're going to see next and Charlie Stevenson is going to present.

The next chart, please.

(Viewgraph.) [Ref. 2/13-3]

The first part of the time line, which is really kind of gross. It is a pre-launch time line which takes us all the way back to the ET on dock at KSC back in August of last year. The orbiter from the last mission returned to Kennedy on the 11th of November. The SRB stacking for this stack was the 4th through the 10th.

ET SRB mate was on the 10th, and it's a little bit out of sequence here. At the same time while the orbiter was in the OPF, we were putting in the Spartan Halley into the orbiter cargo bay in the horizontal.

MR. KEEL: Mr. Chairman, could I make one suggestion, that you don't use the acronyms, for the benefit of the Commissioners.

MR. KOHRS: I will do the best I can.

The external tank on dock is the ET. ORB is the orbiter. Of course, the SRB the solid rocket boosters. Then the mating of those.

The orbiter-external tank mate is on the

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16th. We transferred the total stack from the crawler to the pad on the 22nd of December, secured the vehicle, essentially powered it off. From the 24th to the 3rd was the time for upgrade here at Kennedy.

In the meantime, the cargo, the IUS, and the TDRS data satellite went out to the cargo prior to Christmas, stayed in its payload canister until after the holidays, and then was installed in the orbiter here on the 5th of January.

The TCDT is a terminal countdown demonstration test. It is an all-up test of the flight vehicle with the Kennedy team that goes through a simulated countdown down to T-zero, then after T-zero they run a couple of anomaly cases in plus time just for training primarily.

CHAIRMAN ROGERS: Are you going to be explaining that a little bit later, because I really don't understand that. I don't understand what you just said.

MR. KOHRS: Prior to every flight—and it's normally about two weeks—with the stacked vehicle in its flight configuration, with the flight crew on board, here at Kennedy we conduct what is called a TCDT, which is a terminal countdown demonstration test, that exercises the flight vehicle and the flight crew and the ground crew.

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We do not tank the vehicle, the external tank, during that test, and we do not run our auxiliary propulsion systems, we do not run the APU's or we do not run the booster HPU's. But it is the best we can simulate is a detailed time line countdown that we're going to do on launch day.

DR. COVERT: When you say you don't tank it, does that mean the tanks are empty?

MR. KOHRS: The orbiter OMS tanks and RCS tanks are full. And when I say don't tank, it is the external tank, the liquid launch.

DR. COVERT: And the RCS, this is the rocket control system that you use?

MR. KOHRS: Yes, that is not tanked. And the reason I put that on here, the hyper load came after the terminal countdown demonstration. The hyper load is the OMS and RCS, et cetera. And I apologize for these acronyms.

GENERAL KUTYNA: Dick, one detail on stacking. Did you take out one segment? We heard the aft center segment?

MR. KOHRS: That's going to be covered, I think, on this afternoon's agenda. You will have a detailed discussion of that. Here I was just trying to give you a view of how things progressed here at KSC.

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CHAIRMAN ROGERS: But as far as TCDT, did you take and have the astronauts out there and the whole crew?

MR. KOHRS: Right.

CHAIRMAN ROGERS: And you planned to try to simulate what it would do in terms of time, what each person has to do?

MR. KOHRS: Right.

CHAIRMAN ROGERS: Does any inspection occur at that time?

MR. KOHRS: Not to my knowledge. We don't really do any detailed inspection. It's just a dress rehearsal.

MR. RUMMEL: Does that include a system checkout?

MR. KOHRS: The best we can. It powers up the orbiter subsystems. We don't—the fuel cells are not powered up. We use ground power and the best we can simulate we power up the guidance system. Primarily, it is the guidance system we power up.

MR. RUMMEL: How about the telemetering data system?

MR. KOHRS: All the data is telemetered to the ground through the umbilicals. Some of it is RF and that data is recorded.

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MR. RUMMEL: But at that point you check the propellant meter system out to be sure all the circuits are working?

MR. KOHRS: To be sure all the data flows and all the red lines are passed that apply to that dress rehearsal, yes, sir. It is as close as we can get to—the best words are a dress rehearsal, with the flight crew and the ground crew.

CHAIRMAN ROGERS: Dr. Ride was just saying, in addition, what happens to the engines?

DR. RIDE: Well, we don't—I guess the best way to put it is that it is a dress rehearsal for the crew and the launch control center and the orbiter systems, and you go through an entire countdown, including the data and information that you would be getting on launch day.

In the launch control center and on board, you go through the regular countdown, and really you go down to zero. But we don't light the engines, obviously, and you don't load the external tank, and you don't start the auxiliary power units, and you don't light the engines. But basically everything else is a dress rehearsal for the launch.

CHAIRMAN ROGERS: And that is for the purpose of a dress rehearsal. You don't learn anything about

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the condition of the shuttle at that time?

MR. MOORE: No, you do learn about the orbiter system and so forth, sir, at that time.

CHAIRMAN ROGERS: What do you learn?

MR. MOORE: We learn if some of the orbiter avionics systems on board are functioning properly. We also learn if we have got any problems with the ground processing system to get ready for launch in the launch control center.

So we do learn a lot, and that allows us to put in any kind of corrections required prior to actually doing the launch. That is a very important milestone that we go through on each mission.

CHAIRMAN ROGERS: And I suppose you will be telling us later what you learned?

MR. MOORE: Yes, sir.

MR. SUTTER: When you take the system out of the assembly shed and you put it onto the moving transfer plate—

MR. KOHRS: The mobile launch platform and the crawler.

MR. SUTTER: This introduces—you take it under one loading condition and you put another load on, like the solid rocket boosters. How is that controlled? Is that done the same way every time, and

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are there checks after you take it from one place to another the shifting in load hasn't affected the mating between the solid rocket boosters and the external tank?

Or could something happen there to say affect the loading of the joints and the seals? And how is that, you might say, inspected to make sure that a movement like that is consistent and in line with all of the documented specs?

MR. KOHRS: The best way I would describe that is, when we stack the vehicle in the vertical assembly building we have the strain measurements of the holddown posts, and that data is recorded as we stack the vehicle. We have level-in requirements that the vehicle has to be to a certain level requirement to get the loads balanced between the eight posts, and I think that criteria is something like 10 thousandths to start a stack.

We have criteria dimensionally that says, when you get it to the top where the ET attaches, you have to have a certain dimensional criteria that you have to pass.

MR. SUTTER: But do you continue those measurements as you make these transfers?

MR. KOHRS: Yes. After the vehicle is stacked, you roll out to the launch pad. Then you again

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have measurements to tell you what the loading is on the stack out on the launch pad.

And we also have that data coming down during the liftoff of the loads that we are putting into the holddown posts. And you will see in my later time line one of the things that we haven't completed yet is to accurately reconstruct the liftoff loads based upon that data, strain gauge data, then based upon the film analysis, to make sure that this vehicle has lifted off within our envelope that we had on the previous 24 flights. And that is ongoing.

MR. SUTTER: So you are checking those loads against your previous flights?

MR. KOHRS: Yes, and the film data, because we have film data in terms of the drift as you go out of the ascent.

MR. CRIPPEN: Dick, just for clarification, what were the measurements that you had on the holddown points? We don't instrument, nor do we measure, the joints per se after the stacking?

MR. SUTTER: What about the loads attached between the solid rocket booster and the main tank? Is that an important load that could vary?

MR. KOHRS: Let me back up. In the OFT program which you saw last week, which was our first

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five flights, we flew one—we flew one of two that it was heavily instrumented, as were the SRB's, as were the struts.

We have gathered our data base during that time frame and have used that data base to say, if we stack within these dimensional tolerances we're going to be within our load acceptance.

MR. SUTTER: But since that time, you've changed the structural characteristics of the center tank. You took 5,000 pounds out of it. You've increased the power of the solid rocket boosters, not by much but like about five percent. And you had a margin you were dealing with at that time in those other flights.

But what has happened to that margin for this flight?

MR. KOHRS: Analytically we have done that. We can show you analytically what the margins are with the new configurations. We have not passed—STS-5 had what I would call a heavily instrumented DFI. That is development flight instrumentation.

That is not real time. It was recorded in on-board recorders, and once we landed we analyzed that data on the first five flights. But based upon that data, based upon our structural models, we have analyzed

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what the new loads are, and it is based upon that that we proceeded.

MR. SUTTER: You are talking about doing some more tests, though, to verify that?

MR. KOHRS: Yes, sir. And I think George Hardy is going to talk a little bit later, I think he is, on that.

The hypergolic load, which is the OMS and RCS, was done on the 16th, and on the HPU, is the solid rocket booster.

CHAIRMAN ROGERS: Just before we got to that, what does that mean, hyperload? What did you do?

MR. KOHRS: Sir, the OMS and RCS, which are our maneuvering systems on board the orbiter, are hyper propellant, and roughly the OMS is in the neighborhood of 23,000 pounds and the RCS is in the neighborhood of 7400 pounds.

GENERAL KUTYNA: These are hypergolic propellants. They don't need any ignition.

DR. COVERT: And the OMS is the orbiter maneuvering system. That's the rocket that allows you to rotate.

MR. KOHRS: And the reaction control system are the small jets described last week.

MR. MOORE: Dick, let's make sure we cover

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each of the acronyms up here, because that is a communication problem we've got with some members of the Commission. So let's make sure we explain the acronyms.

MR. KOHRS: The data on the 17th is the HPU, which is the propulsion system for the SRB's. It is very similar to the orbiter's auxiliary propulsion system. And we did roughly a 20 second hot fire prior to launch in the 17th time period.

And finally, at 1/23 we picked up a point in our countdown that is called the beginning of the final count or the beginning of the terminal count, and that begins at T minus 43, which in calendar days, 43 hours in calendar days is roughly three days before the planned launch.

You've got some built-in holds, but this is in our clock counting terminology.

MR. RUMMEL: Could you explain the HPU a little more fully?

MR. KOHRS: The HPU and the SRB control the actuators that drive the nozzle for the flight control.

MR. RUMMEL: How is it powered?

MR. KOHRS: By an auxiliary power propulsion unit, very similar to the orbiter, which is basically a hydrozene powered system. And the only major difference

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between the orbiter and the SRB is the orbiter has some requirements where it has to go through the total ascent. It has to be able to restart it on flight, and it's used during entry for elevon control and surface control.

The SRB HPU basically only needs to work during this 20 seconds prior to liftoff and through the 128 seconds of burn. It is recovered, it is reused for following flights.

MR. RUMMEL: Thank you.

MR. KOHRS: Could you put up the next chart, please?

(Viewgraph.) [Ref. 2/13-4]

What we have done here at Kennedy is, for this flow which involves the orbiter, the external tank, the SSME, and the solid rocket boosters, we have gone back and are reviewing all of the paper that was generated during that flow, and we have appointed special teams, different than the guys or the people that did the work, to go back and relook to satisfy ourselves that that paper was properly closed out.

To date, that amounts to about 2,000 pieces of paper of different actions and things that were done since the 26th of August. That data we estimate we will have complete probably within ten days to two weeks.

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MR. WALKER: Is that a fairly typical level of paper?

MR. KOHRS: That is typical of a flow. And the chart on the right is just an example. I won't dwell on it, but it is an anomaly that occurred when the orbiter landed at Dryden from its last flight, where we had a platform interference with the orbiter and we nicked a couple of tiles. And the people went back to relook at that to make sure that that was satisfactorily closed out.

CHAIRMAN ROGERS: Is there anything in that time line that is of significance in terms of anomalies or anything else that was suspicious that might have affected 51-L so far?

MR. KOHRS: No, sir. The only thing I would think up here—and we've had a lot of weather discussion—is we did go to the pad.

CHAIRMAN ROGERS: Well, we will come to that later. But I'm just thinking as far as this chart, the time line, is concerned, there is nothing unusual about it, and this was the way you normally handled it, and what you're doing is describing how it worked, and it worked in this case without any problem as far as you could tell?

MR. KOHRS: Yes, and the amount of paper we

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created on this flow I would say is typical of other flows.

The next chart then is going to take you from the T minus 43 point, which is the start of—the next chart, please.

(Viewgraph.) [Ref. 2/13-4]

—which is the start of the terminal count. And it extends, and I will have to go through my acronyms, it extends from the pickup at 10:00 a.m. on the 23rd of January, goes through our final launch down here to 11:38, which was our launch time.

Basically, at T minus 43 hours there's a standard set of terminal count flows that were being followed with no unusual circumstances. On the 25th at 11:00 a.m., we had our L minus one day. And "MMT" stands for mission management team. We talked a little bit about that last week.

Next chart, please.

(Viewgraph.) [Ref. 2/13-5]

At that briefing the projects and all of the cargo gave their go for launch. The only questionable item we had on that day is, we had a questionable weather predicted for the Sunday launch on the 26th.

CHAIRMAN ROGERS: How large is that MMT team?

MR. KOHRS: We have some charts. The MMT team

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on L minus one is done both here in person and by telecon around our loop. If you count up all those people, we probably had close to 100 people: primarily all the field centers, their contractors, the cargo people and their support, the Kennedy people and their support here locally.

CHAIRMAN ROGERS: Are the telecons recorded?

MR. KOHRS: No, sir.

CHAIRMAN ROGERS: Were there any summaries made after the conversations?

MR. KOHRS: Normally, at the L minus one mission management team the record is—the presentations that were given at that meeting, that is in the record.

CHAIRMAN ROGERS: But no record of the comments were made?

MR. KOHRS: No, sir, no formal record has been made. That is a good point. At the FRR action items are documented and, as I said last week, we close out the action items that we have from the FRR at the L minus one, and that closeout of all the open action items are documented and in the record as closed out formally.

CHAIRMAN ROGERS: When you say "closed out," you mean checked? Everybody says okay?

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MR. KOHRS: Jess Moore and Arnie Aldrich sign them.

MR. MOORE: And the generator for the action item provides the data for the action item. They are then reviewed by and signed off by that person. They are then signed off by the responsible project manager, and they are signed off by Arnie Aldrich at level two, and then they're presented to me and I have the final signoff on them. And that is the formal action for any signoffs that we have at this L minus one day review or the flight readiness review, as we discussed before.

We've got records of those.

CHAIRMAN ROGERS: And everyone who signed off, the only real question then was the weather?

MR. MOORE: The only real question that we had at the launch minus one day review was in fact the weather. There were no system problems identified at that time.

CHAIRMAN ROGERS: And how did you leave the weather?

MR. KOHRS: I was going to go through that next. The right chart is a couple of bullets on each of our meetings on the weather. Now, as we meet later this week or next week, we will give you a detailed briefing. We actually have charts that were presented

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at the meeting. We have the videotapes that were presented at the meeting.

But if you look at the top of the chart of the right, at that mission management team, the cold front approaching, that was forecast for low clouds and rain at launch time. Temperature forecast for launch on that day was going to be in the mid-60's at launch time. And just for information, it was also reported at that meeting that the rainfall that we had at the pad since

the rollout on the 22nd was approximately seven inches, versus a normal of two and a half inches for this time period.

DR. RIDE: Did anybody express any concern over that? Were there any systems that thought that might be a problem?

MR. KOHRS: No.

MR. ALDRICH: I would like to comment, Sally. We did discuss that throughout this period, about the amount of water that the orbiter might have picked up in terms of additional weight, and that was closely monitored, and the amount of waterproofing on the orbiter had been reviewed to show that we were well within the minimum pickup.

DR. RIDE: And I assume that all of the appropriate systems people, like SRB people, to pick a

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relevant example maybe, had heard that there was more rain than usual?

MR. ALDRICH: That was clearly reported in a very formal way, the same way we're reporting it today.

CHAIRMAN ROGERS: Had there been any evidence that on previous launches that rain had created any problem?

MR. ALDRICH: Early in the program, we had a problem with water pickup in orbiter tiles, and we have had an ongoing program of techniques to waterproof the orbiter and prevent that, both from the extra weight that was carried and from the freezing of that water in orbit, causing tiles to fracture as the freezing expands.

That has been well understood and researched through the whole flight program, and there are techniques and ways to deal with it, including the ones that were discussed here, including 51-L. And we felt that was well within bounds.

CHAIRMAN ROGERS: But that related to the tiles and not to the launch itself, or to any danger involved with the launch from the view of the amount of rainfall?

MR. ALDRICH: It related to the launch with respect to the orbiter and any related causes or dangers

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that might be involved with the launch of the flight.

CHAIRMAN ROGERS: I guess maybe let me ask the question a little differently. Did you have any previous experience with rainfall that had created any possible problem with the launch or with the O-rings?

MR. ALDRICH: I don't believe so.

MR. MOORE: I think George Hardy from Marshall Space Flight Center, I would let him comment on that.

MR. HARDY: No, we had no problems of any kind that we attached in any way to the rain.

MR. WALKER: I understood that the putty was thought to be rather sensitive to moisture and there was a concern that humidity might somehow affect the putty.

MR. HARDY: There is a procedure for handling the putty and storing the putty prior to the time that it is applied. And then of course the segments are stacked. And part of this procedure is relative to the fact that we want to minimize the moisture pickup of the putty.

The experience that we have had is that when the putty does pick up moisture that it gets tacky and sticky and softer.

DR. COVERT: Is there a seal at the bottom of the skirt, or the nozzle, rather, so that the inside of the rocket is isolated from the environment?

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MR. HARDY: Yes, Gene, there is a plug, what we call a nozzle plug, that is bonded in place in the nozzle while it is sitting on the launch pad or while it is being shipped. And it does have thermal protection system material on the bottom of it.

DR. COVERT: Thermal or humidity?

MR. HARDY: Thermal. Now, the bond, the bond itself of course forms a seal, which we think is a good seal. We don't propose it to be a hermetic seal, but we do think it is a good seal.

DR. COVERT: This is actually an insulator. When you start the main engines, with current circulating around, it can come up and cause a problem.

MR. HARDY: That is correct.

DR. COVERT: Is this an epoxy seal?

MR. HARDY: Yes, it is a epoxy seal, and that plug actually blows out when we ignite the solid rocket motors. I forget the exact pressure, but it's 15 or 20 or 30 psi.

MR. MOORE: I was going to say, Mr. Chairman, that at the time of this launch we had not—we were not aware of any water experiences in any of the joints. We are looking at the water history now for the program in a lot of detail, and we're trying to see if in fact there could have been some water in that

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particular joint.

That is one of the failure analysis scenarios that we are looking at, and we are trying to go back in the history and processing of all of the segments and retrieving of all of the segments to find out if we did see any evidence of water. I did hear a report the other day that we did see one instance potentially where we did have some water, and we are looking at that right now.

And I do not have a detailed report to offer you now, but I will tell you that my task force is really looking at that, because water in this joint in my opinion has to be looked at very, very carefully.

CHAIRMAN ROGERS: Well, I think that answers my question. In other words, you still have some suspicion that the rainfall might have affected the joint?

MR. MOORE: Yes, sir.

CHAIRMAN ROGERS: And you are studying that.

MR. MOORE: Yes, sir.

CHAIRMAN ROGERS: But at the time of the launch you had no reason to think that the rainfall in and of itself was part of the problem, going to cause a problem?

MR. MOORE: That is correct. That was my

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feeling at the time, and I will let Dr. Lucas and Arnie and the other people who sat and made the final critical decision that we had no reason to believe, other than the tile, absorption of water had anything to do with any concerns relative to the systems in the shuttle.

DR. LUCAS: Yes, I am Bill Lucas. That is correct, at the time of launch we had no evidence that water would be a problem.

In the first place, these joints are put together with very heavy grease on the tang and on the clevis side of it, and then a bead is put around the top. That is primarily to protect the joint from corrosion as you tow it back through the sea.

But we had not had any evidence that water had been captured in those joints. Since that time, since the launch, we have heard that there may have been one instance in which there was evidence of water in the joint, and we are pursuing that to see if that is in fact the case.

CHAIRMAN ROGERS: Without drawing any conclusions, of course, how far back was that experience?

DR. LUCAS: The experience that has been reported to me, which is not confirmed as far as I'm concerned, is on STS-9, which was—and this would be

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25, so that was several launches ago.

MR. MOORE: STS-9, sir, I think was launched in November of 1983, and it was the first space lab launch in the shuttle program, I believe, November of 1983, I believe.

MR. ALDRICH: Mr. Chairman, could I make one more comment about the weather, because over and over again that will come into our discussions. This mission management team focuses extensively on the weather for every launch, because of a variety of considerations with respect to the orbiter—the fragility of the tiles from ice impact, the crosswinds at our landing site, and the approaches for the landing site.

I mean, we have one here right at Kennedy adjacent to the launch pad. So we had extensive discussion of the weather and a review for the orbiter, and that is why that comes up so much in our discussion.

CHAIRMAN ROGERS: I understand that. But for our purposes, the first objective we have, or the first request by the President to this Commission, was to try to find out the cause of this accident. We have other considerations later on. But I mean, that is our first mandate or part of the mandate.

And so I am trying to focus and we're trying

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to focus on things that might relate to that. And obviously, there are a lot of aspects to the weather. The rainfall itself, there was unusually heavy rainfall, and the question I was asking, did it occur to you that that rainfall in and of itself might affect the joints?

And now of course you're telling me you didn't think so at the time, you are reviewing the previous launches to see, and there is one that might be suspicious.

DR. LUCAS: Mr. Chairman, may I clarify one aspect? And George Hardy has just reminded me that if this moisture or water was uncovered allegedly in the joint during the process of stacking, it was necessary to de-stack. The vehicle was de-stacked, one segment was removed from the other, and water was discovered and of course removed.

To our knowledge, STS-9 did not launch with any water in the joint.

GENERAL KUTYNA: Jess, one last rainfall question. How many launches experienced this much rain since the orbiter got rolled out to the pad?

MR. MOORE: I don't have the data. We will have to go back and track that data and give you a history of the rainfalls as a function of launches. Early in the program, a lot of flights set out a fairly

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long period of time and so forth, so there have been instances where we've seen a number of instances of rainfall.

GENERAL KUTYNA: The point I'm trying to make, this is not necessarily the wettest orbiter you ever launched?

MR. MOORE: Well, I don't know that, Don. I can't commit to that right now, but we are going back and researching that weather history and so forth on the systems.

MR. ALDRICH: A clarification. We reviewed how much water we thought this orbiter had picked up and we felt the pad protection, the launch pad weather protection, and the water-

proofing caused this orbiter to not be excessively heavy, and thought we had a maximum of 200 pounds, which is a low number given the total an unprotected orbiter could pick up and well within the bounds that we've accepted on other flights. And we felt it would fly with no significant effect.

MR. RUMMEL: In addition to the water question, I assume you're calculating the loads imposed on the various attachment fittings, both the shuttle and the SRB, as best you can that were experienced during this flight, is that correct?

MR. KOHRS: Yes, we are.

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MR. RUMMEL: Do those calculations take into account what has to be a very complicated structural dynamic situation between the main components? Have you found any structural deformations? Has this occurred? Is this being done?

MR. KOHRS: Yes, sir. I will show you what we call our detailed ascent reconstruction. We are probably a couple of weeks away from the detailed reconstruction of the loads that we think we saw on launch date, based on the environment, which primarily is the winds aloft environment.

The liftoff loads we got: At SRB ignition we had the strain gauge data, and the wind data we got from our wind balloons that were sent up, and that is ongoing. I will show you some charts later that will show you the wind profiles and some of the Q levels. And I have some backup charts that will show you in a preliminary fashion, for example, the strut between the SRB and the ET, what we predicted pre-flight and what we have reconstructed to date based upon the winds of the day.

And Tom Moser will also show you that.

MR. RUMMEL: Does the recorded information show the relative G loads between the shuttle and the SRB's and the main tanks?

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MR. KOHRS: I don't have it today, but the reconstruction does do that.

MR. RUMMEL: It does to that?

MR. MOORE: Yes, sir.

MR. RUMMEL: Do you have similar data on earlier flights. I'd be interested in how the structural loads might have been affected by the dynamics between the main tank and the SRB.

MR. KOHRS: Yes, sir, we have it for all of our flights up through STS-5. We had our struts instrumented, so we are actually getting measurements for that I call our OFT flights or operational flight test program. We have that data, which shows you what the interaction was for those five flights, and then we have the analytical data modeling that tells you, based upon the wind measured that day, what the strut loads and what the vehicle loads were for all of the flights.

MR. RUMMEL: You don't know yet how that came out for this flight?

MR. KOHRS: Well, on a couple of preliminary things, for example the strut that holds the orbiter—I'm sorry, the external tank, to the SRB, we have reconstructed that, and the load levels are down into like the 50 percent level.

MR. RUMMEL: Were there any unusual wind shear

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conditions?

MR. KOHRS: Yes, sir, and I have a chart here later that will show you that we did have a wind shear around the 60 second time frame. And I think what we will build during these sce-

narios is a combination of the wind shear and the other events probably were some combination to this failure scenario that is being developed.

And I will show you here what the effects were of the wind component of that. The winds, though, were based on our balloon releases. We released balloons at 13 hours before launch—actually, it started 48 hours before launch—24 hours before launch, 13 hours before launch, 7 hours, 5 hours, 3 hours, 2 hours, and then 10 minutes after launch, on this particular flight we did.

DR. COVERT: Does the balloon data correlate with the plume distortion in the 35, 40,000 foot altitude level?

MR. KOHRS: I will show you that, based upon the wind data and what the actual vehicle rates were doing, we have a delta force that we cannot account for.

DR. COVERT: But the plume data also shows a strong shear in this altitude.

MR. KOHRS: And that is the analysis still ongoing.

GENERAL KUTYNA: At Vandenberg, when we let

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our balloons up, since they go much slower than the rocket does, the darn balloon goes way the heck down range before it gets to 50,000 feet, while the rocket goes up through here. Do you have that same problem?

MR. KOHRS: We have the same problem. We have that modeled, and downrange we are looking at a device called a wind profiler, which is installed here at Kennedy, that can give you more accurate wind data. And that is ongoing.

MR. SUTTER: You know, the wind shear and wind is an instantaneous effect. What does an hour before reading do for you to tell you what kind of wind shear?

MR. KOHRS: Well, we have developed our math models based upon a lot of historical data, for every month of the year, for different times of the year. And in our wind models we put in what is called a persistence factor, that says that the wind is this at seven hours and we've got the data from the last 48 hours; mathematically, from a modeling standpoint we predict ahead, and that is why we try to launch our balloons as close as possible to launch.

Now, our data has shown—and we generate the load profiles for each of these winds, and our data has shown a pattern that pretty closely follows the last 10, 12 hours.

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MR. RUMMEL: Has there been, may I ask, any evidence of fatigue or other structural problems in the reusable part of the attach fittings during past launches?

MR. KOHRS: George, you may have to answer that for me.

MR. HARDY: I have no recollection of that. We will check that. Well, my recollection is there has been no problem.

MR. WAITE: Along that line, you were on the pad for a month, is that right?

MR. KOHRS: The 22nd to the 28th.

MR. WAITE: What load margins are represented with the weather conditions? How much does the weather put into that once it's on the pad?

MR. KOHRS: The vehicle is installed at the vertical assembly building, then rolled out to the pad.

MR. WAITE: I mean while it's on the pad, do you monitor it during that month to see what the load history is?

MR. KOHRS: No, I do not believe we do. We do not monitor the holddown posts during the time it is on the pad. We do it after rollout and then we do it prior to launch, and of course we have got the data during the launch phase.

DR. COVERT: Do you monitor loads while it is

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on the moveable vehicle?

MR. KOHRS: No, we don't.

MR. LAMBERTH: Dick, we've looked at them. I need to go back and look at the effect. And I'm Horace Lamberth, I'm sorry, Director of Shuttle Engineering for KSC. We have looked at all of the data that we took on the initial stacking, during the launch, and prior to launch.

But I will go back and see in the periods between that.

MR. WAITE: Well, is it ten percent of max load or five percent? It may be so low it's not significant.

MR. KOHRS: It is very low. We will have to get to those details.

CHAIRMAN ROGERS: Why don't you go ahead. I think we're getting ahead of you, and you've probably got other people that have information. So why don't we move along.

MR. KOHRS: Okay. If we move to the top of the right-hand chart, I've got you down to the point where we had the questionable weather, and what we did at the first mission management team meeting was decided to meet that night at 9:30 for the purpose of reviewing the weather.

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At 9:30 we went through the weather briefing. Again, here's just a real quick summary, but essentially the cold front had progressed this way, as predicted. The forecast was unchanged, and so the mission management team, with all concurrences, decided not to start the tanking, which normally would have started about 1:00 a.m. that next morning.

CHAIRMAN ROGERS: And what was—the decision was to scrub?

MR. KOHRS: The decision was not to start tanking or try to launch on the 26th.

CHAIRMAN ROGERS: Why was that decision made?

MR. KOHRS: Primarily, on the top, we predicted the multi-layer cloud decks, and in our launch criterion we have both ceiling levels for return to the launch site and we also do not want to do ascent through any rain. That is our basic criteria that told us that if we tanked—

CHAIRMAN ROGERS: So it was cloudy and rainy?

MR. KOHRS: Right. So if you come down then, we decided to come in at 2:00 p.m. here on Sunday to again review the weather, and the weather here is listed on this set of bullets. And by that time the cold front had moved into the area. The weather, though, had not deteriorated as quickly as projected the day before.

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However, the weather forecasters thought that a clearing was behind the frontal passage, and the only concern then on that day was the high forecast of surface winds and upper air winds for that day. We decided at that meeting, though, that we would go ahead and proceed with the tanking, and that was given at this time frame, and then the scheduled launch time was 9:37 a.m. for the 27th, which was a Monday.

On the 27th, we basically started the tanking at 1:00 a.m. For the 27th launch, we started the tanking at 1:00 a.m. in the morning. The ice team at 5:00 a.m. went out, like they do on all launches, to do a vehicle inspection.

And our ice team and their criteria is basically one of ice on the external tank, and primarily it is concern of ice formation that during ascent could cause debris that would impact the orbiter. And we do have a set of criteria that says in certain areas of the tank, like very forward, the criteria is no ice, and in other places of the tank you can have up to a sixteenth of an inch, and by analysis, it is areas that if it came off would not damage the orbiter.

That ice team came back at 5:00 and gave that go.

CHAIRMAN ROGERS: Can I interrupt you just a

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second? I guess then that the consideration of ice did not relate to the launch itself; it related to whether there would be damage to the orbiter. But you didn't think the ice on the external tank would impair the launch capabilities?

MR. KOHRS: Yes, sir, on this day. The next day it changed a little bit, because then we had the concern for the facility ice. And I will go through that.

MR. WALKER: Could you say a little bit about how much ice was on the external tank? Is that documented?

MR. KOHRS: That is documented. We have an ice team, and it's headed by Charlie Stevenson, who's going to be the next speaker, to show you the film. But that is documented as a record. We are documenting that data, and also I believe they take voice recorders on their ice inspections and that data—don't they, Horace?

Anyhow, that is documented.

VICE CHAIRMAN ARMSTRONG: Is this ice only—are you talking about glaze ice or are you also talking about frosting?

MR. KOHRS: We're not too concerned about frost. The areas of concern are ice that forms on

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protuberances, where we cannot have the right insulation, when we've got the expanding joints, because when the tank is loaded it essentially shrinks and some of our joints really become exposed because the tank has shrunk, and the members attaching it are changing load during the loading.

We are concerned about protuberance ice, but mainly it is acreage ice on the launch tank.

And this same team has gone out and done this inspection, basically the same people, I would say 85 percent of the same people, since STS-1, have gone out and done this ice inspection.

What happened at 9:18, we had a hatch anomaly, which Arnie Aldrich talked to you about last week. Because of that hatch anomaly we got ourselves into, we had essentially—our IMU's had come out of their realignment. That's inertial measurement units.

MR. MOORE: That's the gyros and so forth, for attitude control.

MR. KOHRS: And we have a constraint that says that they can only give you a hold time of 90 minutes. So as we were working on a hatch anomaly we had a three hour launch window. We decided to go back and do the IMU realignment, which delayed the launch to 12:37 at the close of the window.

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And at 12:36, what happened to us here is we had a launch scrub. We had a high return to launch site crosswinds, and our criteria there is not to exceed 15 knot crosswinds. Based upon that data, we decided to call off the mission for that day, and we then proceeded.

And our normal course of business as soon as we can is to de-tank the external tank of propellant, and that started at 12:41. We then decided to have another review with the team to talk about a launching attempt for the 28th. That meeting was held at 2:00 p.m. on the 27th, primarily to talk about the weather forecast, which is on the bottom chart on the right.

And here again, it was forecast continued clearing, decreasing northwesterly winds, but the temperature there was expected to be below freezing, into the low twenties in the early morning hours, primarily around 6:00 a.m., but over a period of below 32 degrees for about eleven hours.

We had a concern expressed at that meeting on our ice on the facility. That goes back to January of 1985 on the 51-C launch, where we also had a launch, I call it, a scrub, where we had below freezing temperatures and we decided not to tank because of the weather forecast, and we did launch the next day of

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51-C, the following day.

CHAIRMAN ROGERS: What does "the capability of facility" mean?

MR. KOHRS: The facility has a lot of circulating water for things like eyewashes and water spray after liftoff, where we keep that water primed ready to use. In the meeting, we decided that the way to—I'm getting a little bit ahead of myself, but we decided that that water would probably freeze and bust some lines.

So we decided to go out and do what we call a little trickle, like you do with your water faucet at home to let the water run overnight.

The bottom line there is that at that weather briefing the temperature was forecast to be near 30 degrees, and I put the actual temperatures down there. At 9:00 o'clock it was 29 degrees, and at 10:00 o'clock it was 32 degrees.

DR. RIDE: You said that before 51-C you were faced with similar weather forecasts of below freezing and you decided not to tank. This time you decided to tank. What did you learn since 51-C?

MR. KOHRS: What happened of 51-C, Sally, is we did not have the procedures in place for keeping the facility from freezing. We have a terminology, Horace,

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that is called freeze plan. For 51-C we did not have that, and the night that we had our low temperatures we actually broke pipes on the launch pad, and so we had to take the next day to really repair those pipes before we could get into the final launch count.

This method of trickling water kept the pipes from freezing. However, to lead you down that path, we gave the go for a tanking. Tanking was to start at 1:18. It was delayed because we had a launch processing system, LPS, anomaly in the flow of the vehicle.

In the meantime, this ice—

DR. COVERT: Wait a minute. That is an image I have difficulty accepting, the flow of the vehicle. Do you want to explain?

MR. KOHRS: The tanking of the vehicle, the processing flow, is what I meant to say.

DR. COVERT: The processing flow?

MR. KOHRS: The tanking flow.

DR. COVERT: So you're pumping in oxygen, liquid oxygen or something, and it's not right?

MR. KOHRS: The control system, the launch processing system, which is a computer system, had a problem with one of their control cards that would not allow us to safely tank. And so we took the time out to fix that.

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DR. COVERT: Fine, I see now.

MR. KOHRS: Which got us into about a three hour delay. So during that time frame we decided to send the ice team out at 1:30 a.m. in the morning to take a look at the facility, and at that time they reported that we had heavy ice accumulation on some of our areas, especially where we had this trickling water.

We did start tanking at 3:55 a.m., and then roughly three hours later at 7:00 a.m. the ice team went out and made their normal inspection of the vehicle, primarily concerned again with the external tank.

In the meantime, we decided that, based upon their report, we needed to have another review of the temperature data and we called a mission management team, which convened at 9:00 a.m. on the morning of the 28th to re-review the ice condition at the pad.

The concern at this meeting, though, was primarily, was the concern that ice that was on the facility during the ignition of the main engines and during the liftoff of the SRB would fall off of the pad or break loose from the facility, aspirate into the flow of the vehicle, and a potential damage to the orbiter.

We had an extensive review, and Arnie discussed it last week with the orbiter project, at this

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meeting, and after a lot of consideration, we had a go for launch on that day.

CHAIRMAN ROGERS: Is this a fair summary, then: that the ice problem was assessed in terms of the facility, whatever you call it, the capability of facility, which means the water runs in the orbiter, I guess, and you checked that out and you checked out ice conditions because you were worried about the condition of the orbiter?

Was any check made by the ice team or anybody else how the whole temperature would affect the external tank or the booster rocket?

MR. KOHRS: The external 7:00 o'clock ice team inspection was a normal inspection. They did their normal temperature survey of the external tank, and as a matter of course they surveyed the SRB temperatures.

CHAIRMAN ROGERS: What did they do in that connection, the SRB temperatures?

MR. KOHRS: They just recorded the temperatures in some locations on the SRB and the external tank.

VICE CHAIRMAN ARMSTRONG: How did they do that?

MR. KOHRS: Horace, you will have to help me on that.

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MR. LAMBERTH: Yes. The ice team that went out at this time, basically the vehicle was very clean of ice. They did it with an infrared pyrometer all the way up and down the stack, and that is just for a reference item.

DR. RIDE: You're going to go into that more?

MR. LAMBERTH: We hadn't planned on going into it today, but we would go into it later.

VICE CHAIRMAN ARMSTRONG: I think we would like to have a review of that.

CHAIRMAN ROGERS: Why don't we talk about it a little bit today, because I have trouble, if it was that cold and you knew that the orbiter—I mean, that the booster rocket might be affected by cold, why wasn't more attention paid to that aspect of it?

MR. CRIPPEN: I was just going to say that they do and did take temperatures of the solid rockets. However, they had no criteria with regard to that, and that was just a matter—I don't want to mislead you that they were looking at the SRB temperatures.

MR. MOORE: In the meantime, I guess Marshall in the Marshall projects office was looking at temperatures and so forth, and maybe Dr. Lucas can speak to that.

DR. LUCAS: Well, I can comment on that.

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There was a meeting, which we're going to go into in detail, I believe at 8:00 o'clock tomorrow, that evening which I believe went on from about 11:00 p.m. until about 11:00 a.m., at which time it was concluded that it was satisfactory for the launch, the solid rocket booster, and in terms of the forecast that we had.

DR. RIDE: What were the temperature readings on the SRB's from the IR readings?

MR. LAMBERTH: Sally, we've got those documents and we've got some discrepancies between the left and right that we are running tests on now trying to understand those readings and how much they were affected by the night sky. And we had a team out last night looking at that, and we're trying to correlate that to give the best temperature estimate we can.

The left-hand SRB read what you'd expect, in the 23, 25 degree range. The right-hand shows lower readings than that. We feel like the right-hand is somewhat lower than the left due to the night radiation, but we don't believe they're as low as some of the readings we have, and we're trying to understand those and be able to put some logic into those.

MR. MOORE: As I understand it, Horace, we did see some readings as low as, data I have heard—and I haven't seen the actual data—is down as low as 10

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degrees, is what the IR pyrometer said.

MR. LAMBERTH: Yes, we had some readings on the right-hand SRB as low as nine degrees and seven degrees on the nozzle. We do know from last night's data that that is affected some by the factor that you have a night sky looking from that side when you make those readings, and we don't think it was that low. We think it is lower than the left side, however.

DR. RIDE: Did that get fed back to Thiokol, that you saw readings that low?

MR. LAMBERTH: Sally, I'm not sure. The requirement that we had when we go out with the ice team, as Bob said, the requirement we give the ice team is to assess the pad conditions. At this particular time we were looking very heavy at the ice on the facility, the ice in the holding troughs underneath the SRB, and any other ice on the facility, as well as ice on the vehicle.

And like I said, the facility and the holding troughs had water underneath. That was our big concern. We did talk about the temperature we read in the holding troughs, ten degrees. To my knowledge, the temperatures as we read off the SRB's were not discussed at that time.

We did discuss the readings we got in the

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troughs, though.

DR. COVERT: Do the troughs see the night sky?

MR. LAMBERTH: We were reading temperatures in the trough of about ten degrees, and we were taking those basically from the same—

DR. COVERT: But the trough looks up at the night sky and it's out of the wind, so it is essentially a calm, good radiation reading?

MR. LAMBERTH: Yes. We had a ten mile an hour wind, the way we were taking the readings.

MR. SUTTER: Would the wind affect the temperature of one versus the other?

MR. LAMBERTH: The wind, by all of our analysis, the wind would be the effect that you get from the sky radiation, yes, sir.

CHAIRMAN ROGERS: Just because we are in closed session, I don't want to be unpleasant, but—and maybe I am being unpleasant, but it would seem to me that if the temperature at that time you've got down there was in the low 20's at 2:00 o'clock in the morning and it had been in

the low 20's for approximately 11 hours, and everybody knew that that would probably have some adverse effect and there were some limits of some kind about that, why that wasn't a matter of major

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concern?

I can see why your team that was primarily concerned about ice and damage to the orbiter and the facility, whether the water faucet was working and that stuff—but I would think that that would have been a major concern to everybody.

And it would be helpful in this closed session, because you're going to be struck with it in public. What was it?

MR. ALDRICH: Could I speak to that? As I mentioned, I reviewed the details of the situation with respect to the orbiter and the physical ice on the facility. At no time during this period was I aware of a concern for the temperature of the SRB within the ranges as we had from the weather forecaster. It was not known to me as a constraint on the performance of the solid rocket booster as a system or any of its elements.

GENERAL KUTYNA: But, Bob Crippen, you said that there were no criteria on temperature on any of the solids, and yet in previous testimony we heard somebody say don't launch outside of 40 to 90 degrees.

MR. CRIPPEN: I'm saying there's no requirement for us to go out and measure temperature, like with an IR gun on the solids, and play that back

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against the criteria. Yes, there is a bulk temperature requirement on the solids.

GENERAL KUTYNA: But when you qual a system, be it an airplane or be it a spaceship or whatever, and you do qual it within certain temperature limits—there was no such qual done on this and no temperature limits that these solids were qual'ed for.

MR. REINARTZ: Stan Reinartz, Marshall Space Flight Center, Projects Manager for the propulsion element that we had at Marshall.

The qualification of the motor is, yes, as Larry Mulloy, project manager stated, it is for the mean bulk temperature of 40. That was considered in the forecast that was made the night before, and we have a plot for weather, temperature, in the various conditions we could see.

And that was calculated, and it was calculated to be in the 55 to 56 degree temperature range. And when we launched that, at the time we then launched, that was what we were still predicting, in the 55 to 56 temperature for the mean bulk.

GENERAL KUTYNA: So mean bulk was the only constraint you had temperature-wise?

MR. REINARTZ: That was the constraint that we had.

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CHAIRMAN ROGERS: Do you mean you were operating under the prediction that it was going to be 55?

MR. REINARTZ: No, sir. There is in the total solid propellant, you have an enormous heat sink or mass, and during the course of the weather changes down here at the Cape that mass of the propellant changes very slowly over time. And the temperature that we had predicted, knowing the conditions that were coming in starting 48 hours ahead and then for our meeting, is we were predicting we would be at a mean bulk temperature of the 55 to 56 degree range.

We have had a previous launch that launched at 52 degrees mean bulk temperature on the previous launch, and so it was within our experience base that we had for launching the vehicle at that temperature.

CHAIRMAN ROGERS: Put it another way, then, if the temperature was 20 below or 20 degrees for eleven hours, it was thought that that would not affect the solid fuel in the booster to reduce it below 56?

MR. REINARTZ: That is correct, sir.

CHAIRMAN ROGERS: But there was no way to measure that, and so you did it on a projection basis?

MR. REINARTZ: It is analytical, based upon some early data that we had done on the propellants at

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Wasatch, where we had some instruments that measured internal temperature, and then it was done from calculations based upon those types of measurements.

CHAIRMAN ROGERS: How low would the temperature have had to be before it would have been a problem in your thinking, and how long? Suppose it was eleven hours of zero temperature?

MR. REINARTZ: It would have had to have been for several days of time.

MR. HARDY: If I could help, in the model that we have, as Stan said, the model was calibrated from instrumentation that we had on these propellants early in the program, and also from the Minuteman program, which uses essentially the same propellant.

But the model was, say, in effect it follows ambient temperature of about 20 days. That is how big the thermal mass is, the mean bulk temperature. And so to get the temperature of the propellant down to 25 degrees—and this is rough—you would have to cold soak it for, let me say, 15 to 18 or 20 days, in order to get the temperature down that low.

CHAIRMAN ROGERS: That's another way of saying that as far as, based upon the previous experience, as far as you were concerned, that the coldness of the weather really wasn't of concern as far as the solid

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fuel boosters are concerned.

MR. HARDY: Absolutely correct.

DR. COVERT: George, I have a question related to mean bulk temperature, and I understand what you're getting at here. But there's also a problem of temperature gradients, and depending upon the degree of rapidity of the change of the temperature, you could have a tolerable mean bulk temperature, but you could have fairly high temperature gradience which could give rise to anomalous effects that you've had previously.

Have you any estimate of the rate of temperature gradient?

MR. HARDY: Yes, we have analytically calculated gradient across the range for 51-L. I don't have those numbers, but this afternoon I can tell you what that was.

DR. COVERT: And I would like to see also the difference in temperature gradience between the middle of the panel and the neighborhood of the rings for where the field joints are made, because of the difference in thermal mass of the steel. Can you get that for me?

MR. HARDY: I will do that.

Let me just mention one other thing. The primary concern—And I'm sure many of you know this—on bulk temperatures in solid propellant is the strain

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of the propellant and the potential for cracking the propellant.

DR. FEYNMAN: Why is the mean an appropriate number, rather than the differences or the lowest values? Where it cracks isn't very important. The mean only tells you it doesn't crack on the average, where it could crack where it's lowest, right?

MR. HARDY: The effect of the temperature in terms of cracking would obviously be where it is lowest. The highest strains in the propellant are near the bore of the propellant. The bore of the propellant in where the mandril is. You have a mandril with the propellant around it. The highest strains are typically around the bore of the propellant.

Now, the highest strains that propellants will see in the motor are when the propellant is cured. There's a fairly rapid cooldown of the propellant under transportation conditions or under storage conditions. When you ignite the motor, then the pressure is uniformly inside the bore, pushing against the propellant. Then of course the effect of temperature strain rates at that time are much less.

DR. FEYNMAN: You mean cracking deeper in would be closed by the pressure, presumably, so when the burning got down to there it wouldn't penetrate the

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cracks and the rate of burning would be uniform, even though the material may have in fact been cracked and crumbled near the outside?

That wouldn't, presumably, be of much importance because the pressure holds it together.

MR. HARDY: That is correct. And if I could just add one other thing, one other bit of information, in any sort of failure analysis on a solid rocket motor, looking for propellant problems, propellant cracking or anything of that nature, you look in the pressure-time trace.

And if you crack propellants and increase the burning surface any significant amount at all, that will show up readily in the pressure-time trace.

VICE CHAIRMAN ARMSTRONG: Are there concerns other than cracking with the lower temperature?

MR. HARDY: Not with regards to the propellant.

DR. COVERT: Are you saying, George, no anomalous pressure-time behavior in these boosters?

MR. HARDY: We will talk about those events. There are none which we can associate with the anomalous propellant parameter.

MR. WALKER: One other question. Then there is no temperature specification or requirement on the

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steel case? Presumably the steel case follows the ambient temperatures, although in this case it was lower than the ambient in some cases.

MR. LAMBERTH: The requirement is for an operating motor at 40 degrees, the operating motor.

MR. WALKER: But the steel case is going to be colder than that.

MR. LAMBERTH: But I don't know—and we will be furnishing at the time we talk about the detailed weather discussion, we will go into the entire set of qualification data and requirements, and each piece that goes with that for the environment it is to survive and what qualification tests were done to assure that.

MR. WALKER: But what I was asking, is there a specific requirement on the temperature of the steel case, as opposed to the bulk temperature?

MR. LAMBERTH: To my knowledge, there is not a specific requirement on the steel case.

MR. WALKER: So ever though you actually measured this temperature with the pyrometer, nothing was done with that data?

MR. LAMBERTH: Yes, that is a correct statement. There is no requirement to take the measurement or to act on that data.

DR. RIDE: Let me ask that another way. I

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guess the question would really be what launch commit criteria there are related to cold temperature.

MR. ALDRICH: I was just trying to fit that in if I got the floor again, Mr. Chairman and Sally. The launch commit criteria we have for cold temperature is to launch at an ambient outside temperature at launch time of 31 degrees or greater.

Now, the prediction was for that case to be greater than that, and that in fact is how the data turned out. There are no more definitive or solid element-unique criteria specific in that.

DR. RIDE: So the assumption is that, back when the solid rocket boosters were, to pick an example, were qualified and built and certified for launch on the shuttle flight, they had to prove that if the ambient temperature was 31 degrees, all parts of the solid rocket were go for launch?

MR. ALDRICH: That would be implied, and it has been for a series of launches.

CHAIRMAN ROGERS: Without going through this whole discussion, because I know we're coming to it later, but did Thiokol—why did Thiokol advise against it the night before because of the weather, as I understood it? At least I gather that they did.

MR. LAMBERTH: Mr. Chairman, their concern

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that they raised at that time—and we will give you a full discussion of that, and Thiokol will also explain their position. But it was related to the O-rings, was the only point for consideration that they raised in that discussion on the potential lower temperature.

CHAIRMAN ROGERS: Of the O-rings?

MR. LAMBERTH: Of the O-rings, and the possibility that you might have increased erosion due to the lower temperature.

CHAIRMAN ROGERS: Well, we will come to that later.

MR. WALKER: But there is no such written requirement? That is just something they brought up and were concerned about, and it is not written down and it is not a checklist item?

MR. LAMBERTH: That was not a checklist item against the launch procedure.

CHAIRMAN ROGERS: Okay.

MR. KOHRS: I think I will get off the pre-launch and move on to the ascent time line.

GENERAL KUTYNA: Before you do, one more question on launch. What were these delays costing you in terms of either future mission processing or experiments flown on this mission? Were we losing any experiments because of these delays?

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MR. KOHRS: No, sir. We had a three-hour launch window that we had signed up with both the TDRS projects and with the Spartan Halley projects, and there was no constraint beyond the three hours.

GENERAL KUTYNA: So you weren't going to miss Halley's Comet by going beyond the 28th?

MR. KOHRS: No, sir.

CHAIRMAN ROGERS: Go ahead.

MR. KOHRS: The next chart, which is starting the time line for ascent, and let me say a few words.

(Viewgraph.) [Ref. 2/13-6]

I've listed four pages here. To start at the beginning, the main engine start command, which is 6.6 seconds before the SRB command. And I have listed the events in the center of the page, and I will try to make sure I cover all the acronyms. And I have listed over in the right-hand column in "remarks" whether the data was nominal, and the nominal data as we read it is on our telemetry data that comes down.

And then our other data which talks about this anomaly is data that we have recreated from our cameras, different camera locations. And here I've just listed camera number 60, camera number 207. Charlie Stevenson will talk next and show you those cameras and more details on their locations.

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What I will do later on, then I will bring up some charts on the right that show you what downlink or telemetry data we use to pick off these data points. And the camera data data points were picked off really from time tags and the count of frames in the cameras.

We'd look—like I said, the main engines, the SSE start command is the nominal 6.6 seconds before SRB. At about 3.7 seconds into that, the engines build up to 100 percent of rated thrust. There is a series of internal checks that say, we're ready to launch, all automatic within the computer.

And then the zero point which I've listed here, which was 11:38.0.010, is the best estimate of the SRB ignition command.

Now, at the top of the chart I've labeled this data as of 2/12/86, which is yesterday, and we're still refining the detailed times between us and all the other projects. We are within a few milliseconds on these time tags. You may see some data later that is maybe a little bit different here.

DR. RIDE: Was there anything anomalous at all within the main engines?

MR. KOHRS: No. Let me say, all of our engine data from post-flight reconstruction, all of our orbiter data from post-flight reconstruction, we don't see any

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anomalies to date.

MR. MOORE: I would qualify that to say to date. It has not been checked off the list and say it has been exonerated. It is still going on. But as of now, we see nothing anomalous in that data.

MR. RUMMEL: May I ask, do the computers check this in some way or another?

MR. KOHRS: Yes, sir. During the engine startup, the engines have their own managing controller that has a series of red line criteria built in that it has to meet certain gates, of valve openings, throttle levels, and time levels and temperature levels to proceed to launch.

If we don't pass those gates during the 6.6 seconds, we call those internal red lines to the computer. We will get what we call an automatic cutoff.

MR. RUMMEL: Let me ask it differently. Is there redundancy in the computer recording setup?

MR. KOHRS: Yes, sir.

MR. RUMMEL: Have you checked one against the other?

MR. KOHRS: We do have criteria that our redundancy has to be there at liftoff. So we have, like if you have two measurements up until the time of SRB

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ignition command, you have to have redundancy in our critical measurements.

The first movement was plus .05 seconds. The key thing here, you will see on the pictures at .445 seconds, is our first evidence of black smoke from the right-hand SRB near the aft field joint, and we will show you that coming up.

MR. SUTTER: Could you show where that is on that model?

MR. KOHRS: We think it was back in this quadrant over here.

MR. STEVENSON: That's right.

MR. WALKER: Could you point out the lower field joint?

MR. KOHRS: That's right here, this white line.

MR. LEE: This is Jack Lee. You might point out from the camera angles, from the photography we have, it's not obvious where it comes from. It is emanating between the right-hand SRB and the external tank.

MR. KOHRS: You see it right in this area in the film.

DR. COVERT: I assume you've gone back and looked at a lot of other films since then of other,

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earlier launches, and this is the only case that you've seen this black smoke?

MR. LEE: Yes, that's true.

DR. COVERT: Thank you.

MR. WALKER: Are there indeed over 200 cameras that you use?

MR. KOHRS: I think I've looked at about 82 or so films from this flight. There may be more, plus you had the TV cameras.

MR. WALKER: But the film cameras we're talking about here?

MR. KOHRS: These are 16 millimeter here, and we also have 70 millimeter.

DR. COVERT: Yes. Do you really know the framing rate to a millisecond?

MR. KOHRS: Yes. The 70 millimeter has a time tag on it. The 16 millimeter we're having a little bit of difficulty, and that is why you see some differences in persons looking at the film.

These two events you will see were abnormal. The roll maneuver was at 7.7 seconds, where the vehicle rolls. That was nominal.

If I could have the next chart up.

(Viewgraph.) [Ref. 2/13-7]

GENERAL KUTYNA: At liftoff do you measure

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loads on the holddown bolts?

MR. KOHRS: Right.

GENERAL KUTYNA: Was there anything unusual in that?

MR. KOHRS: To date, we're still developing that data and going back through what we call our liftoff load reconstruction, and that still is about a week away, a detailed analysis.

Here again I will just show you—and it is in your handouts, and I know you can't read that chart. But in the right-hand upper corner in your handout will be a time that is tagged here. And the only reason I'm pointing this out—I'm sorry, right here, 7.7 seconds. The only reason I point this out is this is where we pick off the start of the roll maneuver, is our time tag.

And I'm not going to dwell on the details of that chart, but that particular one is the roll rate gyro on the vehicle, and it is normal as predicted.

The next chart.

(Viewgraph.) [Ref. 2/13-8]

The next event is—let me talk about this one. This is an approximate. The last visual indication of the black smoke coming out of this area is in the 12 to 13 second time frame, and that is still

818

under study with different people looking at the film and looking at it with different cameras. This time I suspect we'll refine probably within the next week to get that pinned down.

The throttle maneuvers, and here it is picked off with the chamber pressure movements. This is chamber pressure of the engines, and it just shows you our nominal chamber or SSME throttle setting. We lift off at 100 percent, we throttle up to 104. As we get into our max Q area, we throttle down here to 94 percent for ten seconds, and then down to 65 percent, and then back up to the 104 percent, and fly at 104 percent for the remainder of the mission.

DR. FEYNMAN: What is measured on the vertical axis?

MR. KOHRS: That's chamber pressure, engine chamber. I'm sorry, it is chamber pressure that has been converted to throttle setting. But the reading is really PC or chamber pressure.

DR. COVERT: Which one of the engines is that?

MR. KOHRS: This particular engine is main engine 2, and they are all the same, though.

DR. FEYNMAN: In making this conversion, is it a simple mathematical formula or is it some kind of

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guess? And the reason I ask is that the lines are extremely straight and flat, and I wonder what you measured so accurately that didn't have any wiggles in it.

MR. KOHRS: These are commands that we are measuring, but it is based on the PC measurements.

DR. FEYNMAN: PC measurements are measurements of chamber pressure, measurements of a physical quantity, and there are all kinds of noises and vibrations, and it has been extracted from this?

MR. KOHRS: It has been smoothed out.

DR. COVERT: Well, there's also the zero suppress.

MR. HOTZ: Do the solid motors change thrust in synchronization with the throttling back of the main engines?

MR. KOHRS: The solid rocket motor is cast to a specific burn rate versus time, or thrust versus time.

MR. HOTZ: But does that change during the course of this?

MR. KOHRS: It does change during time, and I think last week we did show you, I think. Jud Lovingood did show you a thrust versus time for the solid rocket boosters.

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MR. HOTZ: There was some other testimony, though, that it didn't change.

MR. KOHRS: In my terminology, the confusion—there were some people thought there was some way to command to change. That is cast into the motor.

MR. HOTZ: No, I understand that. But it is a change which is roughly similar to your throttling back of your main engines?

MR. KOHRS: Yes. If you superimpose the throttle back of the SRBs, it is throttling down in the same time frame the chamber pressure is going from 900 to roughly probably 500 or 600.

MR. HOTZ: The curve that he gave us showed that, down and up.

MR. RUMMEL: On the burn rate inside the solid rockets, for clarification, I take it the motor burns longitudinally from the aft end forward, and then outward?

MR. KOHRS: No, it burns radially, uniformly throughout the length.

MR. RUMMEL: So the exposure of the hottest gases would be toward the end of the burn, then, on the various joints, is that correct?

MR. KOHRS: Yes.

MR. RUMMEL: Well, is it correct to assume

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that the exposure of the seams toward the nozzle are exposed at the same point in time as the rest of the seams or earlier?

MR. KOHRS: I would have to ask George Hardy, but I think it is uniform, I believe. Now, George will have to give you the specific answer.

MR. LEE: That is accounted for in the insulation inside the case. There's a different thickness in the aft sections, if that is the answer to your question.

MR. RUMMEL: So you take that into account by varying the insulation thickness?

MR. LEE: Yes.

MR. RUMMEL: I see. Thank you.

MR. SUTTER: I assume you are studying where did the black smoke come from?

MR. KOHRS: Right, and we're going to show you those photos, and that is still being studied. And we are also using the best enhancement techniques we have to try to pinpoint that.

MR. SUTTER: But what would make black smoke?

MR. KOHRS: That is what we're still trying to determine. There is grease in this area, but there has to be some ignition source or some temperature source for that to happen.

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(Viewgraph.) [Ref. 2/13-9]

The next chart will drop you down to the roll maneuver, and here again I'm just showing you the data we picked off of the gyro. And this event here again is completed here at 21 seconds.

I showed you on the previous chart the throttle-down to 65 percent. At around 40 seconds—during ascent we get our normal actuator movement responding to wind,

We saw a little bit more activity during that area, well within our experience base and not any concern with the loads that were created within that time frame.

The next chart, please.

(Viewgraph.) [Ref. 2/13-10]

Oh, back up one. I need to finish.

(Viewgraph.) [Ref. 2/13-9]

We did the throttle-up which I talked about, and then at 58.7 seconds was our first indication of smoke from the minus Z side, in this area of the right-hand SRB, just forward of this, we think just forward of this aft ring.

CHAIRMAN ROGERS: I can't quite see your pointer. Did the smoke come from the same place?

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MR. KOHRS: The same area, the area we are seeing the smoke up here on the films and then later at the 57 time in this area here.

CHAIRMAN ROGERS: And you're pointing to the right booster?

MR. KOHRS: Yes.

MR. HOTZ: Is this a different colored smoke you're seeing now?

MR. KOHRS: Here you're really approaching the flame and the hot spot in this area. Here you will see in the film that this is definitely what I call black smoke. I think you just need to see that in the upcoming film.

GENERAL KUTYNA: So this thing at 58 seconds should not be called smoke?

MR. KOHRS: Well, it's the first indication. In the next slide I'll put up, it shows how it progresses.

MR. CRIPPEN: But that was not black smoke you saw at 58?

MR. KOHRS: It was smoke; it was not black smoke.

The next chart, please.

(Viewgraph.) [Ref. 2/13-10]

At about 59 seconds, we hit our max dynamic pressure. And I think I should have a chart 15 on the right.

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(Viewgraph.) [Ref. 2/13-11]

Here is just a plot of recreated max dynamic pressure. The NAV derived on top is just a reference based upon the monthly mean winds. The solid line is the max pressure in pounds per square foot versus elapsed time.

And you see here we approach roughly 720 at around 59 to 60 seconds, and we have tagged it here from a detailed look at 59 seconds. At 59-1/4 you get a well-defined, intense plume, which you will see on the camera, and then the next slide will show you at 60.1 you start to get a chamber pressure divergence from the right-hand to the left-hand SRB.

The next slide, please.

(Viewgraph.) [Ref. 2/13-12]

Let me back up one. While I have this one up, I was just going to show you what the wind profile was for that day, reconstructed. The point to make on this chart is that at about 60 seconds we were also getting this change, high rapid change in the wind direction, which is in our normal design base, experience base, but this happened to occur around the same time as you see the build-up of this smoke area and plume area.

MR. WAITE: Also, max Q.

MR. KOHRS: Max Q was right around 59 seconds,

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right here. And the thing that makes max Q right here is that the vehicle is flying and this actually is a headwind to the vehicle's nominal flight path. The vehicle is trying to fly up against this wind and here, as it sees this apparent headwind to what it's trying to fly, the headwind is going to up the max dynamic pressure.

And that is really what you see on that chart I had previously. You see that hump in the change of the dynamic pressure, which is just reacting to that wind.

GENERAL KUTYNA: That is all happening, that zig-zag is happening, after you had a problem? Is that right?

MR. KOHRS: Apparently it occurred right after the first 59 seconds. This is the in-plane wind. The next chart just shows the out-of-plane component of that same velocity plot.

The next chart—

(Viewgraph.) [Ref. 2/13-13]

—which is at 60.164, and what is plotted here is the left-hand solid rocket booster and the right-hand solid rocket booster chamber pressure. And, as we discussed earlier, the chamber pressure in this time frame as you're in the SRM bucket gets down to the

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600 psi pressure level.

This difference here is not abnormal. Where you really see the right-hand SRB is it starts diverging out here, and this point is the point we picked for the 60.164 time frame.

Other people, just for background, you may see a little different time, may pick this point or that point, and you will get a few tenths of a second time difference.

MR. WAITE: Is that monitor real time?

MR. KOHRS: This data is sent down real time. It is run through our mission control center. It's processed, displayed to the flight control team at one sample per second. The data you see here is data that is recorded, that is coming down and then played back and analyzed at the higher sample rates.

I think this sample rate is probably up at the 100 samples per second. But in terms of what the person on the flight control team sees on the ground on launch day, it is displayed to them at one sample per second, and the data through the processing time is in the three- to four-second time delay, in that time frame.

DR. COVERT: And this thing really can read two psi out of 1,000?

MR. KOHRS: Yes.

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MR. WAITE: How is that chamber pressure derived?

MR. KOHRS: We actually have on each booster two or three—three chamber pressure measurements that are fed directly from the SRBs over to the orbiter and then down.

MR. WAITE: Is that a direct pressure measurement or a strain gauge?

MR. KOHRS: It's a direct pressure measurement, a transducer.

DR. FEYNMAN: What is the horizontal scale? I can't read the numbers. It goes from 55 to zero?

MR. KOHRS: That is time, sir. If you slide this chart down a little bit—to put this in English, down a little bit further, slide that right chart down if you can.

Up here I have put the time. 60.164 is this point right here.

DR. FEYNMAN: I see.

MR. KOHRS: We didn't have time to go back and put the right readable plots on there.

MR. WAITE: When do you think the control room first saw the pressure change, four seconds after that?

MR. KOHRS: I don't really think, in terms of the control room display, they—and I will have to check this—ever saw a pressure

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change. By the time they recognized that—they really didn't see it until this data was played back and looked at in detail. This type of pressure change was probably not recognized.

The next chart, please.

(Viewgraph.) [Ref. 2/13-14]

During this time frame, which is also the time we're reacting to the wind load, the max dynamic pressure, and we've got now a plume, the elevons and the gimbals are moving to react. And all we're showing here in this sequence of telemetry data from 60.2 seconds is that we've got elevon movement and actuator movement.

And I can go through these charts pretty quick. The charts on the right—or on the left, are just showing you these time points and how we picked them up. And you can see here a spike

and here a differential pressure. But you've got to look at a lot of missions to pick out that this is a change in differential pressure, this little spike right here.

The next chart, please.

(Viewgraph.) [Ref. 2/13-15]

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MR. KOHRS: This timeframe is when we started the SSME pitch variations, and you would expect the SSME to come down this line right here (indicating), and you've got a variation here which says that the vehicle is reacting to some external forces. The next chart, please.

(Viewgraph.) [Ref. 2/13-16]

DR. COVERT: Could I ask a question on the previous chart that you just had? If you look at that mean slope, and then it seems to pick up the mean slope again, so that this is probably a wind shear, and it is not probably any other anomaly?

MR. KOHRS: That, sir, is what we have to sort out, because at the same time this event is occurring, you've got this wind change. You've got a normal reaction; then we've got an external force. And what we have to do is try to separate out those two variables.

The other thing I would mention here is this wind data is based on balloon release that is in this case based upon a balloon that was released about ten minutes after launch, but it in itself is not totally accurate. So you can also have a different wind environment than we actually measured.

DR. COVERT: That is why I asked you before if you had watched the plume spreading and worked backwards.

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MR. KOHRS: We are in the process of doing that.

DR. FEYNMAN: What does "pitch" mean?

MR. KOHRS: "Pitch" is the angle from the horizontal you are changing out of plane and looking forward at the pitch plane, and the yaw plane is to the right or left.

The next chart, in the center—well, let me keep that one here. We also saw some other deviations.

DR. COVERT: I guess I really want to go back to that other chart, please.

MR. KOHRS: Could you go back to that previous chart, number 16, on the left?

(Viewgraph.) [Ref. 2/13-16]

DR. COVERT: Now my question, the real thrust of my question is: Is that sort of gradual slope down there, ignoring for a moment that little thing, is that pretty much normal?

MR. KOHRS: It is pretty much normal. The vehicle is normally trying to control through the CG as we are depleting propellant. So you're normally going to get some movement of the actuators to keep this thing trimmed out.

DR. COVERT: But if we sort of put an average on this, the points that are labeled 07-001 are within a

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fraction of a degree of where they would have been normally?

MR. KOHRS: Right.

DR. COVERT: Thank you.

MR. KOHRS: The next chart on the left, and here again let me pick up.

(Viewgraph.) [Ref. 2/13-17]

The next chart on the left—and I need a new chart in the middle, chart 9 in the middle—

(Viewgraph.) [Ref. 2/13-17]

Here at about 72 seconds, 72.141 and 72.661, looking at the gyros we're seeing a rapid change acceleration, which means we're getting a side force on the vehicle. Y acceleration, this is the Y direction out the side of the vehicle, so the vehicle is moving on one side in this case, as compared to the other. This is at 72 seconds. At 72.4—we also had a tracking data relay system on board, which has its own gyros.

That data was telemetered to the ground. We have gone back and looked at that data, and it confirms that it matches the gyro data that we were seeing in the orbiter. And there is nothing anomalous with the TDRS. It just is another data source that says that it matches, and we happened to lose this data at 72.4 seconds.

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I pointed out the lateral Y acceleration. Here we see the MPS, which is the main propulsion system, LOX, LO2, and hydrogen inlet pressures drop slightly.

DR. RIDE: Could you go back to the 66 seconds? Were you going to say anything about the LH2 pressure?

MR. KOHRS: I wasn't going to say anything, Sally, other than we see a slight drop in the LH2 pressure. I did not put a chart in this. Well, I guess I did put a chart. It's really 66.484. It's really figure 23 that shows that deviation.

(Viewgraph.) [Ref. 2/13-18]

And if I could, I could put chart—here it is over here. By the way, we have three LH pressure transducers that drive three, or provide information to three flow control valves in the orbiter. What you are seeing here is (ullage) they track each other pretty well, and then you get down to this point and you start to see a deviation. And this one is SSME engine number one, and these are engines two and three, which are still tracking each other.

DR. FEYNMAN: By ullage pressure do you mean the pressure above the fuel, or the oxidizer?

MR. KOHRS: Yes, sir.

DR. COVERT: I guess, sir, I don't want to

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argue with you, because you know more about this than I do, but if I just take my pen here and try to see what the discrepancy in these two lines are, the difference persists pretty much across the whole chart. It looks closer because of the steep gradient, but if you just sort of measure and count the number of dots involved——

MR. KOHRS: These are very subtle changes, and what we have looked for are anything that is abnormal. We may find in later analysis that you are indeed right, and this is normal.

DR. COVERT: I'm not saying it is normal. What I am objecting to is that your arrow is located there, where I could locate that arrow anywhere on this chart.

MR. KOHRS: I would accept that.

MR. ACHESON: Is your lateral movement in the direction you would expect it to be if it were driven by a combustion leak back there?

MR. KOHRS: Tom Moser I think will have some charts later that will take this data and build you a failure scenario based upon this data. I'm trying to present the data in a cut at the time line of these major events.

MR. MOORE: Well, really, the answer to your question is, yes.

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(Viewgraph.) [Ref. 2/13-19]

MR. KOHRS: Following on this chart is the TDRS data. We had the chamber pressure drop which showed up on the other chart in this timeframe. If I could have the other chart, I think it would just—I'm sorry, it is not the next chart. These last ones we essentially had based all of our data and timing on the camera locations. And you will see these cameras coming up. Chart 10 in the center.

(Viewgraph.) [Ref. 2/13-20]

Turbine temperature. That is just the pitch rate divergence. The other thing that we found—and this data is a couple of days old—as we looked further at our down list of data, is that the main engines, the SSMEs, were approaching their redline limits for the high pressure fuel turbopump. The redline limit I think in this case is about 1960 degrees.

DR. COVERT: This is essentially the turbine temperature?

MR. KOHRS: High pressure fuel turbopump discharge temperature. That is what the HPFT stands for, high pressure fuel turbopump redline temperature. And then later where we think—and this data is still being reviewed at this timeframe—we actually had a shutdown of SSME engine number one. At 73.605 was our last validated data. And if I had the next chart, I

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could show you—and that is a very subtle change. I need chart 29 up on the right.

DR. COVERT: Is the number one engine on the right?

MR. KOHRS: That's the top one. Here again are the RCS jets, which are the reaction control system jets, which are located up here on the vehicle, aren't firing during this timeframe. They do have pressure measurements. So when we do fire them, we see the pressure in the chamber. And the only thing that is happening here is normally during ascent, the chamber pressure should just be dropping from 14.7 atmospheric down to essentially a zero pressure.

(Viewgraph.) [Ref. 2/13-21]

Now in this timeframe, which is noted over here, you should see just before the loss of data we can see a buildup that says that it was seeing some change in pressure up in this area, probably from the plume or the explosion, and that was our last validated data. And a few milliseconds later was our last data frame. And then on the pictures at 76.425, we saw the drogue chute coming out of the right-hand SRB, and you will see that. And then at 109 seconds, the right-hand SRB range destruct system destructed that booster. And then at 110.2 seconds, the range safety system destructed the

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left-hand booster.

MR. CRIPPEN: Dick, just so people don't think there were anomalies. You were talking about the main engine temps getting high. That was explainable.

MR. KOHRS: That was a reaction to the event. We feel that that was the reaction that we were loosing LOX or hydrogen to the pump, and it was overtemping.

GEN. KUTYNA: Where were those boosters at impact?

COLONEL LINDSAY: Their normal impact area is about 140 knots or about 90 miles offshore. So a normal flight would carry them about 140 knots.

GEN. KUTYNA: Where were these going to go that caused you to blow them up?

COLONEL LINDSAY: I don't know. When the explosion occurred, all data was lost. The next set of information we have was visual from the videos that we displayed, and you were able to see, depending on which video you were looking at, one propulsive SRB coming out of the cloud, and later you could see on other screens, too, and displaying some erratic flight as far as we could see.

DR. COVERT: You could see some tumbling of the right-hand booster?

COLONEL LINDSAY: I wouldn't call it

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"tumbling." It was fishtailing.

DR. COVERT: Did you have enough time to actually see it come back? Or just veering off?

COLONEL LINDSAY: No, you could see it do a turn.

GEN. KUTYNA: I think there was some insinuations that if we hadn't blown those things up we would have had wonderful data to retrieve.

COLONEL LINDSAY: I doubt that. One motor, the one that you will see later, the right motor that we have the best data on was still climbing at 122,000 feet when it was destroyed. Estimates on its impact would be very significant damage, maybe fracturing when it impacted on the water, and, if there was propellant left in it, probably detonation of some percentage of the explosive content.

MR. MOORE: Don, we do have a detailed presentation on range safety, if the committee would like to look at it. We are planning to cover some of that.

GEN. KUTYNA: There was an article in The Post that, gee, if the Air Force hadn't destroyed these things, we would have had wonderful data.

MR. KOHRS: This was the last timeline chart. What I put together on my last chart is kind of an

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observation chart of this ascent timeline, which is on chart 30. But basically, as I said earlier, the orbiter data and the main engine data, up until the start of this event, all appeared nominal. And the areas that we are concentrating on in our further analysis are the photoanalysis of the SRB and the external tank.

DR. FEYNMAN: You say "nominal" up until when?

MR. KOHRS: Up until we started to get these divergences. That was about 59 seconds, in that timeframe. And the last chart, chart 30, kind of summarizes that.

(Viewgraph.) [Ref. 2/13-22]

The only thing, we talked earlier about the angle of attack profile, critiques this deviation, based on this wind shear we had, but when we try to reconstruct it, we cannot totally reconstruct it. We have got some external force which undoubtedly is this plume, and we've got some uncertainty in our wind profile. That is what we will be refining within the next weeks to give you a better backout of the effect.

And the other point is that our wind load indicators, which are the measure of the stress that the vehicle is seeing during ascent, which we have done for these load indicator routines during all our flights, and in this particular flight our highest

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indicator, which happened to be a wing indicator in this case, was 87 percent. And the SRB indicators, which are really the indicators that we developed back where the tank is tied to the vehicle, we were down in the 30 percent of their limit load capability as measured against their load capability.

MR. RUMMEL: Again, Are the limit loads the maximum computed applied loads? Is that correct? As opposed to design loads, which would then have the load factor applied? Can you just clarify what the 30 percent is of here?

MR. KOHRS: It is 30 percent of a factor of safety of 1.4. That's the way I would say it.

MR. RUMMEL: Are those factors applied to what we would normally call the design loads? That is, the actual load plus a load factor? I notice there is a load factor of 3 someplace.

MR. KOHRS: In some cases—and, George, you could help me on the SRB on the load factor.

MR. HARDY: I'm not sure I understand the question.

MR. RUMMEL: Well, I'm trying to understand what these percentages are. Is it the design load, the maximum load? Or, is it what is called the applied load? Or is it a limit load? What is it?

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MR. HARDY: As best I recall, it is the design load.

MR. RUMMEL: So you say it is of the computed maximum load to which is applied the load factor, to which then is applied the safety factor which is 1.4. Is that correct?

MR. HARDY: I believe that is correct.

MR. RUMMEL: Thank you.

GEN. KUTYNA: Jess, 87 percent shouldn't give you any problems or concern. You've been there before.

MR. KOHRS: That is well within our base.

MR. MOORE: We've been above 87 percent many times.

MR. KOHRS: This actual trajectory for that day was what we call a benign trajectory in a load sense.

Mr. Chairman, that's all the charts I had.

CHAIRMAN ROGERS: You were mentioning you would have some of this data completed in two weeks. Is that about what you are estimating?

MR. KOHRS: Well, here again what we call our ascent trajectory reconstruction, and part of that takes you back to the liftoff loads and gets you all the way through the completion of the events, we should have a pretty good, what I call a preliminary cut, in the next

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couple of weeks. The final cut is probably in a month's timeframe to reconstruct all of this.

CHAIRMAN ROGERS: Does that include everything? In other words, will all of the collection of material and data and photographs be completed within a month?

MR. KOHRS: I can't answer to photographic. I'm talking about the flight telemetry data.

CHAIRMAN ROGERS: I'm not trying to hold you to it.

MR. MOORE: We're going to show you tomorrow, Mr. Chairman, what we think the analytical and testing process associated with our task force is going to take in terms of time. I think from a trajectory reconstruction and so forth, we hope to be able to get everybody agreed and in sync on the details of what happened sometime within the next couple of weeks.

I think with respect to the photography data, it depends upon what we find in that data. I mean, it may take us several additional weeks once we have identified an area to go back and get the best experts in this country to go and take a look at those photographs, enhance them and do digital analysis on the photographs. So that is kind of a to-be-determined with respect to the photographic data.

With respect to the data of the testing and

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trying to re-create the set of circumstances on this thing, that will take a fairly long time.

CHAIRMAN ROGERS: I wasn't asking about that. I was asking about the analysis of the telemetry.

MR. MOORE: I think we are in what I would say, and anybody on our team can comment on it, I think we are in reasonable agreement about the major sequence of events. We are probably still off in terms of the precise times by a few-tenths of a second, or milliseconds, and so

forth, which we have not exactly pinned down yet. And based upon Dick's assessment, a couple of weeks is probably a reasonable time to expect us to be in fairly good agreement in terms of a timeline.

CHAIRMAN ROGERS: As you may know, I am going to have to testify Tuesday before the Senate Committee, and it would be helpful if I could make some general comments about it, to say that our review with you—

MR. MOORE: I'm going to be there, as well.

GEN. KUTYNA: Jess, I asked the question about liftoff loads because I've gotten through the back channel that there were some. You're not going to have that data for a few weeks, but do you have any preliminary that there might have been some liftoff loads that were unusual?

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MR. MOORE: I will ask my Cape friends or Marshall friends, and so forth. I have not seen any done at this point in time.

MR. HARDY: We don't have the liftoff loads totally reconstructed. We're very interested in those loads, and we are working with Dick and his people to reconstruct those. I believe that that is somewhere in the neighborhood of a week or so away.

GEN. KUTYNA: But how about first-look?

MR. KOHRS: We have not seen anything. There are a few things that the guys are double-tracking to make sure that they've got the right timing, and the liftoff loads are a function of the liftoff time. As the vehicle bends over and is coming back, essentially we are minimizing the loads at liftoff, and there is some disagreement on the actual liftoff time. I showed on my chart 6.6 seconds SSME start T-06.6 seconds later. The guys that have looked at the strain guage data to date, and have just started looking at this, are about .08 to maybe a tenth of a second in disagreement that they are still trying to resolve. And to say we've got a problem today would just be conjecture until we get that detailed analysis. But I've had that same input and guys are working on it with all the priority that can be put on it.

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MR. MOORE: Where is that work going on? At JSC?

MR. KOHRS: At JSC in conjunction with Marshall.

MR. MOORE: We would be happy to provide the status of what we have right now, Don.

GEN. KUTYNA: Well, Jess, what I'm saying, I don't want the Commission to be surprised that, oh, you didn't tell us something. Because there's word going around that, hey, there was something squirrely about the liftoff loads. So, for the record, you are looking at it. Maybe there was, maybe there wasn't?

MR. MOORE: I had not even heard that. So I think you are using words I haven't heard yet.

MR. KOHRS: I personally did not get that until last night, but we are working on it.

DR. COVERT: Could I ask Dr. Williams a question, please? Walt, if I recall correctly, one way to get an increase on the turbine temperature on the high pressure fuel turbopump would be to reduce oxygen flow. Is that right?

MR. WILLIAMS: No. You reduce hydrogen, if you go with more oxygen and less fuel.

MR. CRIPPEN: Which is what we were showing.

DR. COVERT: I just want to be sure my memory of

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that was correct.

MR. ACHESON: I don't need a long presentation on this, but just a clarification. What is destroyed in this destruct case that is not destroyed in the normal flight termination of the boosters?

MR. MOORE: Are you familiar enough with the range safety, Dick? Maybe we could get George Hardy up here to clarify what we think is destructed when a range safety device is fired. George, why don't you go illustrate.

MR. ACHESON: But it did come down on the chute? Is that correct?

MR. MOORE: No. The drogue chute did come out, but it did not come down on the chute. Following that, the range safety destruct sent commands up to destruct the vehicle. So it did not come down on the chute.

MR. HARDY: Well, as you well know, the booster is built in what are referred to as the four field segments, and then we have a forward skirt, and then the frustum, which houses the recovery system. And if I could, I would take just a few seconds to tell you how the normal recovery works. In a normal recovery after separation the recovery system is armed, and then, after it times out during the fallback phase, the pyrotechnics would jettison the nose. That pulls the pilot chute out,

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which employs a drogue chute. That is at roughly 16,000 feet. And at about 6,000 feet, we pyrotechnically jettison the frustum which allows then the drogue chute to pull out the main chutes and the booster falls back into the water impact. It stabilizes very quickly under the three clustered main chutes, and it falls back in tail-first.

The range safety destruct obviously for a normal flyback is not actuated at all. The range safety destruct system is a linear-shaped charge. It is configured to concentrate the jet of the explosion, and it covers a portion of this forward segment, and then the forward center section, we call it, and most of the aft center segment. It terminates, as I recall, about 18 to 20 inches above the field joint here, and it is not on the aft segment at all. It is designed such that under pressure it will simply open up the case. It opens up the case, and the pressure will drop, and you lose all propulsive capability.

MR. RUMMEL: Is there a notch in the case, or a seam of any kind?

MR. HARDY: No, sir. It has been qualified that it will fire through the full case thickness.

MR. CRIPPEN: George, excuse me. I think maybe there's one other point. Someone correct me if

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I'm wrong. I think some people thought that if we had not destructed on the range safety system, that we perhaps would have gotten a normal separation sequence which would put the chutes out, and it would have deployed the SRBs in the water.

These SRBs tore off of the system, and consequently they never received the normal set sequence that they would need to activate the system. So in fact by shutting down the thrust, it may have kept them from going higher and falling further.

MR. ACHESON: Parts of the booster that are valuable for evidence examination after the fact would have been lost in the destruct mode?

MR. MOORE: Well, all of—the key element that we are after in this thing is the apparent leakage in the field joint down there, and we do not think that is destructed. We think that field joint was not impacted by range safety, and we have got some sonar data that says that we have located the righthand solid rocket booster, and so forth, which will be extremely valuable evidence to us in terms of going back and fitting some of these failure scenarios.

We plan to talk to the Commission this afternoon about what we have located on the bottom, but we do think we have the righthand aft case segment

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intact enough to allow us to get some physical evidence back from the recovery, which is of paramount importance.

CHAIRMAN ROGERS: On that point, in the NBC report we heard just as we got on the plane that there was an official of NASA that said that you were 95 percent certain of what the cause was. Was that just a leak from somewhere? Was that a newspaper rumor?

MR. MOORE: I would certainly categorize that as a newspaper rumor.

CHAIRMAN ROGERS: You heard it, I assume?

MR. MOORE: I did not hear it.

CHAIRMAN ROGERS: It was the lead story on NBC that said a high official of NASA—now I think I'm an expert on leaks.

(Laughter.)

CHAIRMAN ROGERS: I'm just asking, as far as you know it was not a high official?

MR. MOORE: No, sir. We stand on the statements we made the other day. We do not have any cause pinned down at this point in time.

CHAIRMAN ROGERS: I'm not talking about these kinds of leaks. I'm talking about newspaper leaks.

(Laughter.)

COLONEL LINDSAY: Let me add one thought on

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the destruct mechanism on the solid rocket boosters. The linear-shaped charge merely opens the case. It is designed to penetrate the steel case, and then it opens like a clamshell to instantly vent and go to zero thrust, so there is no further propulsive power. And that last segment did not have, by design, the linear-shaped charge.

MR. SILVEIRA: By stopping the burning at that time, you probably stopped the hot gases if you had any leak there, so you probably preserved the damage in the state we would like to look at it, rather than damaging it any further. The range destruct was probably a help rather than a hindrance.

DR. COVERT: Well, it has a potential, but it may well, when it hits the water, break apart. I agree that it has that potential.

DR. FEYNMAN: How are these things made? If you wanted to put out a chute, do you have to have the signals received by a radio that is located somewhere else, and it is wired across? But if you want to destroy it, it has its own receiver? How does it work?

MR. SILVEIRA: The normal chute functioning——

MR. HARDY: Well, I will talk to the chute functioning, and you might talk to the range safety. But as Bob Crippen mentioned, as an interlock to make

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sure that we don't get any separation during the boost phase, which would not be healthy, we require two signals from the orbiter, two independent signals, what we call fire one and fire two, and those are the signals to separate the booster. And until that has happened, we have an interlock such that there is no power on the recovery system. The recovery system has its own battery power, and there is no power on the recovery system at all. When separation signal is received, those two separate signals, the power is—the bus is powered up by the battery; the switch is closed, so that you've got bus power; and then it is a combination of timing, timing

circuits, and altitude detection from the barrel switch that's sequenced, in the remaining things that I just described in the recovery system.

The range safety system does have its own receiver.

DR. FEYNMAN: Located on?

MR. HARDY: It's located on the booster. There is one on this booster. There is one on this booster. But they are redundant. Let me take that back. They are redundant on this booster, and they're redundant on this booster, and they are also on the external tank. And they are cross-strapped, so that if you had any occasion for destruct with the vehicle

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intact, a signal received from either one of two receivers here, or either one of two receivers here, or the one receiver here would effect destruct.

After the booster is away, it has its own battery-powered redundant receivers.

DR. FEYNMAN: Thank you.

MR. SILVEIRA: Don't forget the separation motor sequence two.

MR. HARDY: In a normal separation, when the solids—when the separation signal is received, it is received to fire an explosive bolt here (indicating), and an explosive bolt in each of the three struts here. Those are redundant signals. Each end of the bolt has a pyrotechnic device to set it off, and that also signals ignition of the separation motors. There are four down here on the bottom, and there are four up here on this frustum.

So all of that, timewise, is essentially simultaneous.

COLONEL LINDSAY: The destruct commands, the commands you pray you never have to send, in the case of the shuttle are through an encoding device that is built and provided to us by the National Security Agency. And each mission has a different code, and it is completely independent of the shuttle system. It is separately

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transmitted and received.

MR. SMITH: As host, could I suggest we break for a few minutes? Lunch is outside.

MR. MOORE: Before we do, let me discuss with the Chairman his druthers this afternoon. We've obviously spend a lot of time on this part, and there are also going to be a lot of very interesting things this afternoon. Is your cutoff time somewhere around the 5:00 p.m. time-frame? Is that something we should target for?

CHAIRMAN ROGERS: Well, I think, myself, and any other members of the Commission could speak for themselves, but I think we should do as much as possible today, and I think we ought to cut down the social activities, although it is nice, but it is so vital that we know about all of this as quickly as possible. I mean we are here to work and not to enjoy ourselves.

MR. MOORE: You are getting the latest data information that we have from all our teams.

CHAIRMAN ROGERS: That is vitally important, and that is what we are here for. I would like to work as late as we can stand it.

MR. MOORE: We will proceed with the agenda as we have it right now.

MR. SMITH: Lunch is self-service outside for

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everyone. And, please, eat off your tray so we can just take the trays back out and pick it up in a hurry.

(Whereupon, at 1:05 p.m., a luncheon recess was taken, to reconvene at 1:45 p.m., this same day.)

STS 51-L

PRE-LAUNCH/ASCENT TIMELINE

D. KOHRS
2/13/86

[Ref. 2/13-1

OUTLINE

0 PRE-LAUNCH TIMELINE

0 WEATHER SUMMARY

0 ASCENT TIMELINE

[Ref. 2/13-2]

PRE-LAUNCH TIMELINE

8/26
△ ET ON DOCK KSC

11/11
△ ORB ON DOCK KSC

12/4----12/10
△ SRB STACKING

12/10
△ ET/SRB MATE

12/9
△ SPARTAN HALLEY INSTALLATION

12/16
△ ORB/ET MATE

12/22
△ TRANSFER/MATE PAD B

12/24------1/3
△ HOLIDAYS

1/5
△ IUS/TDRS INSTALLATION

1/8
△ TCDT

1/16
△ HYPER LOAD

1/17
△ HPU HOTFIRE

1/23
△ T-43 HOURS

FLIGHT READINESS REVIEW HELD ON JANUARY 15

[Ref. 2/13-3]

STS 51-L

TERMINAL COUNT

1/23 (10 AM EST)
△ T-43 HOURS

1/25 (11 AM EST)
△ L-1 MMT - QUESTIONABLE WEATHER FOR LAUNCH ON 26TH

1/25 (930 PM EST)
△ MMT - WX SCRUB

1/26 MMT (2 PM EST)
△ MMT - GO FOR TANKING/LAUNCH ATTEMPT FOR 9:37 AM, 1/27

1/27 (1 AM EST)
△ START ET TANKING

1/27 (5 AM EST)
△ ICE TEAM GO FOR LAUNCH FOR 9:37
△ 9:18 AM EST - HOLD FOR HATCH ANOMALY
△ 10:31 AM - IMU REALIGNMENT
△ 12:36 PM SCRUB DUE TO RTLS X-WIND EXCEEDANCE > 15 KTS
△ 12:41 PM - START ET DETANKING

1/27 (2 PM EST)
△ MMT - GO FOR TANKING/LAUNCH ATTEMPT FOR 9:38 AM, 1/28

1/28 (1:18 AM EST)
△ TANKING DELAYED - LPS ANOMALY

△ 3:55 AM - START TANKING - LAUNCH DELAY TO 10:38 AM
△ 1:30 △ 7:00 △ 11:00 - ICE TEAM ASSESSMENT

1/28 (9 AM EST)
△ MMT - TEMPERATURE REVIEW

△ 11:38 AM - 51-L LAUNCH

[Ref. 2/13-4]

STS 51-L

WEATHER SUMMARY

0 L-1 MMT AT 11 AM EST 1/25

- o MAJOR FEATURE WAS APPROACHING COLD FRONT
- o FORECAST MULTI-LAYER CLOUD DECKS, RAIN SHOWERS (NO GO)
- o TEMPERATURE FORECAST - MID 60's
- o RAINFALL SINCE ROLLOUT TO PAD - APPROXIMATELY 7 INCHES vs 2.5 NORMAL
- o QUESTIONABLE WEATHER-RECONVENE AT 9:30 PM EST TO STATUS COLD FRONT PROGRESS

0 9:30 PM MMT ON 1/25 FOR 1/26 LAUNCH

- o COLD FRONT HAD PROGRESSED THROUGH MOBILE (30 KTS MOVEMENT)
- o FORECAST UNCHANGED
- o DECISION TO SCRUB

0 2:00 PM EST MMT ON 1/26 FOR 1/27 LAUNCH

- o COLD FRONT HAD MOVED INTO AREA, WEATHER HAD NOT DETEORIATED AS QUICKLY AS PROJECTED
- o FORECAST CLEARING BEHIND FRONTAL PASSAGE
- o CONCERN ON SURFACE WINDS AND UPPER AIR WINDS
- o AT NOMINAL T-0 SURFACE WINDS AND UPPER WINDS ACCEPTABLE
- o 12:37 PM EST - SCRUB DUE TO EXCESSIVE RTLS CROSSWINDS (15 KTS)

0 2:00 PM EST MMT ON 1/27 FOR 1/28 LAUNCH

- o FORECASTED CONTINUED CLEARING, DECREASING NORTHWESTERLY WINDS
- o TEMPERATURE EXPECTED TO BE BELOW FREEZING (INTO LOW 20's) FOR APPROXIMATELY 11 HOURS
- o CONCERN EXPRESSED ON ICE, CAPABILITY OF FACILITY
- o TEMPERATURE AT BEGINNING OF WINDOW (0938) EXPECTED NEAR 30°F (ACTUAL 0900 - 29°F, 1000 - 32°F)

[Ref. 2/13-5]

(AS OF 2/12/86)

STS 51-L

ASCENT TIMELINE

		REMARKS
L-6.6 SECs	SSME START CMD	NOMINAL
- 0.0 (11:38:00.010 EST)	SRB IGNITION CMD	NOMINAL
+ 0.0587	FIRST MOVEMENT	NOMINAL
0.445	FIRST EVIDENCE OF BLACK SMOKE RH SRB NEAR FIELD JOINT	CAMERA 60
1.606	BLACK SMOKE DARKEST	CAMERA 60
2.147	BLACK SMOKE EXTENDS HALFWAY ACROSS RH SRB	CAMERA 60
7.724	ROLL MANEUVER INITIATION	NOMINAL
~ 12-13	LAST VISUAL INDICATION OF BLACK SMOKE	CAMERA 217
20.084	START SSME THROTTLE DOWN TO 94%	NOMINAL
21.124	ROLL MANEUVER COMPLETED	NOMINAL
36.084	START SSME THROTTLE DOWN TO 65%	NOMINAL
40.0	VEHICLE RESPONDS TO WIND	NOMINAL
52.084	START SSME THROTTLE UP TO 104%	NOMINAL
58.774	FIRST INDICATION OF SMOKE FROM -Z SIDE OF RH SRB FORWARD OF AFT ET ATTACH RING	CAMERA 207

[Ref. 2/13-6 1 of 4]

(AS OF 2/12/86)

ASCENT TIMELINE (CONT'D)

		REMARKS
59.0	MAXIMUM DYNAMIC PRESSURE	NOMINAL
59.249	WELL DEFINED INTENSE PLUME ON SIDE OF RH SRB -Z DIRECTION	CAMERA 207
60.164	SRB CHAMBER PRESSURE DIVERGENCE (RH FROM LH)	TELEMETRY DATA
60.600	EVIDENCE OF FLAME FROM RH SRB IN -Z DIRECTION	CAMERA 206
62.484	RIGHT HAND OUTBOARD ELEVON HINGE MOVEMENT SPIKE	TELEMETRY DATA
63.924	RIGHT HAND OUTBOARD ACTUATOR ΔP CHANGE	TELEMETRY DATA
64.604	START OF VEHICLE PITCH RATE CHANGE	TELEMETRY DATA
64.937	START SSME PITCH VARIATIONS	TELEMETRY DATA
65.404	END OF VEHICLE PITCH RATE CHANGE	TELEMETRY DATA
65.524	LEFT HAND OUTBOARD ACTUATOR ΔP CHANGE	TELEMETRY DATA
66.174	BRIGHT SPOT ON RH SRB IN PLUME IN -Z DIRECTION START OF BRIGHT SPOTS ON +Z SIDE	CAMERA 207
66.484	ET LH_2 ULLAGE PRESSURE DEVIATION	TELEMETRY DATA
66.625	BRIGHT SUSTAINED GLOW ON +Z SIDE OF RH SRB	CAMERA 207
67.650	APPARENT MERGING OF PLUME	CAMERA 207

[Ref. 2/13-6 2 of 4]

(AS OF 2/12/86)

ASCENT TIMELINE (CONT'D)

		REMARKS
67.684	MPS LO_2 INLET PRESSURE RISE RATE DECREASED	TELEMETRY DATA
72.141	VEHICLE LATERAL +Y ACCELERATION 0.227G	TELEMETRY DATA
72.201	START OF DIVERGENT -YAW RATES RH vs LH SRB	TELEMETRY DATA
72.281	START OF DIVERGENT + PITCH RATES RH vs LH SRB	TELEMETRY DATA
72.40	LAST TDRSS DATA	TELEMETRY DATA
72.661	VEHICLE LATERAL -Y ACCELERATION 0.254G	TELEMETRY DATA
72.884	START MPS LO_2 AND LH_2 INLET PRESSURE DROPS	TELEMETRY DATA
73.044	RH SRB CHAMBER PRESSURE 24 PSI LOWER THAN LH (5 SIGMA)	TELEMETRY DATA
73.175	SUDDEN CLOUD ALONG SIDE OF ET	CAMERA 207
73.200 73.201	FLASH FROM REGION BETWEEN ORBITER AND ET LH_2 TANK	CAMERA 207 CAMERA 206
73.226	EXPLOSION NEAR SRB FORWARD ATTACH	CAMERA 206
73.326	INCREASED INTENSITY OF WHITE FLASH IN REGION OF ET LOX TANK	CAMERA 206

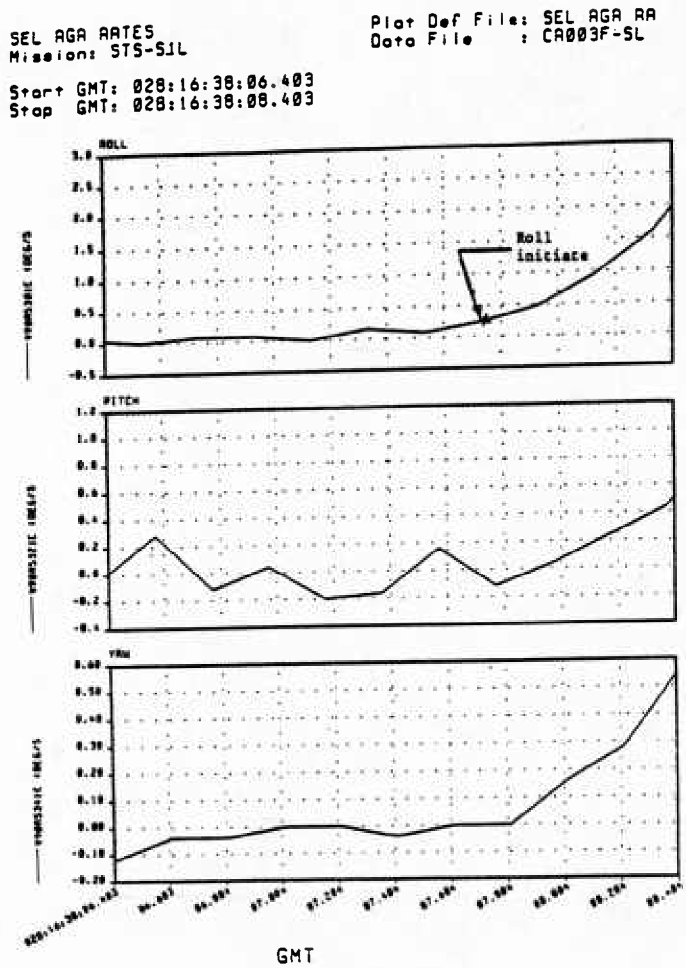
[Ref. 2/13-6 3 of 4]

(AS OF 2/12/86)

ASCENT TIMELINE (CONT'D)

		REMARKS
73.399	SSMEs APPROACHING HPFT REDLINE LIMITS	TELEMETRY DATA
73.473	ORBITER RCS CHAMBER PRESS MEAS FLUCTUATIONS	TELEMETRY DATA
73.534	SSME NUMBER 1 SHUTDOWN DUE TO HPFT DISCHARGE TEMPERATURE EXCEEDANCE	TELEMETRY DATA
73.605	LAST VALIDATED DATA - ORB RCS PRESSURE	TELEMETRY DATA
73.621	LAST DATA FRAME	TELEMETRY STOPS
76.425	RH SRB NOSE CAP SEP/DROGUE CHUTE DEPLOY	CAMERA 206
109.604	RH SRB RSS DESTRUCT	CAMERA 230
110.266	LH SRB RSS DESTRUCT	CAMERA 201

[Ref. 2/13-6 4 of 4]



Vehicle roll program initiate.

[Ref. 2/13-7]

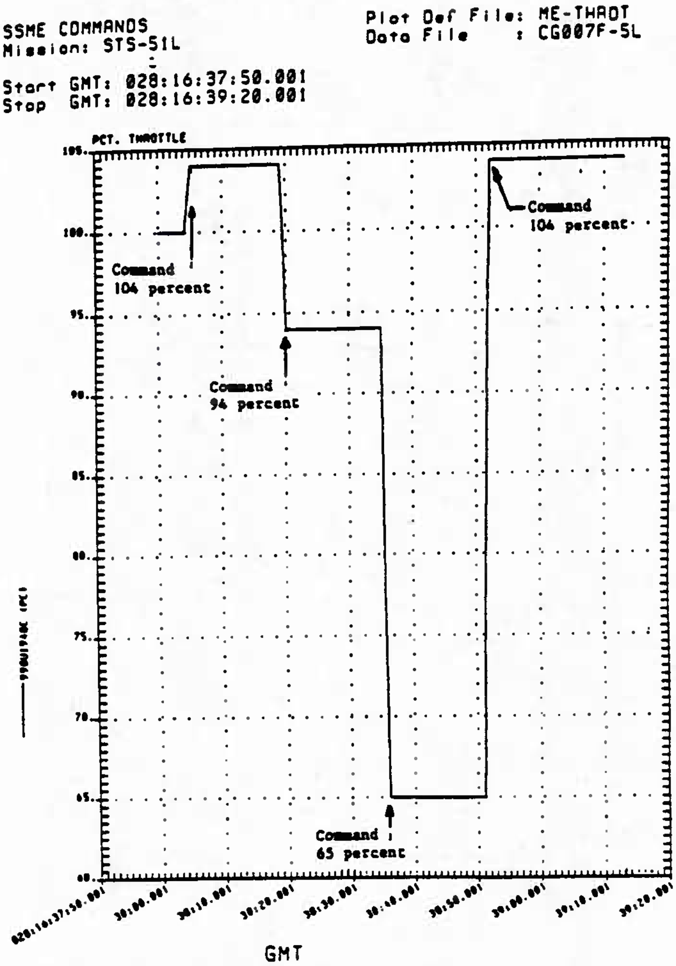


Figure 8.- SSME engine throttle commands.

[Ref. 2/13-8]

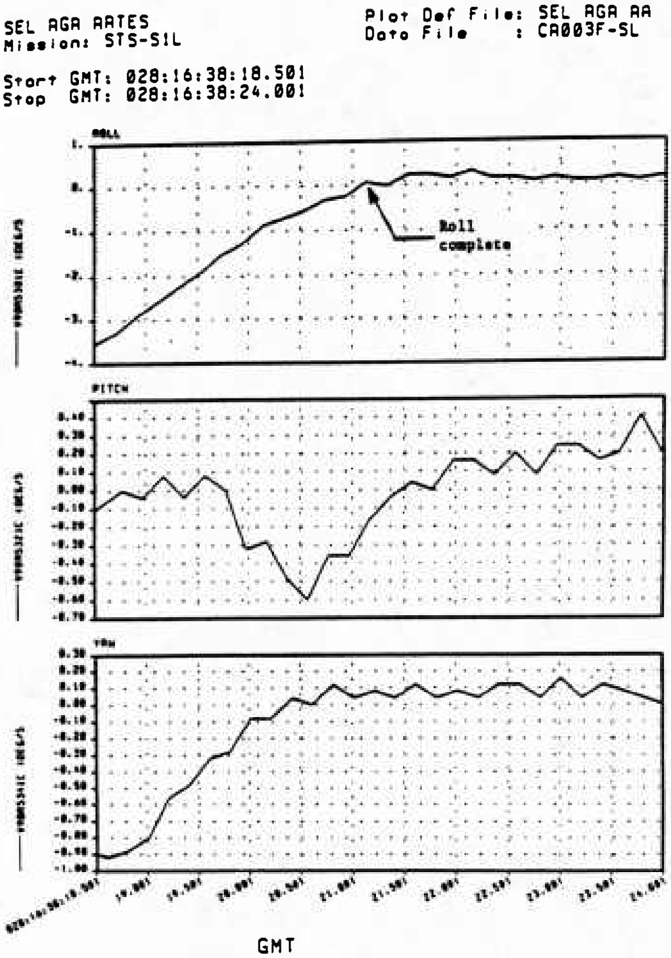


Figure 10.- Vehicle roll program complete.

[Ref. 2/13-9]

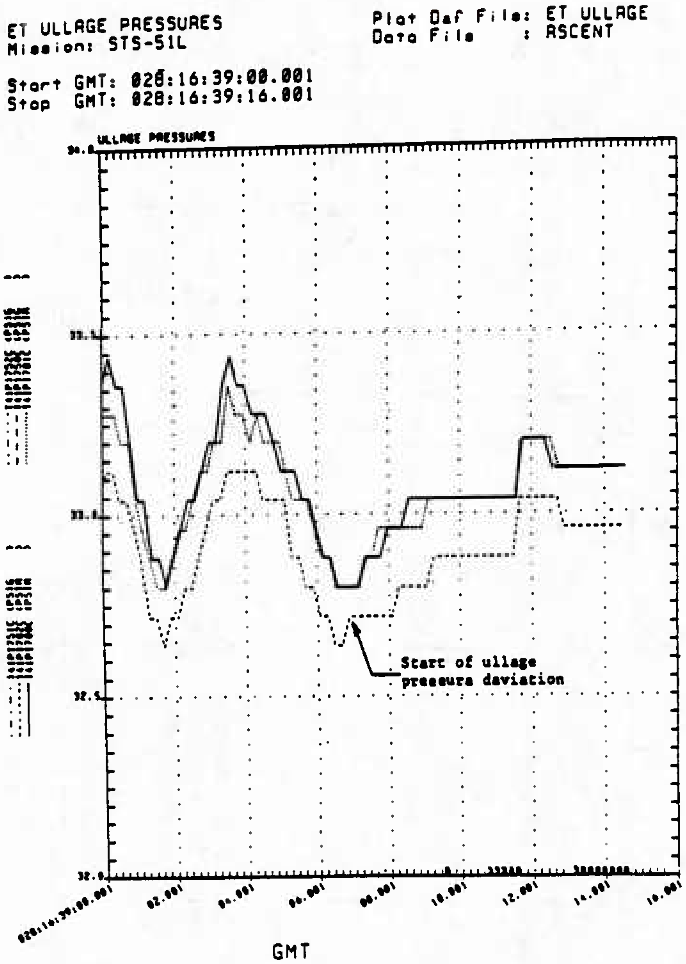
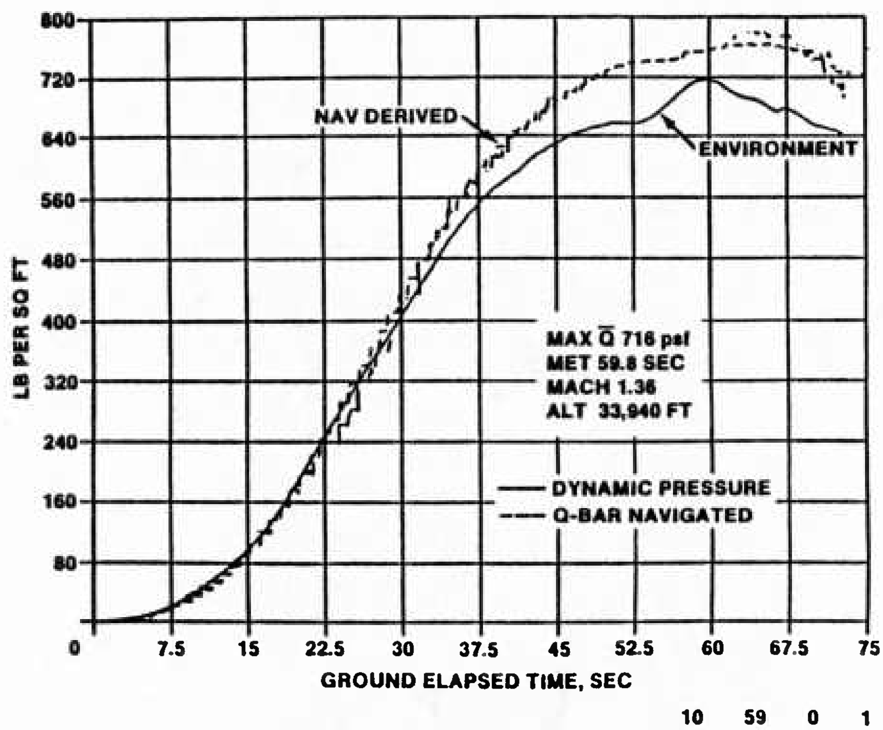


Figure 17.- Start of liquid hydrogen ullage pressure deviation.

[Ref. 2/13-10]

NASA
Johnson Space Center

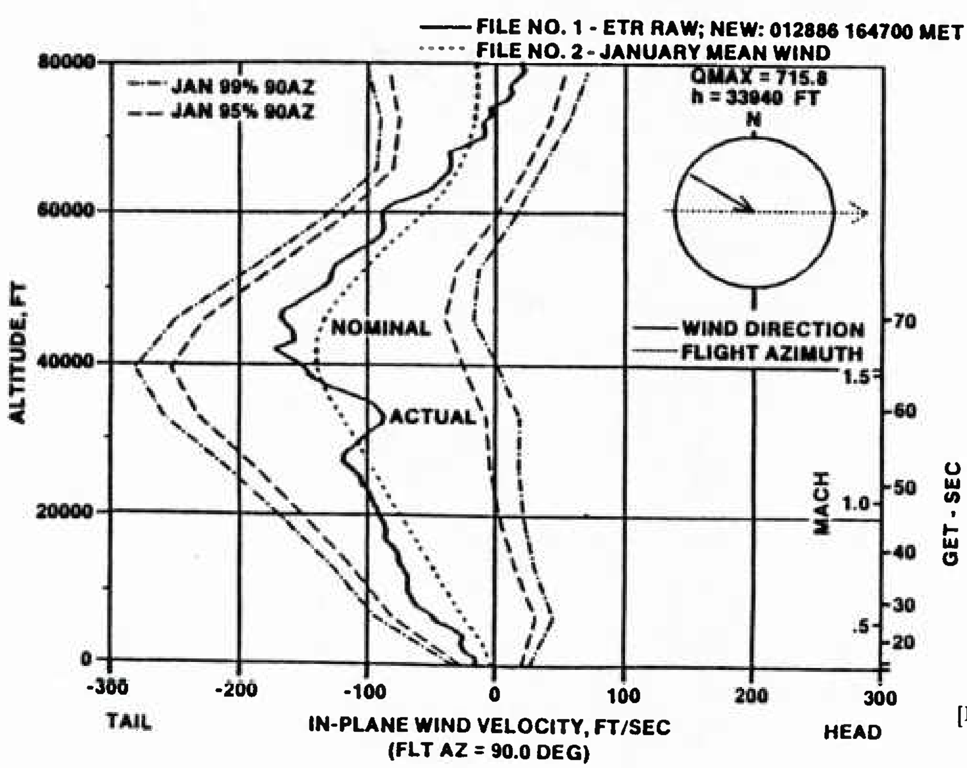
Systems Integration
STS 51-L Dynamic Pressure



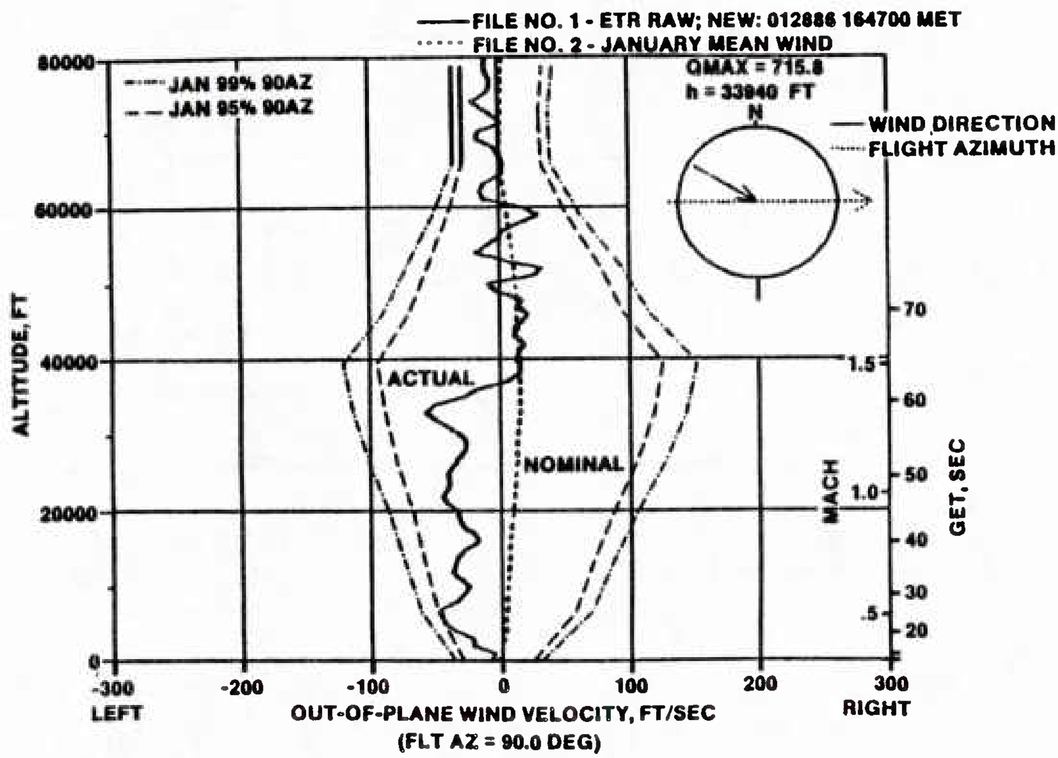
[Ref. 2/13-11]

NASA
Johnson Space Center

Systems Integration



[Ref. 2/13-12]


FILE NO. 1 - ETR RAW; NEW: 012886 164700 MET
FILE NO. 2 - JANUARY MEAN WIND
JAN 99% 90AZ
JAN 95% 90AZ
QMAX = 715.8
h = 33940 FT
N
WIND DIRECTION
FLIGHT AZIMUTH
ACTUAL
NOMINAL
ALTITUDE, FT
80000
60000
40000
20000
0
MACH
1.5
1.0
.5
GET, SEC
70
60
50
40
30
20
-300
LEFT
-200
-100
0
100
200
300
RIGHT
OUT-OF-PLANE WIND VELOCITY, FT/SEC
(FLT AZ = 90.0 DEG)

[Ref. 2/13-13]

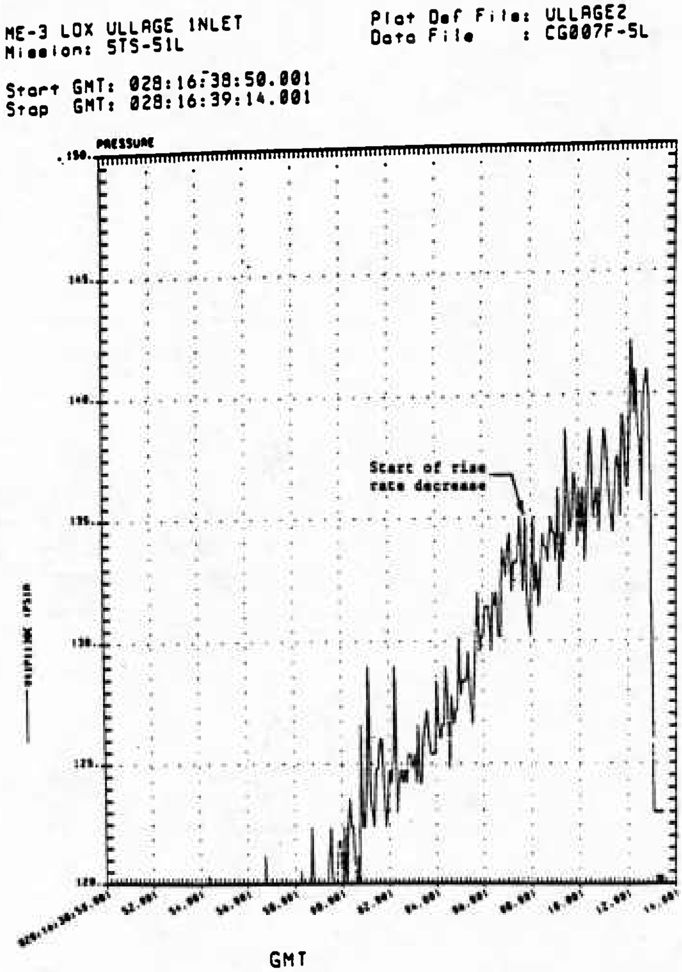
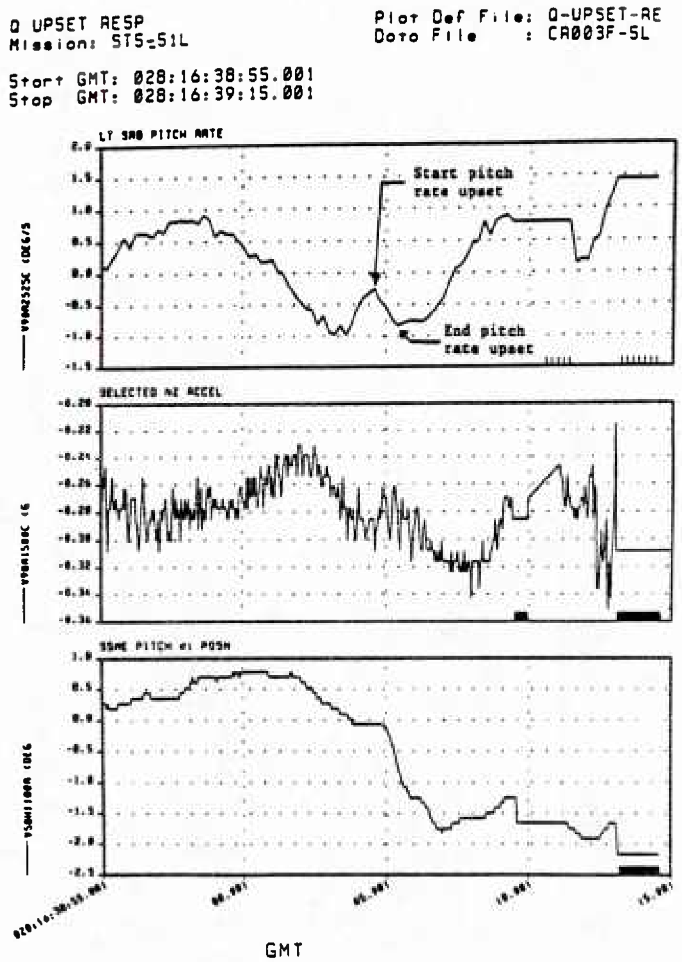


Figure 18.- MPS liquid oxygen pressure rise rate decrease.

[Ref. 2/13-14]



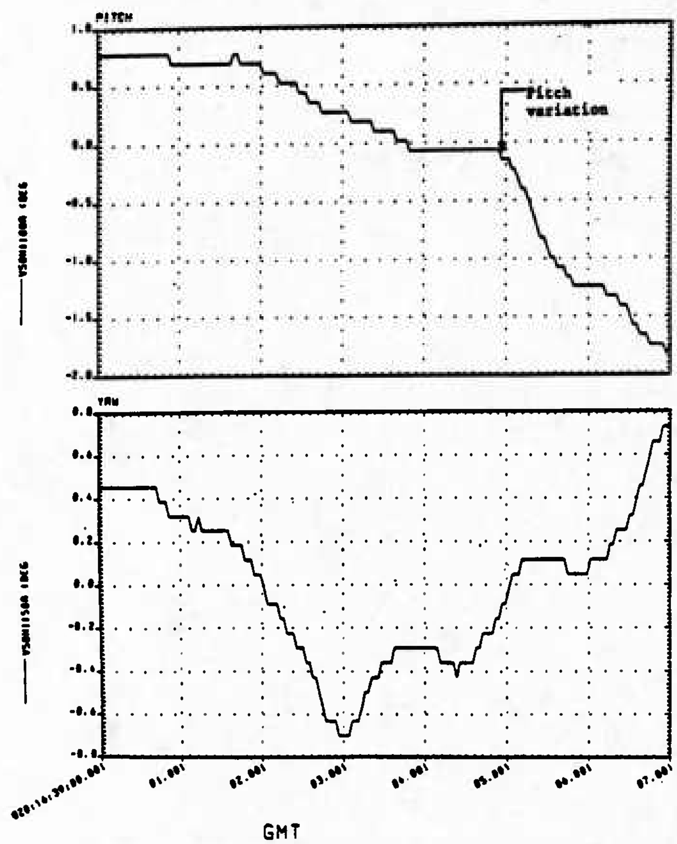
SRB pitch rate upset.

[Ref. 2/13-15]

ME-1 ACTA POS
Mission: STS-51L

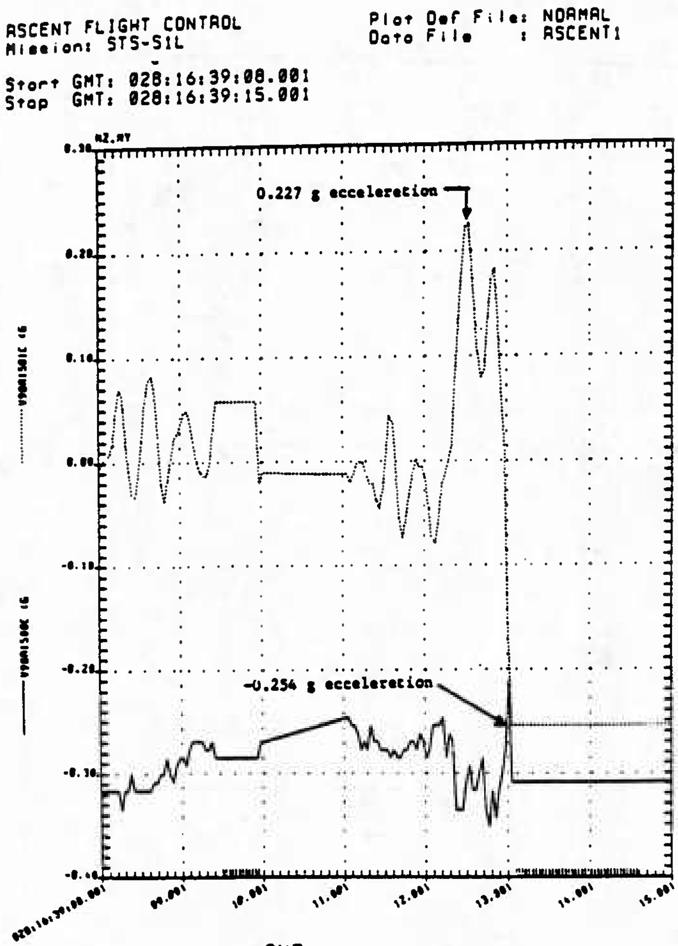
Start GMT: 028:16:39:00.001
Stop GMT: 028:16:39:07.001

Plot Def File: ME-1-POS
Data File : CA003F-5L



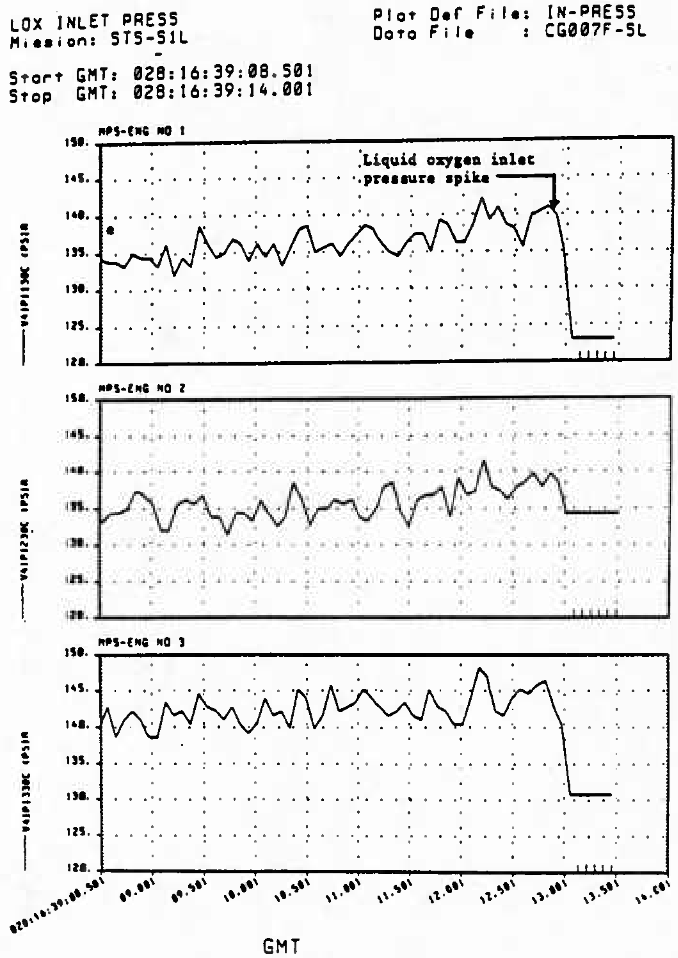
Start of main engine large pitch variation.

[Ref. 2/13-16]



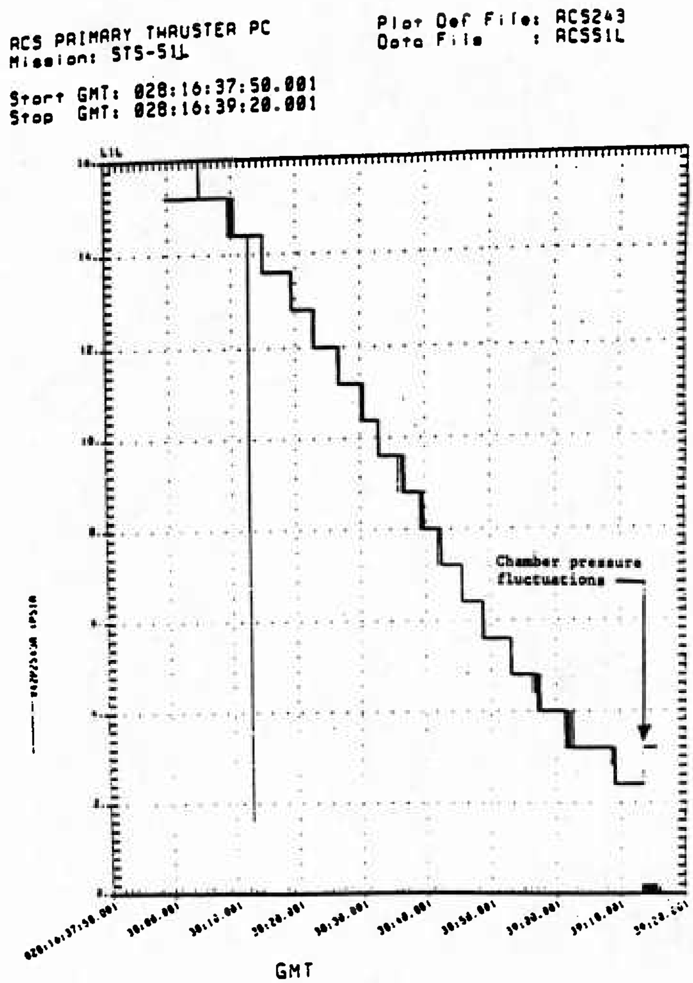
Vehicle maximum lateral acceleration.

[Ref. 2/13-17]



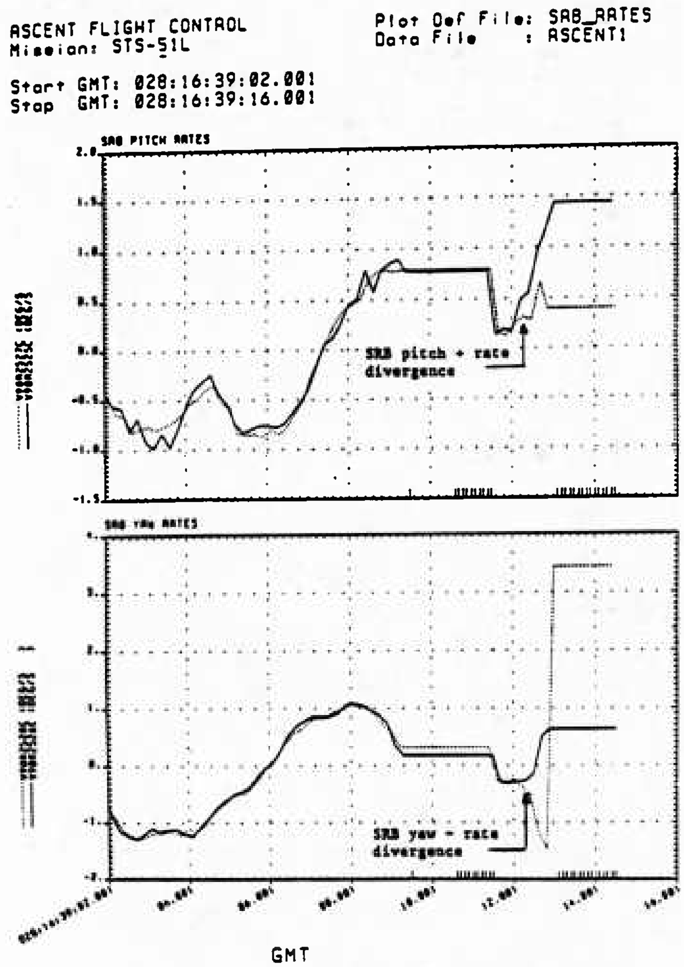
Liquid oxygen inlet pressure drop.

[Ref. 2/13-18]



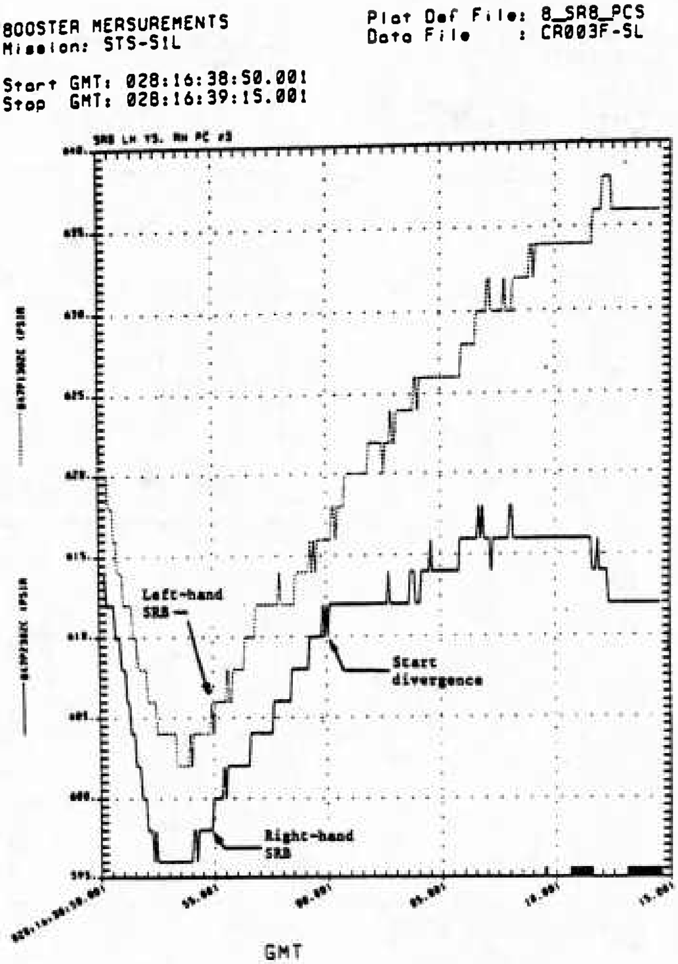
RCS thruster chamber pressure fluctuations.

[Ref. 2/13-19]



.-SRB yaw and pitch rate divergence.

[Ref. 2/13-20]



SRB throttle bucket and divergence.

[Ref. 2/13-21]

NASA
Johnson Space Center

Systems Integration
Observations

- ASCENT TRAJECTORY AND LOADS ANALYSIS INDICATE SYSTEM OPERATIONS WELL WITHIN SYSTEM CAPABILITY
- MAX Q $\approx$ 716 AT 60 SEC
- ANGLE OF ATTACK PROFILE PREDICTS DEVIATION AT 62 TO 65 SECONDS CONSISTENT WITH VEHICLE RESPONSE TO EXTERNAL WINDS IN THIS TIMEFRAME
  - SIGNIFICANT CONTRIBUTION TO OBSERVED VEHICLE DYNAMICS
- LOAD INDICATORS WELL WITHIN SYSTEM CAPABILITIES
  - LEADING INDICATOR 87%
  - SRB INDICATORS LESS THAN 30%

[Ref. 2/13-22]

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AFTERNOON SESSION

(1:45 p.m.)

MR. MOORE: Mr. Chairman, this is Charlie Stevenson of the Kennedy Space Center here, and he is in charge of our photographic team analysis, and we are going to try to give you a fairly comprehensive rundown of a lot of the photographic data that we've got from Flight 51-L.

Charlie?

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STATEMENT OF CHARLIE STEVENSON, PHOTOGRAPHIC TEAM ANALYSIS, KENNEDY SPACE CENTER

MR. STEVENSON: I guess what I'm going to try to do is give you a quick rundown of where we are, and show you some of the things we have come up with. We have a variety of visual aids here we are going to try to run.

(Hereafter, a film was shown.) [Not published]

First we are going to show you the film from our 70 millimeter cameras. We have picked the ones that show you the best views. Of course we don't have a 70 millimeter projection here, and so the machine we have will be this one, which is older than I am. As a matter of fact, it was going out when I was young. But we're going to try with this one. It is an analysis type machine, and it is not there for projection, but we're going to try it anyway. So let's go on, Gary, and we will slow it down when we need to.

What I will do, as we run along I will stop, and as we get into the other film, the things I am pointing out will become more and more obvious as you see more of them.

VICE CHAIRMAN ARMSTRONG: Is this 70 millimeter?

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MR. STEVENSON: This is 70 millimeter. Now this camera is located at Pad A, and it looks north at the orbiter part of the vehicle. And the reason we have it is it shows a good example here of the smoke cloud. And we show it in this camera because it is all the way around the vehicle looking through the vehicle, more or less, at where the cloud is here.

MR. SMITH: The Commission members may want to get up closer, because some of this is hard to see. I'm sorry.

VICE CHAIRMAN ARMSTRONG: Now it appears to me that that could be geometrically located very close to that. Am I wrong about that?

MR. STEVENSON: Yes, sir. As I go along, I will show you some viewgraphs later that will more or less show you where that is coming from.

DR. WALKER: Is there a blemish on the screen?

MR. STEVENSON: Yes. This is a very old machine, and we cleaned it as good as we could last night. You are actually looking at a view like this, side to side, from Pad A.

(Pause.)

On the TV system, which we will show you later, we can start and stop and really show you exactly what we have. This is the ET attach ring right here.

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DR. FEYNMAN: I notice it is higher now.

MR. STEVENSON: We're going to walk around the vehicle this way with our projection until we get around the back, and so several additional films will be available.

MR. SMITH: This is Dick Smith speaking. If I could point out, it appears on all the films that the smoke was projected upwards initially, and you kind of fly up through it, and so you do see the change in elevation, and it does appear to be above the joint, the large cloud.

MR. STEVENSON: It comes up and touches 30 feet or so, and then we fly on out.

MR. SMITH: We apologize for this projector. It was flown in from Houston last night. We were trying to find a projection machine we had out in the other area. We had one we couldn't move. This is an antique machine, and as you can tell it does not work very well.

MR. STEVENSON: We usually look at 16 millimeter, and if we see something we're interested in, then we go to the 70 millimeter.

CHAIRMAN ROGERS: Do you think that is smoke, or something else?

MR. STEVENSON: That is definitely smoke.

CHAIRMAN ROGERS: It almost looks as if it is

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continuing to come out.

DR. FEYNMAN: It is flying up.

CHAIRMAN ROGERS: I guess what I was wondering about was whether there was a leak.

(Pause.)

MR. STEVENSON: Okay, you can see it as we reach about this level, the 200-foot level. The cloud has actually disappeared, and from many camera angles. We only have really one camera that really shows the puff good down at the bottom. So we can see at a later time when the cloud of smoke begins to appear again.

(Pause.)

With this camera, we are able to—we ran out of form before we got to the critical point of where we saw the plume in question. This camera eventually looks right up at the bottom as we gain altitude. Now the little dark spot you see on the aft dome are normal. That is just charring. There's nothing abnormal about that. Occasionally you'll see a little white puff come by.

DR. FEYNMAN: Is that normal, too, the white puff?

MR. STEVENSON: We do see that.

DR. COVERT: The boundary of the righthand plume is considerably furrier than the other one.

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DR. FEYNMAN: But you're looking at a different part. One is below; the other is above.

MR. STEVENSON: Okay. As we continue to rise in altitude, you'll notice we are looking more and more into the dome. And on the lefthand rocket, the area we are concerned with, you can see very clearly on the lefthand—I'm talking the lefthand, not righthand—you can see the ET ring here (indicating). This little ring (indicating), the structural ring that we attach, the ET, to the external tank, and this is also the area that we attach the orbiter to the external tank. So that the SRB ET ring right here is on the other side in this area (indicating). This is where we see the plume.

We won't see it on this film, but we will see it on the next one. Go ahead.

(Pause.)

DR. FEYNMAN: Can you stop it there? Is this the plume?

MR. STEVENSON: What you see here is the IEA, which is the same as this (indicating).

MR. HOTZ: What is that red area there?

MR. STEVENSON: This is the bottom of the aft dome of the external tank.

(Pause.)

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MR. STEVENSON: In the area of the righthand booster, you now begin to see more smoke in this area than we were seeing before. Some of it is attributed to the flame being this way toward the camera, rather than just let's say smoke coming off the righthand booster.

DR. FEYNMAN: Do you know what time it is now?

MR. WAITE: Aren't you looking through the exhaust of the main engines, as well?

MR. STEVENSON: Some. Now this, the second film, is from camera site 10, which is north of Pad B, several miles north of Pad B, and I apologize for the orientation, but the image goes into the camera, and we need one more lens in it to turn it around correctly. But let's run it.

(Pause.)

Here you can see your righthand SRB here (indicating), with the plume here (indicating), and the smoke. The aft dome is in this area. This is just a reflection.

DR. FEYNMAN: From the beginning of the plume until now is about how long?

MR. STEVENSON: The first flame was visible at about 59 seconds.

DR. FEYNMAN: And when is this, now?

MR. STEVENSON: You're probably talking about 65

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seconds or so, in that timeframe.

DR. COVERT: Now there is, it looks like some strings coming out, or some little things being thrown out. Has it exploded already?

MR. STEVENSON: No.

DR. COVERT: Do you see right below the plume? To the left it looks like there are hot particles being ejected.

MR. STEVENSON: What is happening here is that the plume is beginning to wrap around the rocket case, so it is just expanding. The rocket case is that way (indicating), up.

DR. COVERT: So that is just the reflection along the line of the cylinder?

MR. STEVENSON: Yes. It actually wraps around this way, between the orbiter and the SRB. It will come around the other way, and then actually they will meet here in the middle.

As we go along, we will show you a better one of this that you will be able to orient yourself. You see the tail is up here instead of down. The film was just taken the wrong way. So here (indicating) is the tail of the orbiter, the righthand wing, the external tank, the righthand rocket.

MR. SMITH: Why don't you stop it right there,

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Charlie, and explain what you've got. It's wrapped all the way around just about now.

MR. STEVENSON: Now you can see the flame coming between the lower surface of the orbiter, the righthand SRB, and around the backside, what I call the backside, the minus-Z side of the SRB.

MR. WAITE: Why wouldn't that just melt the case and cause a separation?

DR. LUCAS: That analysis is under way to respond to that very point, and you will hear some of it this afternoon.

MR. STEVENSON: This is just a reflection off the smooth part of the external tank inner tank area.

DR. FEYNMAN: Which way is that moving through the air?

MR. STEVENSON: The orbiter normally flies down toward the ground.

MR. SMITH: What is the angle of attack it typically flies, Dick?

MR. KOHRS: At 65 seconds, it's about 2 degrees negative.

(Pause.)

MR. SMITH: Stop there a second.

MR. STEVENSON: This is the right rocket, and you can see the chute coming out here, the drogue, and

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the nose cone is long gone. And here is somewhere, if I remember correctly—well, we missed that. This is the plume we were referring to. And of course the nozzle here, the aft skirt, and the aft booster. The chute blooms, and then is immediately consumed.

Okay, this third one, again, is—there is your plume. This time it is aligned correctly. This again is the lefthand rocket this time. The righthand rocket on the other side. The plume is coming toward you. The orbiter is here, and the external tank.

DR. RIDE: Are we seeing the plume wrap around?

MR. STEVENSON: Yes.

(Pause.)

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MR. STEVENSON: You can see here, it has now spread to where it is around between the orbiter and the external tank, and in this case the lefthand rocket.

(Pause.)

DR. FEYNMAN: The last time we saw the plume it was coming out and down, and now it's supposed to be coming out and around and between?

MR. STEVENSON: The plume over here is coming under this way and out here and in between.

MR. SMITH: Speak up a little louder, Charlie, so everybody can hear you.

DR. FEYNMAN: This must be later, then, from when you had the other picture.

MR. STEVENSON: Yes. And what we have found, what we are doing in all of these, we are going back and making 8 x 10s of these, and we are putting the exact time reference on each frame so we can giggle them where they should be, and that way we will get the three-dimensional analysis.

(Pause.)

We can slow this down for you when we go to the TV tapes, and we will show you this in more detail than we can show you on this machine. This was the loss of the LOX tank, and we can see on film that it actually lifted right up and we can see sky completely under the

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forward top of the LOX tank. It blew the top right out.

DR. COVERT: Is that a vaporization pressurized, a heat boiloff, heat boiled off oxygen?

MR. SMITH: Yes, from the engine.

(Pause.)

MR. STEVENSON: Again, we're really not dwelling on these type objects here, but this is the orbiter. And once again, the righthand rocket that you can see here, the extra plume that we have that we normally would not have.

MR. HOTZ: What is that on the lower left?

MR. STEVENSON: That's part of the orbiter. We have passed the part where you can see the cabin and the lower portion, but there is some question about the RCSV. We think it is an explosion following behind. Again, you can see the chute and the obvious two plumes here.

(Pause.)

This is the wing, by the way. The wing just came across here.

DR. COVERT: Are these all manually aimed?

MR. STEVENSON: Some go with radar. They are remotely tracked, and they are corrected as we need them during flight.

(Pause.)

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Again, this is the right SRB, and the left one is here (indicating).

(Pause.)

This film was underexposed in order to study the plumes, to get an idea of what the plumes look like, which fell right in line with what happened. In this case you'll be able to see the extra plume we have.

MR. ACHESON: What angle are we looking at here?

MR. STEVENSON: We're looking right up the bottom.

GEN. KUTYNA: What would you call that?

MR. STEVENSON: I would call that inboard toward the minus Y from the struts. And I will show you by viewgraph.

DR. FEYNMAN: You were going around the circle, weren't you, with the zero angle?

GEN. KUTYNA: It's not on the bottom of the solids?

MR. STEVENSON: It's in between the rocket and the external tank, and I will point that out on the viewgraph.

(Pause.)

You see now you appear to have what is three plumes instead of two. This one here, the top plume,

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you normally don't have that.

(Pause.)

DR. FEYNMAN: Did you have a special camera set at a lower sensitivity?

MR. STEVENSON: We had a camera just to look at the SRB plume. Okay, this again is the righthand rocket. You can tell by the extra plume we have off to the side. You can see it rotate around. Hold it just a second. This is when the range safety system destructed the vehicle. A typical type range safety system destruct. Okay.

(Pause.)

Here's the frustum right here. The forward part, this part here of the SRB actually under magnification is in extremely good condition. A little bit of smudge marks, but it generally is in good condition, at this point anyway. Now that's the end of that one. Let's turn the lights on again, and let me orient the guys as to where we were.

CHAIRMAN ROGERS: Is there anything about that that you can tell us that is significant to you, more than what we observed? Can you draw any tentative conclusions from that? What I've learned is that you have a little smoke early on in the first seconds, and it disappears, and then the plume, and more smoke about

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that same time. Is there anything else of significance that you can draw from that?

MR. STEVENSON: Well, I think—can I show you the TV version, and that will be able to answer your questions a lot better. Let me have the three viewgraphs on the smoke, please.

(Viewgraphs.) [Ref. 2/13-23]

MR. STEVENSON: Okay, left to right timewise. The one on the left on this side under screen A is typical. The photo was taken out of a long stream. I just went and pulled one out just before the smoke started, and the smoke again is in this area here (indicating). And I will show you viewgraphs in a minute that will describe this entire area here.

GEN. KUTYNA: Where is the field joint?

MR. STEVENSON: This is the ET attach ring and the field joint is roughly a foot above that. I will show you that in detail in just a few seconds here.

MR. HOTZ: Where is the smoke coming from in that first picture?

MR. STEVENSON: Over here in the first two frames, we have no visible smoke. In the middle frame on the B, we have the smoke here. It actually goes up to right about here. It goes 25 or so feet up the vehicle.

DR. FEYNMAN: You mean it starts low and goes

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up?

MR. STEVENSON: Yes. It starts here, goes up, and we fly out of it.

CHAIRMAN ROGERS: What is the length of that smoke?

MR. STEVENSON: It's about 25 feet tall.

GEN. KUTYNA: Is the dark area on the bottom of the ET, is that smoke—that dark shadow?

MR. STEVENSON: Yes, sir. You see, we're already flying out of it. This is about the sixth or seventh frame where there is smoke. I'm just going downstream a little way so it is obvious to everyone that it is there.

MR. RUMMEL: Does the solid motor burn out up to that joint at that point?

GEN. KUTYNA: No, it burns the whole thing.

MR. RUMMEL: The entire length?

MR. STEVENSON: Right. You burn from here down internally.

MR. RUMMEL: It's ignited at the upper end up there?

MR. STEVENSON: Yes.

DR. WALKER: When it's ignited, how long does it take for a fire to start? Does it start very quickly, I imagine? It ignites at the top?

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MR. HARDY: It is microseconds. Just a very short time.

MR. STEVENSON: The smoke is real small in here and going up.

MR. WAITE: Do you think those are combustion gases?

MR. STEVENSON: I think these are carbon particles in the materials that make up the joint, is my opinion.

MR. WAITE: So it is just pressure leaking out? What is it that's leaking out?

MR. STEVENSON: Black smoke is what we see.

DR. WALKER: How long after the ignition is the first frame?

MR. STEVENSON: Rough time showed this to be about 610 milliseconds.

CHAIRMAN ROGERS: Going back to the question before that, what do we think is leaking out? What do we see as smoke? Do we have a guess?

MR. STEVENSON: I'm guessing, but to me it is the SRB exhaust coming through the field joint, and you are seeing the hydrocarbon particles that make up the joint is what makes the black smoke.

DR. COVERT: Could that be O-ring material?

MR. STEVENSON: It could be O-ring material. It could be

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grease and putty.

DR. COVERT: Putty is zinc chromate. That probably doesn't burn black.

MR. SMITH: Mr. Hardy is going to try to cover that when he gets up.

DR. WALKER: One other question. In the first frame——

MR. SUTTER: Could I ask a question? Can't you run a test and find out what the hell it is? Can't you set one of these things up, and build a leak, and blast it, and see what the hell it looks like?

MR. HARDY: We know that the product combustion of the grease in the joint that blows the O-ring would be black, dark in color. The insulation on the external tank would be black if it were burning—the foam insulation. So there are at least two things that I just mentioned that could be black.

MR. SUTTER: But it seems to me that maybe this is not the cause of the accident, but this is certainly one thing that might be. And it seems to me you guys would be going hell bent for election running tests.

MR. HARDY: We are, and I plan to talk about that.

DR. LUCAS: If we could look at this as

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background, George Hardy plans to take these step by step right along and describe what we know.

MR. SUTTER: I know, but I think the Chairman was reflecting a little bit of the frustration. We're not trying to solve the accident. We want to do it in an orderly manner.

DR. FEYNMAN: The orderly manner is first to get the facts and see what we see, and then hear an interpretation, because the interpretation might be wrong. But what we see, we see, and might be right. So the orderly way I think would be to look closely at it and see what we see, and then hear what they think they see.

MR. SUTTER: But this is not something that has been leaked to the press yet.

DR. FEYNMAN: We don't care about the press.

MR. SUTTER: Well, this is going to get out to the press, and I don't think it would do anybody, the Commission or NASA or anybody, any good to say we really don't know what the hell that is.

DR. WALKER: Could I ask one question? In the first frame, how well localized is the smoke, and exactly where is it on the first picture of the smoke that you saw? You say this is the fifth one?

MR. STEVENSON: There are four or five in

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front of this, and we actually first see it here.

DR. WALKER: It's really pretty close to the seam?

MR. STEVENSON: It's really big when you first see it.

DR. WALKER: It overlaps the seam?

MR. STEVENSON: We can't quite see the seam, and I will show you where we can see it in just a minute.

CHAIRMAN ROGERS: But just for our purposes, now you sound as if you think it is most likely coming from the seam.

MR. STEVENSON: The field joint.

CHAIRMAN ROGERS: Is there anybody of the opinion that thinks it's actually coming from the external tank? I realize you can't exclude it as a possibility.

MR. HARDY: We have not excluded it as a possibility.

CHAIRMAN ROGERS: But the more likely possibility is the one he says?

MR. HARDY: That certainly is a possibility.

CHAIRMAN ROGERS: The more likely possibility? I'm just trying to figure out your thinking. If it should leak out, I gather that is one that seems more likely?

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MR. ACHESON: Not to anticipate, but what is there in that vicinity in the external tank, structurally, that would permit leakage of some material?

MR. HARDY: Well, the external tank is covered.

MR. ACHESON: Is there a seam there, or any valve?

MR. HARDY: There's a number of longitudinal welds, circumferential welds around the tank in the general region between the booster and the tank.

CHAIRMAN ROGERS: So it is possible that something might have happened to the external tank that would cause this black smoke?

MR. HARDY: Yes, and I do plan to discuss that.

MR. RUMMEL: I would like to return, if I may, to Joe Sutter's point. Do you plan a test to examine the design of the rings once more and what the nature of the smoke is, and that sort of thing? It seems to me that should be something that should be of interest.

DR. LUCAS: Yes. We're going to discuss the test program.

MR. RUMMEL: When? Oh, you going to discuss that? Oh, all right.

CHAIRMAN ROGERS: Why don't you move along?

(Laughter.)

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DR. COVERT: What is the framing rate of this camera?

MR. STEVENSON: I'm sorry. I don't have that with me. There's a big thick book of all those things.

DR. COVERT: I'm curious what the time is for this plume to go 25 feet up in the air in four or five frames, and I wonder what the drift velocity is of that plume.

MR. STEVENSON: We haven't worked on that yet.

CHAIRMAN ROGERS: Why don't you go ahead?

MR. STEVENSON: We just picked a third series of photos here that show that when we are roughly at 200 feet in elevation, the black smoke has disappeared, and it is some time again before we can positively identify it as a matter of fact completely out of the frame.

MR. SUTTER: Could I make a point? Maybe I've got the cart before the horse, but if you look at this sequence of shots over here, the main motors are just about up to power, right?

MR. STEVENSON: Yes.

MR. SUTTER: And when they do, they bend the whole machine and it is held to the ground, and you've got a structural joint where that bending, really a lot of that load is going right into that joint. Isn't that right? And that might be changing the seating of those

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seals. And I'm just throwing out a theory, and I would like to get a comment on it. I will tell you the whole damn theory, and you can tell me how crazy I am.

As soon as that happens, then you put the solid rocket booster motors on, which reorients the whole load. That might be changing the seating of those seals again. And just about when

that load changes again, then you see the smoke. And when you get around to this testing, it seems to me you ought to run a test where you put those great big hellish loads in, in the direction they are here. You've got the right pressures, and you build a leaky seal, and you see what the hell happens.

This is why I sort of get antsy. Are you going to do a test? And how real is it? I think these ground-holding loads, and the changing of loads, and maybe a slightly worn eroded seal, could be the whole damn trigger. And I'm going to pursue that test. And if it's a few million dollars, or it takes a month, what the hell. I think there ought to be a hellishly good test of that.

DR. LUCAS: Well, certainly, sir, we do plan to do some testing. Let me just mention two things, though, that fall in line with your theory. One is, when the Space Shuttle main engines ignite, as you

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suggested, the whole stack leans over away from that application of force, and the thing is calculated such that just as the tip of the tank comes back to the vertical, then the solids ignite.

Now that is one of the things we are looking at. In case that was a little bit off, it would put a different load pattern into the stack. The maximum bending moment, however, on the SRB is not at this joint, but at the joint above this one. The maximum bending moment is, at that joint, however, at max Q. And so we are looking at those things.

And as you have already observed, this first light flame occurs at about max Q. And that happens to be the position of the maximum bending loads on the SRB at that time. So we are looking at those things, and I'm sure some test programs will ultimately be required.

DR. COVERT: And that's also the point where there is the maximum windshear.

DR. LUCAS: That's right. All those things are fact.

DR. COVERT: I'm not speculating at this point. I just want to see if I'm right.

MR. MOORE: Those are the factors that we're looking at.

DR. COVERT: Thank you.

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MR. MOORE: The other thing in this thing is it may turn out that a test like that is going to be required, and so forth. You know, we do flight readiness firing in preparing and checking out the new orbiters where you just simply keep the system bolted to the mobile launch platform and bring the engines up to their rated thrust. So that may be a test that we're going to have to go back and do.

DR. WALKER: But you don't fire the solids.

MR. MOORE: No, we do not fire the solids, but you can get a feel for what the loads are on the solids prior to liftoff.

DR. WALKER: But the launch pad couldn't withstand a full firing of the solids?

MR. MOORE: No. No, sir. So those are the kinds of things we're going to be looking at in terms of laying out a test program, and additional test programs may be required for the solids themselves.

DR. WALKER: When you test the solids, of course, on their test stands, they don't have these kinds of moments.

MR. MOORE: You normally test them on the horizontal.

DR. WALKER: We could impose those bending moments on it.

DR. COVERT: It would be hard to put the

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dynamic moment on it. The static ones you probably could do.

DR. LUCAS: That's right. And we did, of course, in the test program.

MR. MOORE: There could be dynamics for the mobile launch platform, as well as the base mount and pad itself.

CHAIRMAN ROGERS: Okay. Out of a matter of politeness, let's let the briefer finish his briefing, and then we will ask the questions.

MR. STEVENSON: Let's have the next series of slides on the ET ring.

(Viewgraphs.) [Ref. 2/13-24]

I'm going to present several slides here that will show you roughly what the ET ring looks like, starting with the orbiter here, the main structure, attach structure between the external tank and the orbiter. The large pieces——

DR. WALKER: Is this 51-L?

MR. STEVENSON: This is the vehicle, right. As a matter of fact, you see the frost here. This was taken at T minus 3 hours. The cable tray that goes between the SRB, across the ET and to the orbiter. The upper strut, which is well protected with what we call a milk can. It is a stainless steel fairing that goes, which has cork on it to protect the cabling between the

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external tank and the rocket.

The IEA unit——

DR. COVERT: What's that?

MR. HARDY: The integrated electronic assembly.

DR. COVERT: Thank you.

MR. STEVENSON: In a lot of our films, you will see this reflection because it is white. You also will be able to see the strut. This is the ET attach ring where we attach the rocket to the external tank. The cable tray is on the Y axis outboard of the vehicle and runs the entire length of the rocket.

DR. LUCAS: Would you point out the pressure checkport up there?

MR. STEVENSON: I'm going to go all the way around it. I'll get to the back in just a minute. We stopped at the cable tray here. Here is the cable tray again. This is the IEA, the end of the IEA assembly, the ET attach ring, back around to the Z. These are stiffener rings which we put foam on to protect from water impact. We do get a little gassing on these during the ascent, which does attribute to some of the smoke you see under the bottom side of the engine.

The next two.

(Viewgraphs.) [Ref. 2/13-25]

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We are moving now from here, which is this point. This is the backside which is in question as to the plume. Our analysis says the plume is in here. The leak checkport, which you will hear, or have heard, or will hear much about today, is here.

DR. COVERT: So that is minus-Z?

MR. STEVENSON: Minus-Z on the righthand booster is right there (indicating). Well, I'm saying it's there. It is supposed to be along in there, I believe. But I'm assuming that is what that is.

DR. WALKER: That is frost on that bracket.

MR. STEVENSON: Yes. The external tank. This is what we call the EV fitting, and this is the lower strut, the lower righthand strut. This fitting is flown bare. This way, as a matter of fact you can see the green paint here. It is isolated from the hydrogen tank.

DR. WALKER: This is where the glass insulators are.

MR. STEVENSON: Yes, there are phenolic insulators right under here. There are six bolts on the bottom, and six on the top. They do have a layer of insulation on them to protect them.

GEN. KUTYNA: Dr. Lucas, the distribution of stress around that circumference, is it equal or are there points where it is greater or lesser?

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DR. LUCAS: I think there are points where it is greater.

MR. HARDY: Yes. Are you talking about on the booster, or on the tank?

GEN. KUTYNA: On that solid, on that joint, as it bends through all these moments, is there a point where the stress is greater? And might it be at that point where it is connected to the ET?

MR. HARDY: It would be higher up above.

MR. STEVENSON: It would be higher in this area (indicating).

GEN. KUTYNA: Where the general higher stress is on SRB?

MR. HARDY: In that black area.

MR. STEVENSON: Again, I didn't point out the diagonal strut, which is on the other side. You couldn't see it in the first series of pictures we showed.

DR. WALKER: There are actually three struts, and there are only two on the model.

MR. STEVENSON: There is another one on the backside here. The lower strut is flown bare, with the exception of under the bottom here is where we have the ordinates, the cable comes across here and goes into here for the separation. So we do protect the ordinates

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wire, and this also has a layer of insulation on it.

DR. COVERT: Is that an optical illusion on that dark-colored cover there, that it doesn't stick up, that it just projects forward?

MR. STEVENSON: It does come out here. This is for this joint to move. So it isn't just a flat surface circumferential type cover. It does have a leg in it like that.

DR. COVERT: Go ahead.

MR. STEVENSON: It is to make that square with the strut. As a matter of fact, you can see it better over here.

DR. COVERT: Has it always been that way?

MR. STEVENSON: This design, yes, sir. You can see the joint here. You can see the grease here.

MR. RUMMEL: Is any degree of relative motion possible between the tank and the SRB with all that insulation?

MR. STEVENSON: Back and forth this way, practically none. The tank does rise about 4 inches when we tank it, and the strut becomes horizontal. The tank shrinks as we go to cryo temperatures and the tank goes up to the forward attachments, so this part of the tank moves up to the beam here during cryo loading, and this strut becomes horizontal.

Normally, if you looked at it untanked, it would

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have as much as I believe 8 degrees cant down. Let's go to the next three slides.

(Viewgraphs.) [Ref. 2/13-26] [Ref. 2/13-27]

I included these this morning to show about some of the lines of sight of the cameras, and the particular ones I'm showing you, we have camera flats around the pad as well as up on the pad and out on the beach, and as far north as Ponce de Leon, and as far south as Melbourne.

The camera, like one of the cameras I'm showing you, happens to be located here and looks at this angle. And what we're doing, what I've done here, when we finish, camera E-60 sits over in this northeast corner of the pad, and the field of view of camera 60 shows that I definitely can see the Z axis where the leak check plug is. I very plainly can see this part of the vehicle, and should be able to see the strut. Looking from this way, the camera on the other side of the pad on the northwest side of the pad, I can nearly see the same thing.

So by doing a comparison of all of our charts, we will be able to say exactly how far in this way between the crack we will be able to see.

Okay, what I would like to do next is just go through about ten minutes of this or so. We have a TV.

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We can start and stop that. The TV shows some things that are much more detailed than you were able to see on this one. So I guess if we could cut the lights again.

(A film was shown.)

Again, this camera is from Pad A looking north at Pad B. Why don't we just run this one through? This is one series. Let's just run it through and we will come back and pick this one up a little later.

We are going back and reviewing all of our data from previous flights. Part of this smoke is not unusual. As we go higher in altitude, the amount of smoke you see here does become unusual, and it will actually engulf part of the wing before we finish. We probably edited this one too much.

DR. COVERT: What was that bright spot up near the bow of the righthand booster?

MR. STEVENSON: We have a shockwave in here.

DR. COVERT: No, earlier.

MR. STEVENSON: It's condensation.

(Pause.)

This is the same camera, and here's the spot here (indicating). The next one—we haven't seen this one before, but you will see the smoke right here. This is a TV, black and white TV, camera located on the top of the pad.

(Pause.)

Again, you can see the plume.

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(Pause.)

GEN. KUTYNA: There appears to be a rotation in that one on liftoff. Is there any oscillation, or is that my imagination?

MR. STEVENSON: I think the oscillation you saw right at liftoff was in our transposing the camera. This came off a 35 millimeter, transposing to a videotape. It actually came off a 16 millimeter.

(Pause.)

Okay, in this one we're going to slow it down for you, but you see the plume here (indicating). Again, this is actually the lefthand SRB. The righthand was behind, and the flame is coming towards you. You will see it increase around to this side, and you can see it flash in here, which is actually coming all the way under the dome, and it is actually on the lefthand SRB.

DR. WALKER: It is just surrounding the tank.

DR. COVERT: The flame on the tank is attributed to the oxidizing nature of the exhaust, and the combustion products?

MR. STEVENSON: Well, I think the flame you see here is actually coming from where I think it was actually coming from the righthand SRB.

DR. COVERT: So there is no combustion?

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MR. STEVENSON: The heating, the charring of the foam can have a combustible out-gas, but it chars.

DR. COVERT: You're saying it's later, in other words?

MR. STEVENSON: Yes.

(Pause.)

This again is the one we partially underexposed for the flame.

(Pause.)

In this sequence, we were showing what we feel is our scenario, anyway, of how we are losing the forward part of the hydrogen tank and the LOX tank. As the flame increases from here over to here (indicating), we will see it propagate around the ring frame here. You will also at the same time we see it—do you want to stop it, Pete; back up for just a second—okay, we feel that at this timeframe we have lost the weld joint, or the barrel section just about the weld joint, between the weld joint and the 2058 ring frame, which is where the aft dome attaches, and the hydrogen we feel is coming out here.

DR. FEYNMAN: That is the joint that usually carries the hydrogen? No, it has nothing to do with that?

DR. WALKER: So you think you lost the

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hydrogen feed, too?

MR. STEVENSON: No, sir. The hydrogen feed is over here. I believe we lost the weld, just the weld joint. It just unzipped.

MR. RUMMEL: So you've got a hole in the main tank now?

MR. STEVENSON: Yes, where the barrel section joints the main ring frame here to the aft dome. But once again, we feel this is coming from the righthand SRB toward us.

DR. FEYNMAN: You see, it flashes up there. Is that hydrogen coming from somewhere else?

MR. STEVENSON: We believe also about this same timeframe, due to what we feel, and based upon the gyro data we have, that we actually lost this strut on the lower end here, and that this rocket had freedom of motion to cause a problem in the inner tank. And what you see here is the hydrogen coming from the inner tank, and this now again we are looking at the side that has the minimum amount of activity.

There is considerably more leakage on the other side, and this flash from here we feel went around the back side of the vehicle, and you see it here again. And then it propagates up.

CHAIRMAN ROGERS: When you say "vehicle," what

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is that?

MR. STEVENSON: The external tank and the orbiter.

DR. COVERT: But the negative angle—but this is at a negative angle of attack, and so that is the windward side, and so you were at some mach number like 4 or so?

MR. ALDRICH: Mach 2.

DR. COVERT: Okay. I'm not going to quibble about a factor of 2. Then, unless there is some separation there, I don't believe the propagation of a hydrogen flame will go upstream against a

mach 2, because I think the hydrogen flame propagations are around at 150 meters a second. So what is happening, causing it to move forward that way? As I say, if there is some separation there, then you could have the separation as a dead zone, and you could propagate forward, but you are at a negative angle of attack, and it should be relatively clean in there.

VICE CHAIRMAN ARMSTRONG: How well do you think we can identify the time of these TV frames?

MR. STEVENSON: Pretty well, within two or three frames, two or three thousandths. We feel that about 73.1 seconds we lose the aft barrel section on the hydrogen tank.

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MR. HOTZ: How many seconds?

MR. STEVENSON: About 73.1, and approximately 70 milliseconds after that is when the inner tank and hydrogen tank fails, along with, followed immediately by the LOX tank mainly on the righthand side, but you can see here, we do have other photographs that show a lot of activity on the other side that you can't see in this frame.

We calculate this area to have been 73.2 seconds, roughly a tenth of a second difference.

(Pause.)

You can see more action on this one than you did in the last one.

DR. COVERT: Could we go back and start that one all over again, please? And could we go a frame at a time?

(Pause.)

Thank you.

MR. STEVENSON: We have already had the mixing of the LOX and hydrogen in this area, about three frames.

DR. COVERT: In fact, that may even be a detonation.

MR. RUMMEL: What evidence is there that the strut failed?

MR. STEVENSON: We have in the TM data we have

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a rate change. The gyro rate.

(Pause.)

Again, the righthand SRB, and we believe we actually have part of the external tank we feel still attached to it, and it is actually burning out around it.

DR. COVERT: So you think that external tank held and the external tank failed, and you took a piece with it?

MR. STEVENSON: Yes, sir.

(Pause.)

Again, your righthand rocket and lefthand.

(Pause.)

This is the righthand rocket again, and actually this stage appears to be in excellent shape, except for this area. And of course the front shows a little more burning than we normally would see, or a little more charring.

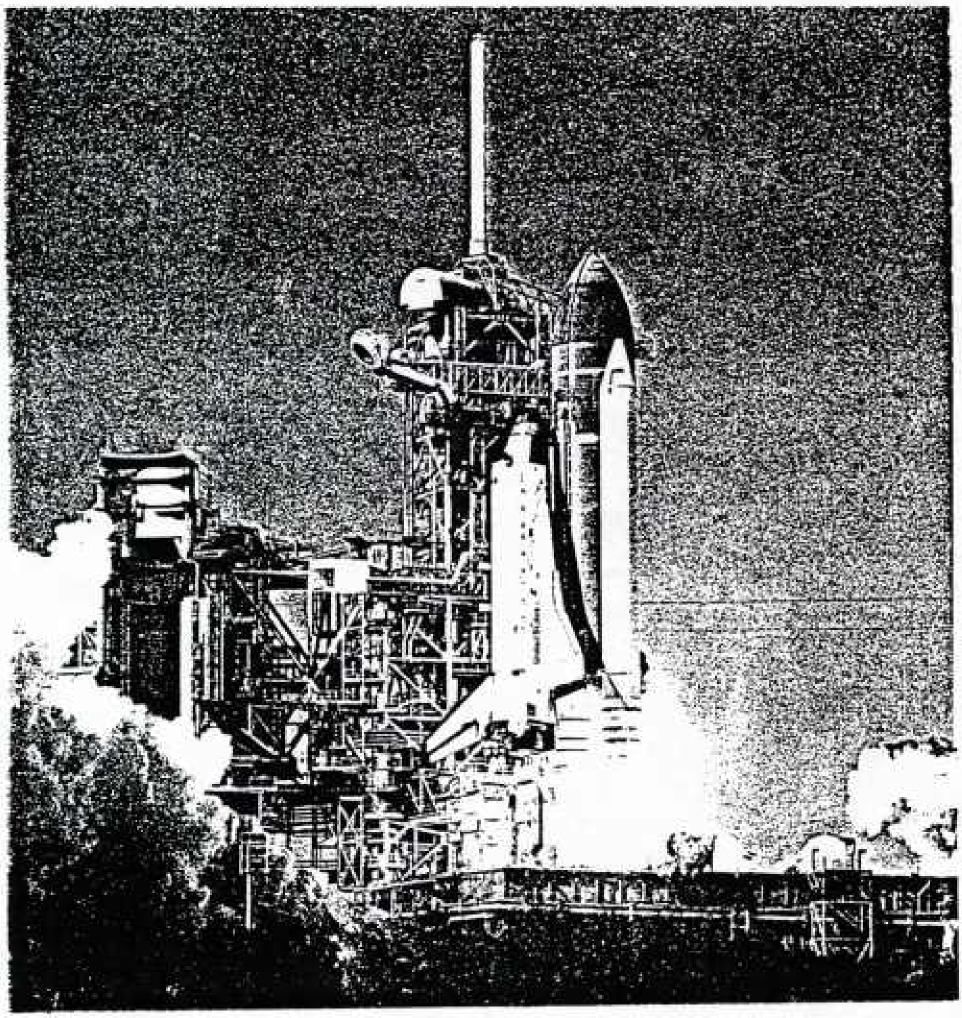
(Pause.)

This is the righthand rocket which is going to destruct in this one. You've got two plumes.

(Pause.)

There's the destruct. And again, the righthand rocket actually showed very little. It was still intact the last time we saw it. And we feel that

at least in our frames it was still intact, except for the forward assembly. The lefthand SRB, the destruct on it was what we would call normal, in that the pieces, the actual segments separated and we are only left with the aft booster portion.



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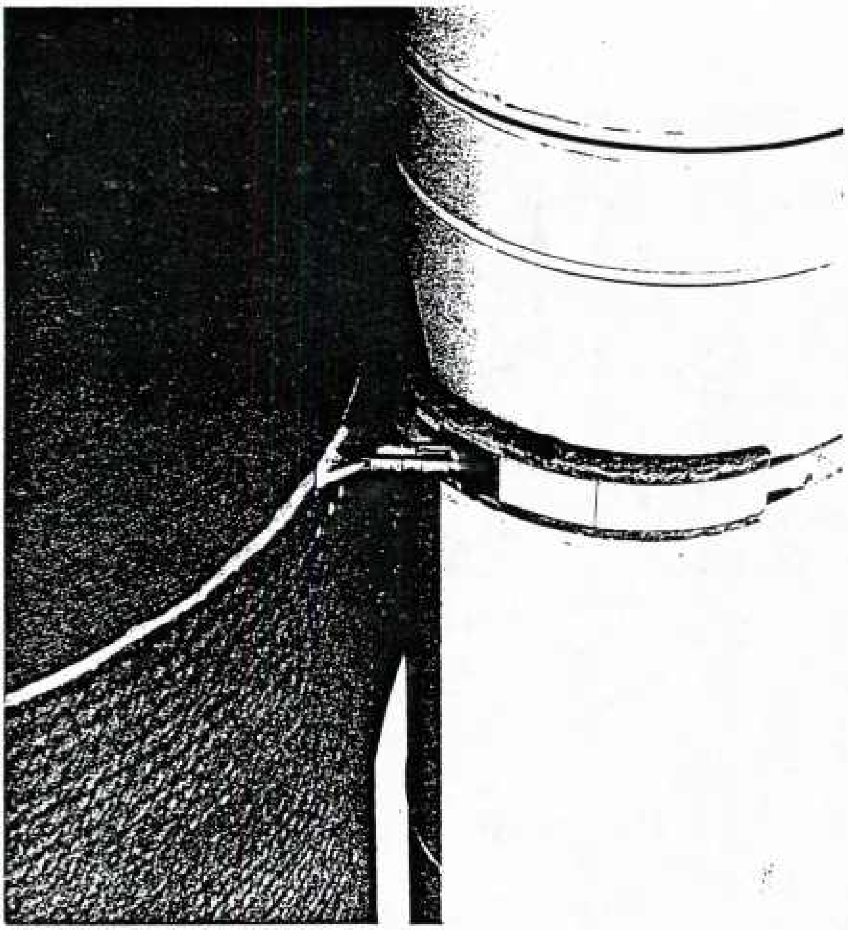

[Ref. 2/13-24]

[NOT REPRODUCIBLE]

[Ref. 2/13-25]

[NOT REPRODUCIBLE]

[Ref. 2/13-26]

[Ref. 2/13-27]

MR. MOORE: That is some of the photographic data we have. We are working on the enhancement of this with some of the best experts in the country. In fact, we got some people from Los Alamos working on this to try to blow up certain segments of this thing and see if we can duplicate this. But that is kind of what we thought was relevant to let the Commission see, and the kind of data that we do have in hand.

And we are going through, as I said, additional analysis.

CHAIRMAN ROGERS: It seems to me, from looking at it, very convincing. Why don't your photographs exclude the external tank?

DR. LUCAS: We're going to talk about that, and that may be the way it turns out. But I'm not prepared to say. I think there's a strong probability that a leak from the tank was then triggered. The thing that gives me the most trouble is how does one explain a leak through a joint in that rocket nozzle that lasts as long as it does. You see it's running for 100 seconds.

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I think we will show some calculations that say if that hot gas is going through that period of time, you would have a large hole in there. And I believe the analysis will say, although they have not yet been done, that the tank would come apart before you would get that far.

It may be, and I don't think there's any question that ultimately that joint gets hot and leaks hot gas. That is very evident here. But I am not certain that it did not come from a heating source somewhere else. If you have an external source heating that joint, it would have the same effect, ultimately.

This is speculation.

CHAIRMAN ROGERS: How about the first black smoke? Did that come from—could that have come from the external tank?

DR. LUCAS: Well, here's the thing. You see, if one postulates hydrogen is leaking, it burns with a nonluminous flame, and you wouldn't see it until it hit something. So if if hits that bead of grease going around that joint, for example, that would give you a puff of black smoke. If it hit the cork that overlays that band over the pins, it would give you black smoke. If it happened to hit some of the insulation on the tank itself, it would give you a puff of black smoke.

Now I don't know whether any of those things

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happened, but until we can exclude that, I think we ought to continue looking at it.

CHAIRMAN ROGERS: That explains why you haven't excluded it. And so it is possible that one theory would be a leak from the external tank, which wasn't apparent, and it might cause the puff of black smoke, and it wouldn't then be more apparent until the plume—how did you get the plume on the right booster that comes from the external tank?

DR. LUCAS: I think ultimately there's no question that ultimately that joint up there allows the leakage of hot gas on the solid rocket motor. My only question is: Is the flame, the penetration of the flame, through that joint? Did it originate from heating that came from inside the solid rocket booster and deteriorated the joint from the inside? Or did it happen by a flame from the outside of the solid rocket booster that deteriorated that joint? It could be done either way.

Now the thing that got my attention was that I had asked some of my people to look at how much leakage of liquid hydrogen could I have at liftoff and not detect that in any of the systems? It is about four pounds per second. That's a lot of hydrogen.

Now I want to emphasize—

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CHAIRMAN ROGERS: I think it is clear.

DR. LUCAS: —that my speculation has to include that one could have had a leak in the tank, and that it could have come from various sources. Maybe it is some defect. We are looking back at the tank to make sure that is not the case. It could be some piece of ice or something blew up, or something that penetrated, perforated the tank, or maybe it is a leak in a joint somewhere.

MR. RUMMEL: Could it have come from the filler valve, somehow?

DR. LUCAS: Well, that is disconnected. You mean at the disconnect?

MR. RUMMEL: Yes.

DR. LUCAS: Well, I couldn't exclude that as a possibility. It is supposed to be disconnected, and it shouldn't leak, and I don't think it was leaking before we lifted off, because we have hydrogen gas detectors all around, and so far as I know, no detection of a leak was observed.

DR. COVERT: There once was, long ago, some leaking, if I recall correctly, in an early preflight readiness fire.

MR. CRIPPEN: That was STS-6, and inside.

DR. COVERT: Right. But it never was

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satisfied to me that anybody knew where it was coming from.

DR. LUCAS: Oh, yes. We pinpointed that one.

MR. WAITE: Can the crystal be extruded through the crack by the pressure inside the tank?

DR. LUCAS: I beg your pardon?

MR. WAITE: Is this a rubbery material that can be extruded through the crack by the pressure inside the O-ring?

DR. LUCAS: The O-ring? It is rubber elastomeric material.

MR. WAITE: No, I mean the propellant itself, so it's actually burning on the outside of the tank.

DR. LUCAS: It is a rubbery material, but I wouldn't think that it would extrude out the tank. It is still, I believe, too viscous to flow through that joint.

MR. WAITE: What is it? 600 psi?

DR. LUCAS: It originally starts about 900 psi, and it is about 600 psi at the time one observes the flame coming out of the joint.

MR. WAITE: When you do your test, you can have a crack that's open on one end, then one typical of this, and see if you can get that.

DR. LUCAS: That's an idea. We ought to

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consider that.

DR. COVERT: I think the propellant, and I don't remember correctly, I think it's got some aluminum trichlorate in it, some solid crystal, so it's not only got a rubbery-like material, but it's sort of like a rubbery material with sand in it. And so it will extrude with greater difficulty than if it were merely a rubbery material—at least I believe that to be the case.

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MR. WAITE: Like you, I can't understand how you could have that much flow through a crack without eroding everything around it.

DR. LUCAS: Well, you will see some preliminary analysis this afternoon that supports that, and we also early on looked at the possibility of a separation of the insulation from the wall of the tank in such a way that because of the thermal gradient, that the flame could get down to the membrane of the solid rocket motor, and the calculations are that when you get a hole on the order of one inch—and I'm speaking from memory now—on the order of one inch or so in the membrane section of the case, it would become unstable and come apart.

MR. ACHESON: Is there anything on these pictures that is inconsistent with a leaky test port?

DR. LUCAS: No, sir. That is why I wanted to make sure we understand where the port is. It is in that area.

MR. ACHESON: But either way, it would look about the same from the photographs?

DR. LUCAS: Yes, sir.

MR. CRIPPEN: I guess you can clearly see where the test port is on the bottom side of that tank, and Charlie showed you the smoke, and it appeared to be

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coming from further around the tank than where the port is. But again, it is hard to tell.

MR. MOORE: I would only qualify what Bob said in that I saw the black dot up there, too, and that was the first time I saw it. That is alleged to be the test port. I am not convinced that is the test port. I mean, that is where we think it is. That is where it should be, but it is a real dot, and if anybody can see the test port on that photo, they have got a lot better eyes than I do.

DR. COVERT: I would be prepared to guess that that is grease because it looks furry.

MR. ACHESON: Did the segments have to be put together in a way that makes the test port appear at that point?

MR. MOORE: Yes. You are going to hear about how they are stacked in what comes up. We will give you that whole sequence, but the way they are stacked is that all test ports on the right-hand booster should line up on one side of the axis, and on the left-hand booster they line up on the other side. So they all should be down a vertical line drawn down the solid rocket booster roughly 90 degrees from the way we measured the clock angle on these drawings.

DR. COVERT: When you did the wind tunnel

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test, were there vapor screen pictures taken to show the shock wave conditions?

MR. SILVEIRA: Yes, we did that in oil flows.

DR. COVERT: So if it becomes a problem to trace why this flame acted like it did, there is a great body of data to refer back to?

MR. SILVEIRA: The flow between the tank and the orbiter is very, very miniscule, and it——

MR. HARDY: Excuse me. If I could mention, we are looking at setting up some tests in Huntsville where we can actually evaluate the interaction of the flow with the plumes.

DR. COVERT: And you will put smoke probes in there and so forth, and how are you going to maintain the same sensitivity constant with smoke and air as opposed to hydrogen and air?

MR. HARDY: We are just looking at how we are going to put together the tests now.

(Laughter.)

MR. MOORE: Maybe you can help us design the tests.

(Laughter.)

DR. COVERT: I am supposed to be independent, and if I helped you design it, then I would have a strong prejudice to believe what I saw.

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(Laughter.)

MR. SUTTER: On that very first film where there was a very isolated plume for a while and then it seemed to spread around the circumference, and then it got much bigger, but couldn't there be that you would say have that hole, for instance, the test hole, allowing whatever was coming out and starting the thing to still be heating it, but it would be working on just the seals so you wouldn't be having the total burnthrough to make it go slower?

MR. HARDY: That could be correct, and as I say, in another 15 minutes we are going to cover this.

DR. LUCAS: That is one of the scenarios we have. I think that's right. You see, if you assume this puff of black smoke comes from the solid rocket motor, then you have got to explain how it can stop.

Well, if it turned out that it came from the test port, which is a threaded insert with an O-ring around it, and if that leaked, then you could imagine that it would stay in there for a while, but there would be a slight little bleed of hot gas coming through, and I think it possibly could show with the aluminum oxide that you produce from the burning of the propellant, could choke those threads, but ultimately it is going to get hot enough to blow out, and then it quickly goes

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around.

MR. FEYNMAN: There is another possibility for a transient; the O-rings don't have good resilience when they are cold, and the first bit of gas that comes in gets through and makes the black smoke. And then it becomes warmer, it takes time, but it goes through where the metal is cold, and it has to warm up the metal.

I am not sure myself, and I don't know how long it should take, but warm up the piece of rubber, and then the rubber expands, fills the hole. But in the meantime, putty and junk has gotten in, and it is not really a perfect seal. It just seems to work for a while, but it gradually leaks and finally breaks down.

DR. LUCAS: I think that is a plausible scenario.

MR. FEYNMAN: I don't think it is fair to make up these things now. We should wait and hear what you say.

I'm sorry, I apologize.

DR. COVERT: It is a lot of fun, though.

(Laughter.)

DR. LUCAS: It is encouraging though that you are coming up with similar ones as we have.

MR. FEYNMAN: But in your scenario, I have a question with the hydrogen coming out of the ET tank, is

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it obvious that it immediately flames, that it burns? Is that automatic?

DR. COVERT: Well, there is enough oxygen in the air that it would burn out the interface.

MR. FEYNMAN: It starts itself automatically?

DR. COVERT: The combustion ratio is 5 percent to 95 percent, or something like that, and so that it is hard to not burn in the air.

DR. LUCAS: Its flammability, its ignition temperature is very low, and its flammability range runs from 4 percent to 96.

DR. COVERT: I'm sorry about that.

DR. WALKER: The hydrogen tank is welded?

DR. LUCAS: Yes.

DR. WALKER: It is aluminum?

DR. LUCAS: Yes, and it is a very good low temperature material, obviously, because it is containing hydrogen at minus 423 degrees.

DR. WALKER: I didn't hear the answer to the question that someone asked if you ever had any leaks in those tanks?

DR. LUCAS: No, sir.

DR. WALKER: But they do, of course, have to go over a pretty large temperature range.

DR. LUCAS: That's right, but that is—aluminum gets stronger at low temperature, and the

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environmental temperature is insignificant as compared to the minus 423 degrees of liquid hydrogen on the inside.

DR. COVERT: Are those automatic welds?

DR. LUCAS: Those are automatic welds. And the tanks are all x-rayed as you may remember. All of them are x-rayed. Each one of them is x-rayed, and then they are proof tested subsequent to the x-ray.

DR. COVERT: At what temperature are they proof tested?

DR. LUCAS: I think it is ambient but I will have to check.

DR. COVERT: That is just a leak test?

DR. LUCAS: I am sure it is ambient, because if they are proofed at ambient, the strength increases by about 30 percent.

DR. COVERT: Except it gets more brittle?

DR. LUCAS: No, it gets tougher, as a matter of fact; the aluminum alloy, this particular alloy gets tougher at low temperature than it is at room temperature. That is not true of steel, and I think Dr. Walker this morning was raising some questions there. The D-6A, which is the material in the case, would not respond the same way as aluminum, but it does have a fairly high toughness even at the temperatures, at these temperatures.

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DR. WALKER: The proof test is a pressurization of the tank?

DR. LUCAS: Yes.

MR. RUMMEL: It seemed to me the plume was almost instantaneous, a small fraction of a second. If that is the result of heating from the hydrogen, wouldn't the hydrogen leak have had to occur almost prior to ignition?

DR. LUCAS: Are you talking about for the luminous flame that appears from the SRB?

MR. RUMMEL: No, the puff of smoke. I thought you were implying that might have been the result of heating from the hydrogen flow, which could well be the case, but in any event, the leak would have to have occurred almost prior to launch, wouldn't it?

DR. LUCAS: Or coincident with.

MR. RUMMEL: And my question is, is there any way to go back over ground instrumentation or photography or in some manner to find that out? It is awfully early.

DR. LUCAS: You may be missing that the SSMEs have been burning, first one has been burning about 6 seconds before we get to the point of ignition of the SRM. You see, you ignite the SSME at intervals and the time line is when you ignite the solids, you have ignited

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the SSMEs some six seconds earlier, and that is what bends the stack over, and then when it comes back, you launch with the solids.

MR. MOORE: I think the question that Mr. Rummel is asking is is there instrumentation on the tank that will allow you to detect a leak that was small enough or large enough, as the case may be, to cause this, or on the ground, and I think the answer to that is no.

MR. LAMBERTH: That's right. The leak detection we have looked at the 17 inch disconnect area. We looked at that with fire detectors, and we also know that enough of the purge could come from that area, comes in from the aft. We had no indication of leaks on that. We have leak detection up at the vent, but we do not have any leak detection that would cover maybe someplace in that area on the tank in the same area of the right hand SRB. If you had a leak and it did not ignite, the instrumentation and the pressure and things like that didn't see it, you would not see it.

MR. RUMMEL: If it did ignite——

MR. LAMBERTH: I think we have enough fire detection around that if it ignited, we would pick it up prelaunch.

DR. WALKER: I think you said, Dr. Lucas, that

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four pounds could be leaking without being detected? How big a hole would that be?

DR. LUCAS: Four pounds per second is eight-tenths of an inch in diameter. I want to emphasize again, I don't have any evidence at all that we have a leak in the hydrogen tank.

MR. MOORE: But just as we have been saying, I mean, we have been focusing on the solid rocket booster as maybe being a prime, and we have not resolved the external tank and so forth.

CHAIRMAN ROGERS: What about the main engines?

MR. MOORE: The main engines in my book have not been exonerated, but they look pretty clean.

CHAIRMAN ROGERS: What possible data do you have from these photos that the main engines might be involved?

MR. MOORE: We don't have any working theories at this point in time, sir, that would tell us the main engines are a contributor.

DR. COVERT: The sequence is wrong. If you threw a turbine blade into the hydrogen tank, you could have something, but then you would have engine shutdown preceding the event, and there would be a lateral acceleration and lateral feed.

CHAIRMAN ROGERS: For our purposes, it seems

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to me that you always can say everything might have caused it, but with the picture and telemetry, it seems to narrow the focus somewhat, and it seems—it looks now as if it is the right booster, or the external tank tank.

MR. MOORE: I would say there are three major areas we are looking at still. Although we in my task force have not exonerated the orbiter or the main engines, they look pretty clean based upon our preliminary data, and they are likely to be exonerated. We are still looking at the SRB, at the external tank, and also the launch pad system.

CHAIRMAN ROGERS: How could that come into play?

MR. MOORE: There may have been different loads and so forth transmitted to the vehicle. That could have been the cause of this whole thing. And so we have got to go back and clearly understand what loads the system saw from the time of ignition and so forth. And so we are not

exonerating the launch facility at this point in time until we have done our loads analysis. So those are the three main thrusts we are working on at this point in time.

MR. FEYNMAN: Suppose that we do seem to all agree and that we have established something, which is

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that the black smoke appears to come from a region which is the same region as the flame later comes from the rocket. Nobody is proposing that that is a mere coincidence.

So if I have understood our situation, to take a very elementary view without solving too many problems at once, axiom one, you have got to explain the black smoke. After that, the rest of the problems will be less important.

How long does it take before the other thing comes out and so on are clues perhaps to the mechanism of the black smoke, but are not as essential. I mean, they are just helpers.

Is that true? Is that the viewpoint we are taking, that it can't be a coincidence that these things are in the same place?

DR. COVERT: I don't rule that out yet.

MR. FEYNMAN: Good, I am glad to hear there is somebody still thinking because I have ruled it out.

(Laughter.)

MR. SUTTER: I didn't appreciate it when I was lying home in bed sick, and after the last God damned airplane left for the east coast, I was asked to be at a meeting at 2:00 o'clock on Monday, and I couldn't make it because I don't have my executive jet. But I am glad

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I wasn't there because that was nothing more than to reply to a leak, and there are going to be leaks. I am not complaining about leaks, but I think it was important that we discuss these theories, and we discuss every damned thing you know, and you tell us what the hell you are going to do so that when the press hears about the black smoke, which they will, the Commission will say, hell, they've told us everything they know about it, they have told us what the hell they are going to test for, and we will let NASA tell you about it. We have already told them what the hell we expect out of them.

I would rather help you guys investigate this rather than have the God damned New York Times or Washington Post do it.

MR. MOORE: We are with you 1000 percent on that, and if we can stay ahead of the press, we are a lot better off.

MR. SUTTER: I don't want to tell you guys how to run your test program. I know you will run a very thorough one.

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MR. SUTTER: It would have been nice for us to know last Friday that this letter was out. You guys must have known that letter was out.

MR. MOORE: We sure did not know about it.

MR. HARRINGTON: We read it in the Sunday Times like you did.

CHAIRMAN ROGERS: That was our first reaction.

MR. MOORE: But I told the Chairman about the black smoke.

DR. COVERT: We knew about the black smoke I believe it was last week, but I've lost contact with reality.

CHAIRMAN ROGERS: Joe, I want to be sure to get you an executive jet so you can catch up with us.

(Laughter.)

MR. SUTTER: I just want to make one more comment. When one of those dumb guys asks what the hell do you think might be the black smoke, we know you guys have got to do a lot of investigation, and the black smoke may be some cigar left in the pipe, but I mean, you

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fellows said it may have been the damned rubber tubes. We don't have to worry about the press anymore.

MR. MOORE: Well, we are delighted to have this entire group down here so you can hear firsthand from all of the task force people that have been working this, and you can all see the data that we have seen. This is the latest data that we have seen here, and you are seeing it right now.

So you should be ahead of the world.

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DR. WALKER: Jesse, maybe you and a couple of other guys could just have a press conference and present all of this.

MR. MOORE: One suggestion is we were planning to put out some additional data after we had had a chance to brief the Commission here on some of this photography and some other kinds of things, but I wanted to make sure that the Commissioners saw it first hand before we released it.

DR. WALKER: You could have one of these things with a few select people.

MR. MOORE: We also had yesterday Dr. Lucas and Larry Mulloy on a press conference for an hour and a half to try to answer questions.

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CHAIRMAN ROGERS: When should you say something about that? I mean, I certainly would be guided by what you think would be best.

MR. MOORE: I think we ought to say something right away about the black smoke.

MR. HARRINGTON: I think the black smoke was

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mentioned in the press conference yesterday by the press.

MR. MOORE: I think we ought to release the photos on the black smoke.

CHAIRMAN ROGERS: Why?

MR. MOORE: We don't have any problem with that. I think that is the way we ought to do it. In fact, I believe we ought to put together a press kit with these photos in it, talking about the black smoke and explaining it, and release it to the press.

MR. FEYNMAN: Could I add a suggestion that when you present it you simply say that you have known about this for how many days but you preferred to show it to the Commission completely before you presented it, and that was the only reason for the delay?

DR. WALKER: Or that you wanted to document it properly.

MR. MOORE: My task force has not even seen all of this data.

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I think that is what we would like to do.

DR. LUCAS: We admitted yesterday there is black smoke. I had to say truthfully that I had not seen it, because I had not, but we did say that the black smoke had been reported.

CHAIRMAN ROGERS: But it didn't get any attention.

DR. WALKER: I think I saw you make that comment, and they didn't pick up on it.

CHAIRMAN ROGERS: Why don't we do this tonight?

DR. WALKER: Mr. Chairman, I wonder now, from the theories and models that you have, how should they deal with that? One way is just to say we have several theories, and they might want to know what they are.

MR. SUTTER: Well, I would say that they have got several theories, and they expose the theories to the Commission, they are telling the Commission about the tests they are planning, the Commission reviewed their test plans and maybe even augmented them, and they

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are getting on with the testing.

DR. WALKER: The press is going to want to know the theories.

MR. MOORE: Mr. Chairman, I think we ought to put together a draft press package on this thing, and then I think we ought to get on with our presentation here because you are going to hear some theories associated with O-rings and tank leakage and so forth.

MR. FEYNMAN: Good. Let's go on.

MR. RUMMEL: I might suggest that it would be useful for the Commission and the members to see the press releases of this type so we know what's going to come out.

CHAIRMAN ROGERS: He's been giving it to us. We've had trouble transmitting it to all Commissioners, but we have been getting the documents.

MR. MOORE: We will go off and work something for possible release tonight on these photos. It will not make the evening news. It will be for the morning type of thing.

CHAIRMAN ROGERS: Let's tell them we don't have anything for the evening news.

MR. MOORE: It might make the 11:00 o'clock

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news but not the national news because I think we want to get the right photos in there and we want to have the right explanation to go with it.

I would like to introduce Jack Lee who is Deputy Director of the Marshall Space Flight Center.

CHAIRMAN ROGERS: Why don't we take about a five minute recess first?

(A brief recess was taken.)

CHAIRMAN ROGERS: Let's come back to order, okay?

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TESTIMONY OF JACK LEE, DEPUTY DIRECTOR, MARSHALL SPACE FLIGHT CENTER, AND HEAD, MARSHALL CONTINGENCY INVESTIGATIVE TEAM

MR. LEE: I am Jack Lee. I am Deputy Director of the Marshall Space Flight Center, Bill Lucas' deputy, and the day of the incident I was assigned by him to head the Marshall contingency investigative team for the elements for which the Marshall Space Flight Center is responsible.

(Viewgraph.) [Ref. 2/13-28]

MR. LEE: We want to cover two subjects today. I would like to give you a brief summary of our contingency team activities, and more specifically, what you have been waiting for is a status report on our failure analysis.

Could I have the next chart, please?

(Viewgraph.) [Ref. 2/13-29]

MR. LEE: We are operating under a contingency plan which was approved prior to the 51-L flight by Bill Lucas. It consists of working groups for the inertial upper stage, the external tank, the Shuttle main engine, the solid rocket booster, the solid rocket motor, and for this particular activity we established a systems team because a number of these elements come together. Our team is comprised of both NASA and the contractor personnel. We located these people in the Huntsville

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Operations Support Center at Marshall Space Flight Center, and we have impounded the data as directed by Jesse Moore, and we have maintained the impoundment of this data with the possible exception of in some areas we have to take film out for special analysis or some data for special analysis, but it is under our control, and we have a secure area at HOSC.

The areas we chose to review, as they were directed by the contingency plan, is the entire manufacturing process, manufacturing and processing for each of these elements, the acceptance test packages. That is, in the case of a Shuttle main engine, that would be not only the static testing but the factory checkout from the time it leaves the factory until it goes through the prelaunch processing here at the Cape.

In the case of the solid rocket boosters and the used cases of the solid rocket motor, that would include the refurbishment and reprocessing of each of those, and that is acceptance and transportation. We include the transportation from the manufacturing site to the test site and to KSC. Specifically what we are looking for here is any unusual environments which the hardware goes through, usually humidity environmental, accelerations and so forth.

We have reviewed the data packages for the

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prelaunch activities here at the Cape and the actual flight data. Now, within those reviews, the status of those reviews, we vary from 75 to 100 percent of that activity. Thus far we have looked at all of that data. That does not mean we have completed the analysis and stopped looking at that data. That means that one of our teams has looked at that from at least 75 to 100 percent of them.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-30]

MR. LEE: This is a graphical time line.

MR. SUTTER: Excuse me. You are doing this test and analysis?

MR. LEE: No, no, no, this a review of past history of this hardware. Now, what we are doing in relationship to the failure analysis, we have another presentation to go into detail on that, but this is a category review, if you will. We have looked back to see if there were any configuration changes or anomalies or problem reports in the history of this hardware that have been identified which could contribute in any way, either directly or indirectly, to this.

DR. COVERT: Does this also include the catalogue of waivers?

MR. LEE: Everything.

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MR. SUTTER: There have been a lot of design changes.

MR. LEE: No, there haven't been a lot of design changes.

MR. SUTTER: In my opinion there have been.

MR. LEE: Not on this vehicle.

MR. SUTTER: There have been a lot of design changes from the first one to this one, and this one is the last one that had all of them.

How do you go back and look at that as you progressively put them in, and one of them may have tripped an incident.

MR. LEE: Let me clarify. I did not explain properly. This is the deviations from the previous, from previous vehicles. In other words, we are looking at changes specifically to this vehicle and anomalies against this vehicle from previous flights, and we did not go back at this time to the first Shuttle flight. I misrepresented that if that is what you understood.

MR. SUTTER: Well, maybe you are going to talk about this, but one of the things I think is necessary is to look at all of these changes, and yes, the last flight was successful and this one wasn't, but just looking at what happened between this one and this one, maybe it was one of these other changes that because

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something wasn't critical on this flight you got by with it, but when you add them all together on this flight, one of those other things may have tripped it over the edge.

MR. LEE: And we intend to progress progressively further back into the review. When we get to—this is our first blush at all of those elements. As we hone in on a particular like the solid rocket motor, like the external tank that we talked about, we will in fact do that, and we are doing that part of it. If in the case of——

MR. SUTTER: Including waivers and including things that were suggested as waivers and didn't get up to design review board, and including changes that were suggested and were passed over?

MR. LEE: That's right. We will do that for sure on the suspect elements, and the thing I have reported here is we have only gone to the deltas from the last flight. We will progress back into that, yes, sir.

Does that answer that?

MR. SUTTER: Well, I think at least some members of the Commission, I think, would like to somehow review that whole history and review the actions you have taken on waivers, and how it was concluded

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that, for instance, taking the loads on the main tank went down, why was it concluded that the initial design was one way and then somebody thought that he could change the design criteria? Those are some of the things that I think some of us, to do a thorough analysis of everything that has happened, some of us would like to go right back to that.

CHAIRMAN ROGERS: Could I just say that we are looking for a way of setting up subcommittees, and it just occurred to me how to establish one. You are the chairman of that subcommittee.

Who else would you like to have working with you on this aspect? I mean, it should probably be only two people.

MR. SUTTER: Anybody that wants to volunteer. I have got some ideas of guys that could help. Maybe we ought to have a separate meeting on that.

CHAIRMAN ROGERS: Anyway, well, you chair the subcommittee, and I think that is a perfect thing for you to do.

MR. LEE: Our configuration, control, identification, and our problem reporting system will allow us to track that, but we will probably have that in our files.

What I would like to do here is show

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graphically a time line that we talked about that at least two of the other presenters have talked about, and the reason for the underlining in red—and this is a preliminary time line, it was as of—when I made the chart it was on Monday. There have been slight adjustments. There is only, I believe there is less than around a tenth of a second off. The sequence is right, but those times need to be adjusted.

And for my presentation purposes, that tenth of a second is not critical at this time.

Let me start with we ignited the SMSEs at T minus 6.6 seconds, as planned. T zero is the SRB ignition and lift-off. The first instant we report—and by the way, I am reporting what is predicted and was planned in some cases, and some not necessarily anomalies but some things that might be slightly off nominal, and some items that are actually anomalous by our interpretation, and I will distinguish those for you.

The famous black smoke that we just discussed, we see that first indication about a half a second, and we think we see it up until about 12, a little over 12, between 12 and 13 seconds.

I would like to add one other piece of information that didn't come out in the previous

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analysis of that smoke. It is not all black. There is some white smoke in it, too. Just for clarity and for information about this particular column of smoke, it is very essential to us that we understand the location and the time of that smoke, and the camera angles that you have seen are not adequate to be able to produce that for us yet. We are going through some gas dynamics calculations to be able to try to relate that to the vehicle motion so that we can pin that time down exactly.

At about 5 to 9 seconds, we did see a slightly high performance on the right hand solid rocket motor. This was well within our experience base, and we don't indicate that as an anomaly, but because it changed, shifted our Shuttle main engine thrust bucket, where we go from 100 to 104 percent down to 65 percent and back up to 104 percent again, it shifted it slightly, so we went from 104 to 94 because of this higher performance here, which is still well within our experience base.

DR. COVERT: What does that say, 26 or 2 Gs or what?

MR. LEE: This is 2 sigma high performance. That is equivalent to about 18 psi, 17 to 18 psi.

DR. COVERT: 2 sigma is five times in 100?

MR. LEE: It is about three percent off of 100.

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DR. COVERT: I'm talking about statistically 2 sigma being about five times in 100?

MR. LEE: Yes.

GENERAL KUTYNA: What could cause something like that?

MR. LEE: That is just the way the sample that we—the test fire sample of the actual grain, the five inch motor, and its performance is comparable to this. It will be a slight, not a defect, a flaw or a crack in the propellant would give you an increased irregular grain rate for a longer period of time. It is just an irregular shaping of the grain maybe, nothing—I mean, not anything abnormal about that at all from our experience base. The only reason I put it up there is to explain the Shuttle main engine throttling and because we were looking for everything that could be slightly unusual.

At 40 seconds we see a 2 degree gimbal angle on the—on both the solid rocket boosters. This is well within our experience base, and we explain that because of winds, it is directly related to some winds. We don't see anything unusual, so we don't worry about that.

The first incidence that we see of an intermittent hot gas emanating from the right hand solid

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rocket motor is about a little over 58 seconds. When we see it continuous, it is about 100 milliseconds after that. So from intermittent to continuous, and continuous is throughout the flight, this corresponds very closely to the reconstructed Max Q time.

MR. FEYNMAN: This observation is the photographs?

MR. LEE: These were observations of the photographs. These are Max Q reconstructed.

DR. COVERT: I want to go back to that earlier point, please. You sort of gave me a nice warm feeling about this 2 sigma business. You have fired 50 of these things in flight, 25 flights, I believe, and you probably have fired another dozen or so at Thiokol and in the qualification testing and so forth, so that 5 percent is 5 out of 100. So we are talking about a possibility of one failure, having one rocket having this degree of exceedance from normal, and I guess that wouldn't give me a warm feeling under the circumstances.

MR. LEE: This is 2 sigma of performance around the main performance for that time.

DR. COVERT: I understand exactly what that means, and what I am saying is if I take 2 sigma as being a variance that occurs at some degree of

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confidence, five times out of 100, or which is one time in 20, you take the total firings you have faced, you haven't had very many exceedances of 2 sigma.

MR. FEYNMAN: Well, he has presented the data. He has told you what it is. It is 2 sigma, and it is up to you to interpret whether you think the 2 sigma is significant or not.

DR. COVERT: Okay, fine. Press on.

MR. LEE: It is in our experience. That's all I can say. It is within our experience base.

Now, the next event we see is the right hand—and this is just after we see the continuous flow from the right hand solid rocket motor, we see a divergence of the chamber pressure from the mean, from the mean of previous flights and also from the left hand solid rocket motor, and so you kind of expect that with the flow, the hot gas flow coming out of the side of the SRB.

The next event we see is the evidence of impingement of that hot gas on the LH_2, the liquid hydrogen tank.

MR. FEYNMAN: Evident through pictures?

MR. LEE: Yes. That is still photographs. In this period of time we have a 2 degree per second body motion or body rate on the SRBs that we don't totally

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understand. We have some wind sheers and some loading that we can get from what we anticipate this thrust to be. We don't have that completely reconstructed, and so I will have to say that we see this on our instrumentation, but we don't totally explain that.

GENERAL KUTYNA: Jack, let's go back to Max Q. At Max Q we have the max stress on this particular joint that we think failed.

MR. LEE: We have the maximum stress of any field joint in flight, on this particular aft joint at Max Q.

GENERAL KUTYNA: So it happens on that joint at Max Q.

Now, how sharp do we come to Max Q? Does it just happen at 58.83 or does it build up?

MR. LEE: It builds up. It could be two or three seconds.

GENERAL KUTYNA: So, at Max Q, the effect on that joint could have happened before the hot gas flows out?

MR. LEE: I would say yes, and the guys who reconstructed this would probably agree with me.

MR. RUMMEL: How does the stress on the load on the joint at Max Q compare to the so-called tang load in the second joints?

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MR. LEE: The bending load at lift-off, at the tang load, is high. I think it is—I believe—do you know, George, exactly?

MR. HARDY: I don't know the numbers.

MR. LEE: We will get that for you. It is higher. I know that. It is higher at lift-off.

The next event that we see is the first evidence of the liquid hydrogen tank leaking, and you would expect that from the impingement of the hot gas from the solid rocket motor.

MR. FEYNMAN: That evidence was not photo, that was pressure gauge?

MR. LEE: No, that is still photo evidence.

Now, the next one is pressure gauge evidence. The pressure transducers in the liquid hydrogen tank—there are three of these—at 67 seconds we see a decrease, a decrease in the rate of increase, which does not say that we are decreasing the LH pressure; we are decreasing the rate of increase.

DR. WALKER: And that is anomalous?

MR. LEE: That's anomalous.

MR. HOTZ: Could you just go back one event on the first evidence of the hydrogen tank leak? Where physically in the tank does that evidence occur?

MR. LEE: It is in the same vicinity that you saw the plume come out here.

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All right, the next thing we see is another measured event on the liquid hydrogen pressure at about 72 seconds, and we differ a few milliseconds. That has to be adjusted, but at this time we see a decrease in the hydrogen tank, in the ullage of the hydrogen tank, and we are trying to make that up with the pressure, but we are not able to. So we know that the two flow control valves that feed the pressure into the hydrogen tank are in fact full open, and they are trying to make up that ullage.

MR. FEYNMAN: It is leaking out faster than the gas is going in?

MR. LEE: That's right. It's leaking out faster than the gas is going in. At the same time period we see an unusual occurrence of the right hand, what appears to be the right hand solid rocket booster, the base, what appears to be at the base coming out, okay. So like it is pivoting about the top, and it is in fact rotating relative, at an angle relative to the rest of the stack. And we compare that data with what is happening in the orbiter and the other SRB.

MR. FEYNMAN: And you have gyros in each instrument that are different from each other?

MR. LEE: That's right, and the way we would explain that would be that base rotating out. In the

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same time period we get a lot of high rate actuator commands between, say, a little after 72 seconds up to about 72 1/2 seconds, and those are not all tied together and explained.

We think we have come detached at the base, and then—

MR. FEYNMAN: The actuators are the gadgets that turn the elevons, is that correct?

MR. LEE: The actuators are the hydraulic pistons, if you will, that gimbal the engine.

MR. FEYNMAN: Okay.

MR. LEE: At about 73 seconds we then see a very distinct anomaly in the right hand solid rocket motor chamber pressure, and we are seeing about a 20 psi loss in chamber pressure there at around about 600 psi, and so we are seeing a definite anomaly in the right hand solid rocket motor.

So then the terminal events are a little suspect still. We are using some film data, as you have seen, and some instrumentation, and we put just for reference purposes a time out where we saw the right hand solid rocket booster nose cap separate.

Could I have the next viewgraph, please?

DR. WALKER: Could I ask a question, please? Do you have separate measurements of the ullage in each

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of the Shuttle main engines, or is that a single measurement of total feed?

MR. LEE: We have tied to each engine an ullage pressure measurements which allows what we call a flow control valve to divert in this case gaseous hydrogen to go back into the hydrogen tank to pressurize it, if you will, and each of these engines has two flow control valves. In other words, two of these become flow control valves.

DR. WALKER: So all three of them are shown?

MR. LEE: Yes.

(Viewgraph.) [Ref. 2/13-31]

MR. LEE: The way we proceeded at Marshall for investigation is we start with the end item as we see it and work back to build a fault tree, and our end item was the explosion, and we tied that to a rupture or a breakup of both the liquid hydrogen and liquid oxygen tank. We built a fault tree, and this is just the overall major fault tree. There are probably 200 or 300 different elements. If I spread the whole thing out, it would be 200 or 300 elements that make this up.

Just to identify how we construct this, one area would be, to break up the external tank, would be the external tank and the solid rocket booster attach fittings fail. One would be overload of the tank or

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load exceedance, and that could come from a number of things, control, winds, thrust imbalance. The Shuttle main engine structural failure could cause an external tank rupture. Overheating of the external tank could cause it. The external tank flaw, that would be a manufacturing flaw, or some external damage, some flaw in the manufacturing. A premature detonation of the linear shaped charge would give you a rupture of the external tank. A premature ignition of the inertial upper stage that is in the payload bay——

DR. WALKER: Where is the linear shaped charge?

MR. LEE: That is on the external tank, as well as the solid rocket booster, here and here and comes down to about here on each of these, and then down the inside.

DR. WALKER: That is the destruct?

MR. LEE: Yes.

MR. ACHESON: Which one do you mean in that box?

MR. LEE: I mean all of them. Another would be, say, an explosion or a fire in the payload bay. Another would be damage at lift-off or premature separation of the solid rocket booster.

Now, what we have still all of these open,

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there are two that we have close to exonerated, but we are not yet able to we are not allowed to, from the flight data that we have reviewed would be the Shuttle main engine, and we have good

evidence that the Shuttle main engine performed just as prescribed. We actually have data after what you see as the explosion, because we are getting our data at—our 60 kilobit data that we got longer than we did the 128 kilobit information, plus the photography that we saw. And so actually, the engine was actually still running at the time of the explosion because the feed line propellant and the fact that we got data longer, we went to the point of actually, because of the overheating or the high temperature indication on one of the turbopumps, which is a red line that would cause you to cut that engine off, and we think we can see that in the data. We actually went through the process of hitting a red line and cutting the engine on after, pretty much after.

So based upon everything we have seen from flight data and photography, we do not see any connection, any connection whatsoever between the Shuttle main engine and this incident.

We believe we can almost exonerate the inertial upper stage also. We do not have flight instrumentation of that during the ascent phase because

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we don't need it, I guess, but it is in the bay, and we don't have instrumentation on it. We have enough orbiter instrumentation and enough payload instrumentation to be able to see in detail what is happening to that structure. We are able to look at temperature measurements. So we are reasonably sure the solids didn't fire. Everything we have looked at compares very favorably or exactly almost with the previous flight we had with the IUS, so we believe we can—we know that we can't tie the information we see today to that.

GENERAL KUTYNA: For the record, I would like to say that's the first nice thing NASA has said about the IUS in the last four years.

(Laughter.)

MR. LEE: Now, what we are leaving open with all of this—and there is a lot of it—is what happened, what could have happened with the solid rocket booster and the external tank. So we haven't tried to close out any of the external tank or solid rocket booster items. Where we will be taking off from today and in the next presentation will be the solid rocket motor failure and then some of the things associated with all aspects of the external tank.

For this, I would like to now go into the

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presentation that we have been trying to—the group here has been trying to discuss, and I am not criticizing you for that, but we have discussed a lot of items already that we are going to tell you about now. But this is the way we see at least two very probable failure scenarios, how we went about arriving at the scenarios, the processes that we go through for identifying the trails, if you will, how we go about doing the analysis and tests, and we are going to provide to you today what we have completed and the results of what we have completed and the analysis and tests and give you an indication of the type of tests that we presently have planned.

With that I would like to introduce George Hardy who acts as my alternate for the investigating effort at the Marshall Space Flight Center.

MR. CRIPPEN: Jack, while George is getting up, just a little bit further on the IUS.

So we have recovered portions of the IUS, and that includes chunks of solid fuel that show no signs of ignition.

STATUS REPORT TO
51-L DATA & DESIGN
ANALYSIS TASK FORCE

AGENDA

I SUMMARY OF MSFC CONTINGENCY TEAM ACTIVITIES

II STATUS REPORT OF FAILURE ANALYSIS

[Ref. 2/13-28]

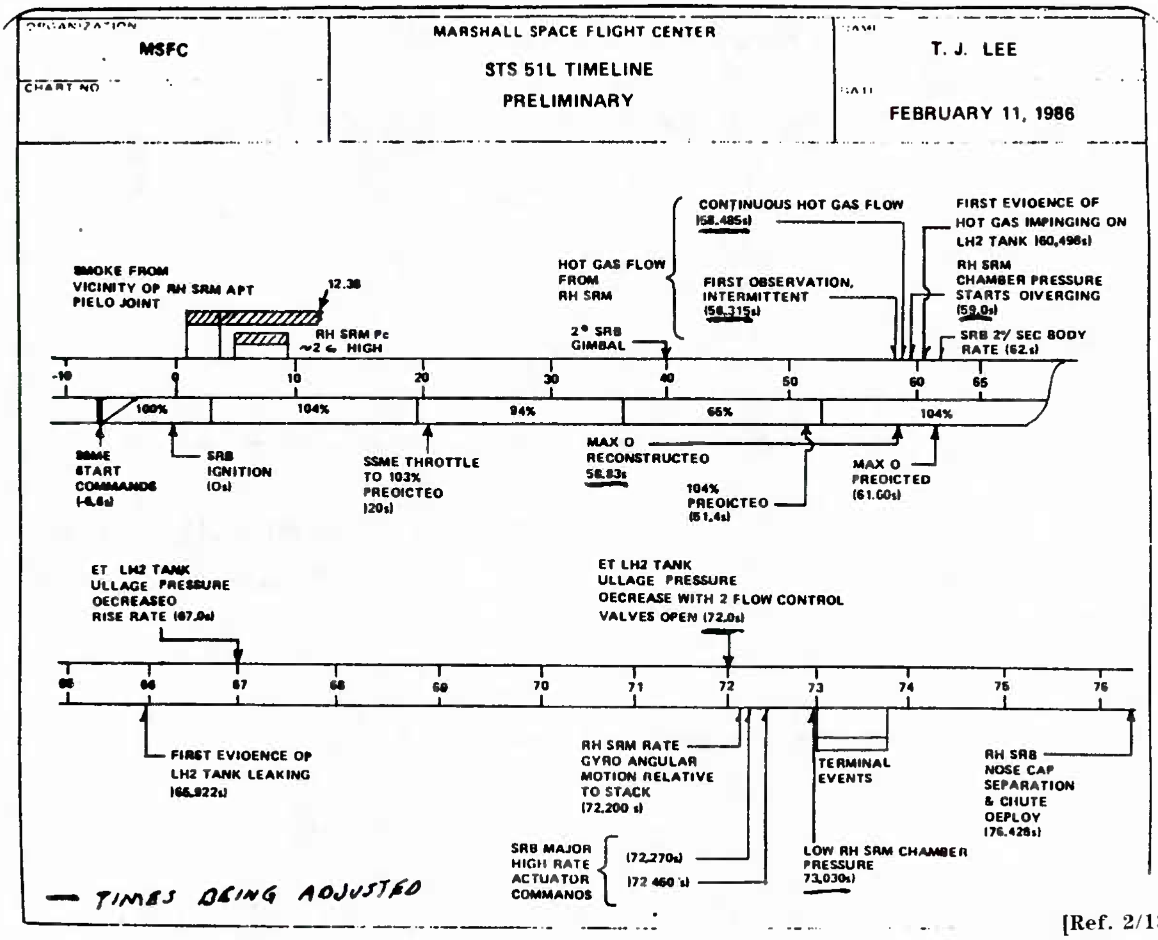
SUMMARY OF MSFC CONTINGENCY
TEAM ACTIVITIES

- o CONTINGENCY PLAN IMPLEMENTATION
  - o WORKING GROUPS

- IUS	- SRB
- ET	- SRM
- SSME	- SYSTEMS

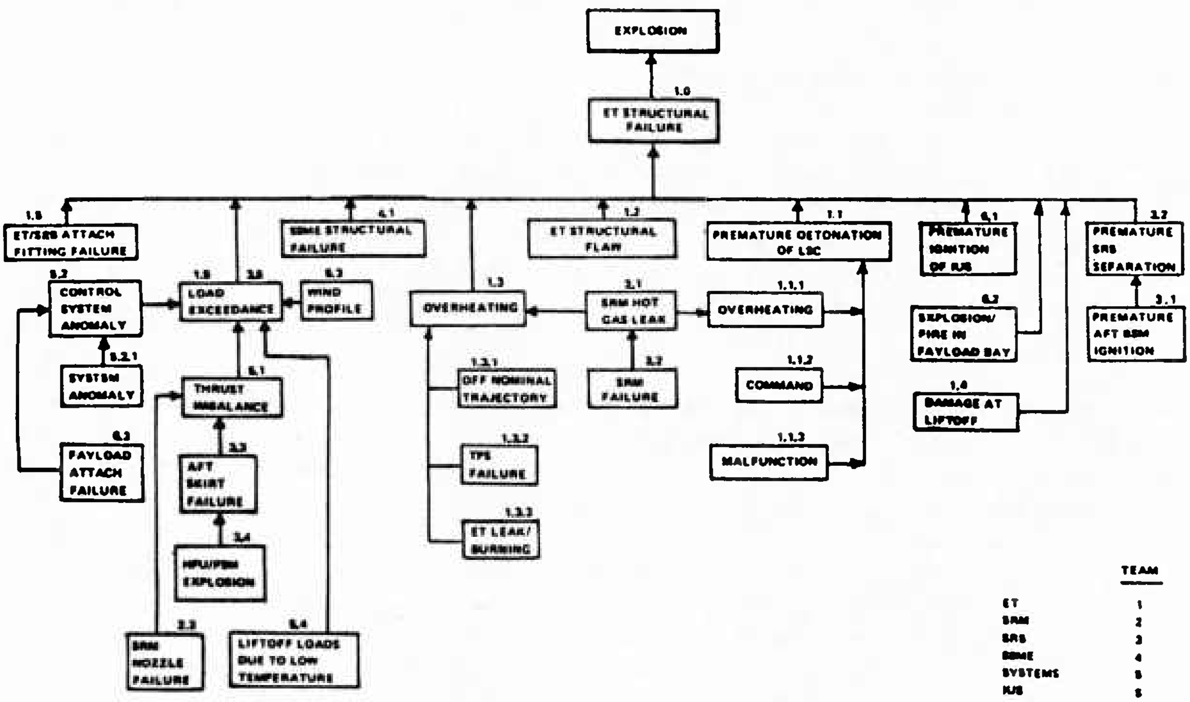
  - o GROUP COMPRISED OF NASA AND CONTRACTOR PERSONNEL
  - o TEAM LOCATED IN SECURED AREA OF HOSC
  - o DATA IMPOUNDED FOR EVALUATION/ASSESSMENT
- o DATA REVIEW STATUS
  - o MANUFACTURE/PROCESS
  - o ACCEPTANCE
  - o TRANSPORTATION
  - O PRE-LAUNCH
  - O FLIGHT

[Ref. 2/13-29]


ORGANIZATION
MSFC
CHART NO
MARSHALL SPACE FLIGHT CENTER
STS 51L TIMELINE
PRELIMINARY
NAME
T. J. LEE
DATE
FEBRUARY 11, 1986
SMOKE FROM VICINITY OF RH SRM AFT FIELD JOINT
12.38
RH SRM Pc ~2 σ HIGH
HOT GAS FLOW FROM RH SRM
CONTINUOUS HOT GAS FLOW (58.485s)
FIRST OBSERVATION, INTERMITTENT (58.315s)
2° SRB GIMBAL
FIRST EVIDENCE OF HOT GAS IMPINGING ON LH2 TANK (60.498s)
RH SRM CHAMBER PRESSURE STARTS DIVERGING (59.0s)
SRB 2°/ SEC BODY RATE (62.s)
100%
104%
94%
65%
104%
SSME START COMMANDS (-6.6s)
SRB IGNITION (0s)
SSME THROTTLE TO 103% PREDICTED (20s)
MAX Q RECONSTRUCTED 58.83s
104% PREDICTED (51.4s)
MAX Q PREDICTED (61.60s)
ET LH2 TANK ULLAGE PRESSURE DECREASED RISE RATE (67.0s)
ET LH2 TANK ULLAGE PRESSURE DECREASE WITH 2 FLOW CONTROL VALVES OPEN (72.0s)
FIRST EVIDENCE OF LH2 TANK LEAKING (66.922s)
RH SRM RATE GYRO ANGULAR MOTION RELATIVE TO STACK (72.200 s)
TERMINAL EVENTS
RH SRB NOSE CAP SEPARATION & CHUTE DEPLOY (76.428s)
SRB MAJOR HIGH RATE ACTUATOR COMMANDS
(72.270s)
(72 460 s)
LOW RH SRM CHAMBER PRESSURE 73.030s)
— TIMES BEING ADJUSTED

[Ref. 2/13-30]

FAILURE TREE



[Ref. 2/13-31]

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TESTIMONY OF MR. GEORGE HARDY

MR. HARDY: Mr. Chairman, members of the Commission, I would like to discuss with you some of the failure analysis work that we are doing under the purview of the task force.

Could I have the first chart, please?

(Viewgraph.) [Ref. 2/13-32]

MR. HARDY: I will discuss some of the failure scenarios, potential failure mechanisms or causes that set those scenarios, and then work in progress, which work in progress is a continuation of the development of the scenarios, particularly that has to do with the analysis and test work.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-33]

MR. HARDY: Just as a summary, the approach that we have taken is I think somewhat classical. It is not unique, develop failure scenario assessment matrix with that. That means to establish event one, event two, event three, etc., and with data that is in hand, the observations, film data, telemetry data from 51-L, special analyses that are run as well as tests, and our experience base. This matrix approach, then, takes each one of those events and attempts to either support it or refute it.

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Now, in many cases, as I mentioned, that does take a special analysis and test, and in some cases we have run some of those tests and analyses, and in many cases we are still in the process, and I will try to describe that to you as we go through. But successfully completing that first step, one comes to the most probable failure scenario.

I am not prepared today to tell you which that is. I am just describing the process.

The next step, of course, is define failure mechanisms and causes. If you can make the scenario fit, something still has to cause that, and there we look at again the 51-L hardware pedigree, anything in the manufacturing of the hardware, the handling of the hardware, any design changes that might have been made. We look at our experience base, if there are any clues there, the actual design itself, all the way back to the initial qualification of that design, and then experience base, by the way, that would look at previous loads that we have exposed to various elements, compare that with our design qualification load base.

We look at launch processing, launch flight, and again, in most cases have to generate special analysis or special tests to either verify or refute a particular cause. And then, of course, if one has done

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that successfully, you get to the most probable failure mechanism.

Now, I am going to talk to you about this and some of the work that is going on right here.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-14]

MR. HARDY: Before I get into the details of the failure scenarios, with respect to the propulsion elements of the booster, we have identified three failure scenarios, defined as first event. The first one is an external tank hydrogen leak. The second one is a solid rocket booster joint leak, and an unanticipated vehicle loads and dynamics.

There has been a lot of discussion here today about lift-off loads, the effect of lift-off loads on the attached structure, the effect of loads at Max Q, and I can assure you that we are vitally interested in that. And at this point in time, since much of that has to be reconstructed, I have not developed a detailed scenario that uses these vehicle loads and dynamics as the first event or the first offender. But we plan to do that as that data becomes available.

Now, I would also hasten to say that one can get into permutations and combinations of these failure scenarios. We are quite aware of the fact that there

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comes a point in time, about 60 seconds in flight, where indeed the aft joint of the solid rocket motor is leaking. I think that is undeniable. The question is is that the cause or the effect.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-35]

CHAIRMAN ROGERS: Does D include on that chart the third one, the unanticipated vehicle loads, does that include the launch problems, or is that a separate category?

MR. HARDY: No, sir, that would include, even include prelaunch activities.

CHAIRMAN ROGERS: I see.

MR. HARDY: If there was some procedural error in making the attached struts or temperature conditions, the temperature around this motor case, as I will show you later, is about a 22 degree profile from the inside to the outside, and so it would include temperature conditions. It would include the effect of ice on the launch pad, if that had any significance.

CHAIRMAN ROGERS: I see. That clarifies it, thank you.

MR. HARDY: And just one other thing I would mention, too, that goes significantly into the dynamics at lift-off. We have heard mention about the bending

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over of the booster, but it is also cantilever dynamics of the lift-off, with the simultaneity of release of the booster from the launch pad. I don't know if that has been explained to you or not, but on each booster there are what we call four hold-down posts, a total of eight, and those are released about a three and a half inch explosive bolt, or rather it has an explosive nut on it, but it has a three and a half inch bolt with an explosive nut on each one, and they are released simultaneously, they are fired simultaneous with the ignition of the solid rocket motors, and so we will be looking at the timing, for instance, of the release mechanisms themselves, anything that can put unanticipated, unexpected dynamics into the vehicle.

But as I mentioned, the detailed development of this scenario has to follow the generation of that data, and it has to be reconstructed. So I am going to be talking in more detail about these two. But we certainly will be working this one. And again, this scenario can interplay with either one of those.

What I would like first to do is to discuss just the overall scenario, the events associated with Scenario A, which is external tank leak, and then I am going to take each one of these events and tell you what we have in 51-L observed data, or what we have in

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analysis to date, what we have in test to date, that either supports or refutes any of these steps, and I will also tell you what we have yet to do.

In the process, if you fail to refute any of these blocks, then that chain has to stay open. At any point in time you can deny one of these things happening and then you block that scenario right there, and that part of the scenario is ended. If, of course, you block it back here, then the entire scenario is ended, but this scenario has again, let me emphasize, and I am talking events

that occur at this point, not necessarily causes, not necessarily why they happened, but it has an external tank leaking at or near lift-off.

Let me go now if I could to the next block, and so I can talk in more detail on each one, could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-36]

MR. HARDY: I am going to address now the external tank hydrogen leaks and this hydrogen burns. When this hydrogen burns, it overheats the aft joint on the solid rocket motor, and the O-ring seals become heated to the point that they can no longer hold the pressure. They fail.

And then we go to the next step which is impingement on the aft struts and the external tank and that starts a major event.

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Now, I am not carrying on beyond the major event. I am just trying to get to that point, and I will go through each one in detail, but I thought it would be better just to talk about them one at a time first. An alternative to the hydrogen leaking and burning is if the hydrogen leaks, it doesn't burn but it cools the joint to the point that the O-rings get so cold that the structural integrity of the O-rings is affected, and they break loose, they can't hold the pressure anymore, at which time the joint starts leaking, and I get on to this next step.

MR. RUMMEL: Excuse me. Why wouldn't it burn?

MR. HARDY: Well, I think some of it will burn, but it may not all be consumed?

MR. RUMMEL: In other words, it might burn but not impinge?

MR. HARDY: That is correct, or the flame front may be—the flame propagation may not be through all the hydrogen. It may be concentrated to the point that there's not enough oxygen there for it to burn.

DR. WALKER: Could we just return to these infrared images from which temperatures are derived, and the possibly anomalously low temperature on the right hand solid motor? Are you going to talk about that?

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MR. HARDY: I will touch on that, yes.

Just again, let me just show you the general timeframe I am talking about these events happening. I am not really trying to tie them all down, but this first, as I say, it happens at or near lift-off. This is occurring from lift-off to about 58 seconds. This occurs at around 58.3 seconds, and that is an observation from the film. And then this also is an observation from the film.

Okay, looking at Event A-1 there, it overheats the right hand half of the field joint. The observation is that we have this anomalous smoke, we have at or near lift-off, and this could be—Iam not saying it is, but it could be external tank TPS burning. So at this time— —

DR. COVERT: That TPS is the insulation?

MR. HARDY: Yes. At this point in time that observation would not deny this scenario. I am not saying that is what happened, but it wouldn't allow us to block this scenario.

So the analysis, some of the analysis we have done Dr. Lucas already mentioned. We asked ourselves, well, how much hydrogen could I be leaking and not detect it in the flight instrumentation of the tank, and the analysis says we could be leaking 4 pounds per

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second out of a hole of eight-tenths an inch, and it would not be detected with instrumentation.

The next step was to, can I structurally survive an eight-tenth inch hole, and the answer to that was yes, it was considerably greater in the area of interest than eight-tenths inch. So I can

leak hydrogen and not detect it with instrumentation. I can have a hole of the size that would be undetectable in instrumentation, and it is structurally sound, the vehicle still is structurally sound, the tank is.

Now, one of the things that we are trying to do—and I am not an expert in this area, but is through film enhancement, to attempt to see if there is evidence of hydrogen, either free hydrogen or burning hydrogen in the area of interest any time from lift-off through the major event.

DR. COVERT: George, what kind of pressure difference would there be between the hydrogen tank and the outside from the insulation?

MR. HARDY: I don't know the answer to that.

DR. COVERT: Have you guys done any experiments with that pressure difference, that the hydrogen would work its way through the insulation?

MR. HARDY: I don't have the answer to that. I feel confident it would because the head of hydrogen

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you have got there, I think that is right.

Now, at this point in time we can't block this scenario because we have found nothing in tests or analysis to date that says that at this point in time that that can't happen. So we go to the next step, and we say, well, okay, so what if it is happening, can you overheat the joint?

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-37]

MR. HARDY: This is event A-3, assuming that everything in front of it can happen and maybe did happen, and this addresses the fact or makes the statement in the scenario that the joint is overheated to the point that the O-rings fail.

Well, certainly the observation of the fact that we have a blowing leak at the right hand field joint in the timeframe of interest here that of course would not deny it. The fact that we have chamber pressure diverging would not deny that. And the fact that we have some excursion in the control system would not deny it.

The analysis that we have under way—and we have some preliminary results from these analyses, but we are refining them at this time—the question is can you get a heating rate to get the temperature of that

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joint high enough in the 58 seconds that you have available, that is, from near lift-off to the time you see the joint leaking, can you have a heating rate high enough to overheat the O-rings to the point that they would fail? We have not concluded that analysis. A preliminary assessment of that indicated that we would heat that joint to—can you give me the number on that, Rick?

MR. REINARTZ: About 450 degrees.

DR. WALKER: Fahrenheit?

MR. HARDY: We would heat that joint to about 450 degress fahrenheit. Now, that is assuming a perfect mixture of hydrogen with the oxygen available, that is burning, and that heat is flowing over to this joint and is being added to the aerodynamic heating that is already there.

Our preliminary assessment is that the O-rings, heated externally, would have to be heated to somewhere in the neighborhood of 500 to 600 degrees before they lost structural integrity.

Now, we haven't made a fit of that element of the scenario yet, but it is close enough that we have to continue to analyze it.

MR. RUMMEL: Can I ask over what period of time would they have to be heated to that temperature?

MR. HARDY: The analysis is taking it from

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essentially lift-off or very nearly lift-off, that burning hydrogen is—the heat from the burning hydrogen is coming into that joint, to 58 seconds where we know the joint leaks. We know that without question.

There is work to be done, both in the analysis area, and we are in fact going to set up a test to pressurize a joint with O-rings and heat it from the outside and see what temperature we have to get those O-rings up to so that they would fail. So at this point in time this event is not blocked. So that scenario is still open.

DR. COVERT: When you make this experiment, obviously it is going to be on a small scale, so you are going to maintain the gap size and the compression ratio in the O-ring and those geometric factors, which means it is going to be at least big enough around so the other curvature is important, is that correct?

MR. HARDY: Yes, and we are considering putting transverse loads on it, too, the kind of loads you would have at a Max Q.

GENERAL KUTYNA: How do you simulate the air flow varying anywhere from zero to Mach 2?

MR. HARDY: Well, what we plan to do is, as best we can, is to calculate the heat rate and then we

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are just going to apply that heat rate directly to the joint.

DR. COVERT: And there will be no insulation on the inside and no putty or anything like that, it would just be a clean metal gap on the inside?

MR. HARDY: Well, we will make the inside of the joint without propellant, but otherwise we will make it very similar to the flight vehicle.

MR. WAITE: Wouldn't the cooling effect of the propellant change the results?

MR. HARDY: No, I don't think so.

DR. COVERT: I would be more concerned about the chemical activity of the combustion products.

DR. WALKER: Ordinarily you bake O-rings at 250 C, so they ought to be able to take this temperature.

MR. HARDY: That is true. We estimate now that they would hold structural integrity to 600, maybe less than that.

MR. SUTTER: In a test like that you will probably run a variation of seals and variations on that to find out how much slack you've got?

MR. HARDY: Yes, we would plan to do that. We will not run a test like this, one sample test set up one way and say that's the results.

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MR. FEYNMAN: How about if you take a clamp and you tighten it up in a glass of ice water?

(Laughter.)

DR. COVERT: The other thing, George, you might consider is seals that have been eroded.

MR. HARDY: Let me say that in these scenarios—well, let me wait until I get to that point.

DR. COVERT: I'm sorry, I didn't mean to get ahead of you.

MR. HARDY: Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-38]

MR. HARDY: Now we come down to the bottom leg of this scenario, the hydrogen is still leaking from the tank, but now we are looking for cooling effect on the joint. Event A-2 says this hydrogen cools the right hand field joint. The problem we've got with that is that does not support the observations, that doesn't cause the black smoke. So the anomalous black smoke that you saw early on would have to be assigned to some other cause, but we are proceeding, recognizing the fact that the instrumentation would allow us to have a leak, and recognizing, and not be detectable, and recognizing structural integrity would allow that. We are

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proceeding with the analysis to determine how cold we can get the joint. Preliminary analysis of that would indicate that we cannot get the joint in 58 seconds cold enough that it would seriously degrade the structural integrity of the O-rings.

So we have a temporary block on this leg of this scenario, and we haven't quit work on it, but it is not a prime candidate.

Could I have the next viewgraph?

(Viewgraph.) [Ref. 2/13-39]

MR. HARDY: I think I have already covered this. Although we think we have blocked that leg of the scenario, we are still going to do the cooling rate analysis to see how cold it could get in 58 seconds.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-40]

MR. HARDY: Now, failing to block either one or both legs of this scenario, we come to the point that approximately at about 58 seconds there is clearly hot gas leakage around this aft field joint, and that is observed both from the film data. It is also evidenced in the tank pressurization instrumentation, and it is also evidenced by the fact that that tank is calling for more pressure to keep that pressure up. And so I think there is no disagreement on any of these scenarios that

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we are working that eventually we get to the point where the SRM joint is leaking.

DR. WALKER: There is another sort of combined scenario in which the leak could be there before ignition and it could have cooled the O-ring and then at ignition the leak, the hydrogen leak, ignites, but now the O-ring is pretty cool, and so it may be more likely to get damaged or eroded, meaning that you have also got in addition to burning hydrogen, you have also got a weak O-ring. So you could actually combine those two scenarios in some sense.

MR. HARDY: Yes. That is one of the main reasons we haven't closed that scenario out. And you notice this scenario was built to start with the hydrogen leak at lift-off, but we need to back that up and see what happened before that.

MR. WAITE: Your hydrogen detectors don't or wouldn't be sufficient to detect this sort of a leak?

MR. HARDY: I believe Horace said the hydrogen detectors are at specific locations, disconnects in other areas where we might have some suspect for leaks, but general acreage of the tank, a survey is not covered for hydrogen leaks.

DR. WALKER: So these are near valves?

MR. HARDY: Near valves and disconnects and

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things of that sort.

Okay. I would like to go now to the next scenario which addresses the first offender being the SRM joint.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-41]

MR. HARDY: This scenario says—and I'm going to talk to each one of these in some detail, too—that I have primary O-ring blow-by. That is, I have gas past the primary O-ring. The secondary O-ring does not seal, which means now I've got gas to the outside or the secondary O-ring does seal and I have leaked past the leak check port. In either case—and I will discuss this in detail—in either case, the scenario says that the joint is either—has either leaked and stopped or it has continued to leak for this period of time from lift-off to this time here, and then we see a major hot gas leak out of that joint which goes on to the same point that the other scenario was going to take all the scenarios and with this, all the scenarios have that in it because that is well established in observation.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-42]

MR. HARDY: Now, let me say at this point, and

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Dr. Lucas has already mentioned this, and I will elaborate on it a little bit more, but we have great difficulty analytically starting a leak or having a leak past both O-rings at or near ignition, and have that leak remain relatively well behaved, and I say that because I don't know just exactly how well behaved it stays, but to have it relatively well behaved in terms of the expansion of that leak and the leak area through that joint up to 58 seconds. But I am going to talk about that a little bit more.

But as has been mentioned here, we are looking at scenarios that—it leaks, then it seals, then it leaks again. That is not easy to come by either. This material, the O-ring material, subjected to the gas temperatures for any period of time, seconds, doesn't tolerate that very well.

DR. COVERT: You are going to talk, I assume, about combinations of these so you might have one of these that seals and then later the high load and vibration?

MR. HARDY: Yes.

DR. COVERT: Okay. Press on.

MR. HARDY: Well, let me just say now, in the scenario of the leak-seal-leak——

DR. COVERT: Let's stick with this one. I

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didn't mean to get you off. I just wanted to know if you were going to that.

MR. HARDY: But if I don't cover that, remind me.

Looking at event B-1, which says primary O-ring blow-by, now we have that in our experience base. Our experience shows that the joint seal design can result in what we refer to as blow-by, which is during a transient build-up of pressure. While that primary O-ring is pressure actuated—could I have the next viewgraph on the right hand screen?

(Viewgraph.) [Ref. 2/13-43]

MR. HARDY: I think you have probably seen this a few times, but the primary O-ring is located here, and the secondary O-ring is located here. The leak test port is located in such a way that we pressurize the annulus, or the volume in between here, and you have heard the description as to how that is done, I believe. It is pressurized up to 200 psi, and it is held for ten minutes, is that right—no, 15, and then that is for two reasons. One is to seat the O-rings or put the O-rings in a position where they can seal, and the other is to ensure that we don't mask a leak in the primary O-ring by the putty holding the pressure.

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DR. COVERT: George, in Larry Mulloy's slides where that zinc chromate putty comes down and seats against the tang, his were always very carefully had a gap there, which is the real configuration.

MR. HARDY: This is not correct. That putty is terminated about halfway back up here.

DR. COVERT: Thank you.

MR. HARDY: I might say this dimension is exaggerated, too. That gap probably shows there about twice as wide as it is configured.

MR. RUMMEL: May I ask when these units are assembled, I guess they are assembled vertically and one unit goes down on top of the other and that tang goes in the clevis. Do they always go in and go home the first time, or is it necessary to pull them out or twist them or somehow displace that putty?

Do you know, in the process of assembly?

MR. HARDY: I believe that Mr. Lamberth is planning to discuss that in quite some detail, is that correct, Norm?

MR. LAMBERTH: That is correct, George. Our briefing will go into that, but to answer your question, no, we do make the measurements that ensures ourselves that we have the proper turns before we mate the joint, and we have never had to come back out unless we had a

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leak or something like that where we had to change the O-ring.

DR. COVERT: Let me ask again, is that gap really that big between the propellant in one segment and the propellant in the next?

MR. HARDY: That is generally representative, is several inches. I can get that number.

DR. COVERT: I don't need the number. It just sort of looks like a big crack in a way.

MR. HARDY: I think you could probably put your hand in there.

MR. SUTTER: That sketch is correct in that the test port and O-rings are tied together so that you could have a combination failure of O-rings and test ports all being involved in the failure?

MR. HARDY: Yes. In fact, that is one of the scenarios I will talk about. But what I wanted to make here, talking about the incident of the blow-by, I guess what I'm trying to say is that we have experience, event No. 1, we have experienced it several times. So I don't have to prove that can happen because in fact it has happened. I was just explaining the primary reason for it to happen is when we do pressure check here, this graph doesn't show it too well, and I've got one later that shows it a lot better, but this O-ring, the primary

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O-ring can move back and does move back against this edge of the groove, and the gap between the other side of the O-ring and this other side of the groove, of course, depends upon several things, not the least of which is the diameter of the O-ring. But it also can be because of the allowable dimension on this groove here, it can be anywhere from 15 to 30 mills.

So when the motor is pressurized and the pressure first hits this O-ring, this is what we refer to as pressure actuating. It has to move back to the side.

Now, I am going to go into more detail about that with some better diagrams in just a few minutes, but I only wanted to point out that we believe that that is where we occasion what we call blow-by. But we do know from experience that you can establish for a transient period of time some blow-by of the primary O-ring. In every case it has been limited. It has been limited

by the sealing, first of all, by the fact that the secondary O-ring is sealed, the leak port has sealed, so there has been no way for the gas to continue flowing.

Could I have the next viewgraph, please?

DR. WALKER: Before you do, I just want to ask a couple of questions on that.

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How many threads are engaged in that plug, in the leak check port?

MR. HARDY: I will have to get the answer to that.

DR. WALKER: I don't see the steel band. It is not shown.

MR. HARDY: The steel band is right here underneath the cork. What is represented here is a shim, and I will talk about that a little bit later.

DR. COVERT: George, Larry Mulloy again said there were two O-rings on that leak check port, and your diagram only shows one.

MR. HARDY: There are two O-rings on that, and I will get this diagram corrected before I show it again.

DR. COVERT: I don't mean to be a nitpicker but as you know, I get confused easily.

MR. HARDY: I think that is a good point. We should represent truly what it looks like. Could I go to the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-44]

VICE CHAIRMAN ARMSTRONG: That is threaded, too, isn't it?

MR. HARDY: Yes, it is.

Now, this is the incident of blow-by that we

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have seen on the primary O-ring, and I think most of you heard a good bit about this in Washington. But I just wanted to show that we have had four instances of blow-by, and I distinguish blow-by from O-ring erosion because those can occur together, they don't have to occur together, and in fact, there is a slightly different mechanism that causes each one of them to happen.

There does, for just a matter of interest, seem to be focused around this leak check port area here. Now, we have at this point in time assigned—haven't assigned any great significance to that. It may well have to do that this is where the gas enters to do the leak check, and there may be some more disturbance of that primary O-ring pushing it back in that area versus in the other area.

GENERAL KUTYNA: That's a contradiction of what we heard in Washington because we asked Mr. Mulloy at least twice, was there any area in which this was localized, and he said no, it was pretty well distributed, and you are now saying that it is at that bottom of the Z axis.

MR. HARDY: Well, Larry may have been referring to the fact that it is not total. We do have one over here, but I will check my facts on this, but

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I'm pretty sure I'm correct.

DR. WALKER: Maybe he was talking about the erosion rather than the blow-by.

MR. HARDY: Now, that is true on the erosion. In fact, the open circles is where we've had erosion, and the half-opened, half-closed is erosion and blow-by. So on erosion you can see that there doesn't seem to be any pattern where you've got erosion on the O-rings.

GENERAL KUTYNA: And yet he did say that given the choice between the two, the blow-by was the more serious.

MR. HARDY: Yes, and I agree with that because the blow-by can in fact get us through the first event in this failure scenario.

Could I go to the next viewgraph, please?

MR. FEYNMAN: I hope it's not improper to bring up something slightly different. The question is whether there is a correlation between the blow-by and the accuracy with which the gaps and so forth were fitting, the mechanical fit of the particular joints when they were put together. There must be, of course, all kinds of records, and you must have heard that a million times, does it turn out the blow-by occurred when the gaps——

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MR. HARDY: There's a number of things that we are attempting to correlate blow-by with, environmental conditions, specific configuration of that joint, material vendors, whether there were more than one material vendor, was there one material that performed a little bit better than the other, and as you correctly state, the dimensions, specific dimension of each joint, and we are correlating all of that, and we see some correlation in certain areas.

I am really not prepared to talk about it right now because I can't remember all of the details.

(Viewgraph.) [Ref. 2/13-15]

MR. HARDY: The second event I would like to go to first is to take this trail down here and look at event B-2. This says if I get blow-by of the primary O-ring and this leak check port is leaking, the first question we ask ourselves, could this anomalous smoke starting at or near lift-off, could that be from the leak check port, and I guess there is some difference of opinion among the film analysts at this time, and I am not going to try to pick sides, but I am going to work to find out which one is right, that there is film that says that we can see the leak check port, you heard that discussion earlier, whether we really know whether we are looking at the right place, that say you can see the

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leak check port, and the leak check port is not leaking. At this point in time we have not put a block on that. It may get blocked if in fact we can verify from observed data that that port is not leaking.

DR. COVERT: George, what is the diameter of that leak check port?

MR. HARDY: Three-eighths is the number I remember.

DR. LUCAS: But it's a one-eighth inch hole.

MR. HARDY: Yes. You saw from the drawing that the hole that went directly into the cavity is one-eighth inch. But as long as we can block this—as long as we can't block this, then we look at the analysis to see what are the thermal flow characteristics of this leak check port. If the leak check port is leaking, it could be several things, theoretically, that could cause it. One would be missing O-rings. The other would be not missing O-rings but damaged O-rings. The other potential could be lack of proper torque.

Now we set up a series of tests. That is shown right here.

DR. WALKER: Do you know that the plug was there?

DR. COVERT: I think if the plug wasn't there,

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when we looked at that white paint, although three-eighths is pretty small compared to that——

DR. WALKER: Do you have some photographic evidence?

DR. COVERT: We looked at it. Didn't we look at the closeout pictures of the ring?

MR. HARDY: I don't know if we have close-out pictures on the photos or not.

MR. LAMBERTH: Yes, George, there are—a close-out photo is not required, but in the area you can see the hole there, and you can argue about the plug being there or not. It looks like it is there, and all of our paper and everything shows normal, buy-off and everything normal.

DR. COVERT: On that Eastern airliner that lost those O-rings in the oil, they had all the right paperwork, too.

MR. LAMBERTH: George, just to kind of correct the record, the drawings and everything, in our architecture there is one O-ring on the leak check plug.

MR. ACHESON: In real life there is one?

MR. LAMBERTH: Yes, and this is a plug and an O-ring.

MR. HARDY: So this chart is correct. I had

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heard that two different ways.

But in any case, what we want to do is characterize the leak. What would be the leak rate for various conditions of anomalies of that plug? And that is important in order to determine—to do the flow analysis and the thermal analysis. And we have done the thermal analysis and determined that for any leak rate we just about we want to pick out of that plug, whether we want to consider the plug is gone, the O-ring is gone, or neither are gone, but there is a very low leak rate out of that plug, we have determined that the secondary O-ring will degrade and erode to the point of failure before the threads are heated to lose structural integrity and blow the plug out.

So as I said, very key in this scenario is coming to some grips with whether or not that might be the source of the black smoke because this scenario would fit the incident of a small leak early on, and then the joint failing at 58 seconds.

MR. FEYNMAN: This way the idea is that it starts to leak through the leak check port, through the primary, and that perpetual gas going through there ultimately destroys the secondary O-ring?

MR. HARDY: That is correct. The thermal damage and strength loss in the threads would not be

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sufficient for it to go 58 seconds, but the secondary O-ring erosion would.

Now, in characterizing this leak, we are going to do cold flow and hot flow tests because I believe, as Dr. Lucas also mentioned, we believe that it would be possible to get let me say, a relatively high leak rate out initially, but as the aluminum oxide and other products of combustion flow through those threads, it would tend to slow that leak rate down.

So we are using a variable leak rate in these analyses. But in every case we see a secondary O-ring failure before we see the leak check port.

GENERAL KUTYNA: Why is that secondary O-ring eroding? There is no flow; there is stagnation at that point.

MR. HARDY: Well, if I am flowing through here and I am flowing out here, I am pumping heat into this gap.

MR. FEYNMAN: It is circumferential.

MR. SUTTER: The first ring is getting cooked fast, but the second one could last a lot longer because there isn't flow by it.

MR. HARDY: Well, there is flow into this cavity.

MR. SUTTER: But if it is sealed, it isn't

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flowing——

MR. HARDY: But if I am pumping 5,000 degree gas in here, in order to get out right here, I am severely degrading that O-ring.

MR. SUTTER: But I am just curious, is that plug, what I see here is just a plug screwed in and some guy torques it, and there's no locking device on it?

MR. HARDY: You are correct, to my knowledge, that is correct.

MR. SUTTER: Are there other nuts and bolts like that, too? It doesn't seem like a standard design practice. I am just curious.

MR. HARDY: Well, it is a standard design process for a lot of electrical connectors and many of the structural fasteners.

DR. WALKER: Did you say it is locked tight?

MR. HARDY: It is standard practice to lock wire or lock tight electrical connectors.

DR. COVERT: This is aluminum tightened into a steel. Is there some sort of a frictional seizing, like if you put brass into steel and tighten it down, why, then there would be a sticking there.

MR. HARDY: This plug is steel.

DR. COVERT: Where did I get the idea it was aluminum?

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MR. HARDY: I'm not sure.

(Laughter.)

MR. SUTTER: The only safety of that system, then, is quality control?

MR. HARDY: That is correct.

MR. SUTTER: And it is a single item.

MR. FEYNMAN: Well, it is supposed by itself not to be a problem because the primary ring is supposed to hold. That is why it isn't at the same level as we are now thinking about it, and we now are thinking about primary rings failing, and that hasn't been communicated that the check valve therefore becomes a critical item.

MR. WAITE: Would you say if the plug were left out that you would have O-ring failure?

MR. HARDY: No, I would not say that. I would not like to have the plug left out because, as I experienced for occasions of blow-by, if it was in one of these cases, then I think I would be in for trouble.

MR. WAITE: Then you would have flow that would cause the secondary seal to degrade?

MR. HARDY: That's right.

MR. ACHESON: In cases of blow-by past the primary ring, what is your experience on the condition of the plug and the check port?

MR. HARDY: In every case that we have seen

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blow-by, we have post-recovery, in examination of the article, has shown that that blow-by is limited to soot deposit back here, and on no occasion have we seen any violation of the secondary O-ring or any violation of the leak check port here.

MR. ACHESON: I see.

MR. HARDY: If I might go on to the next one and mention that this track stays open for the time being, and now I would like to look at the B-1 event.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-46]

MR. HARDY: Now, the B-1 event follows the blow-by on the primary O-ring, and it says that—and these are for the secondary O-ring seals, and therefore I establish flow past both O-rings, the anomalous smoke starting at or near lift-off could be from that field joint, and it could be from gas passing through both O-rings. And I think it has already been mentioned the grease in this joint. From that grease we would see black smoke. We have identified in the investigation a suspect secondary O-ring which is indicated in the close-out photos, and let me hasten to mention before we show you this that we are still collectively trying to interpret these photos.

It is not immediately obvious what we are

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seeing, and that is the reason I classified it as a suspect close-out photo. I have some viewgraphs, and Bob looks like he has got a big blow-up, and so let's put the viewgraphs on, please.

The next viewgraph.

(Viewgraph.) [Ref. 2/13-48]

MR. HARDY: No, I'm sorry. They are out of order.

Do you have the picture viewgraphs?

Take down the viewgraph on the right hand screen.

Jack, could you help me back there get the picture up?

(Viewgraph.) [Ref. 2/13-47]

MR. HARDY: You can probably see better on what you have there, but what you are looking at, maybe I can locate you the features and then you can look at the blow-by that you have.

What you are looking at is a clevis. This clevis has been prepared, this is the upper side of it, and this is the inner leg that you are looking at. The primary O-ring is here. This is a land in between the primary and secondary. This is metal, and this is the secondary O-ring right here. And these are close-out photos that are taken as a part of the process.

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I might mention to you this joint. The metal, before the O-rings are put on, the metal is greased. We put a heavy coating of Conoco grease, which is a stiff grease that is put on this joint, and it is put on there for two reasons. One is, the primary reason is to provide corrosion protection for that joint, and the other is to provide ease in assembly of the installation of the O-rings. The O-rings are delivered prelubricated from Thiokol to Kennedy, certified ready to install, and the secondary O-ring is put on, and I am not going to go into great detail about this because you are going to hear a lot about it and maybe even see it tomorrow, but the secondary O-ring is put on around the vehicle, over this joint, and then the primary O-ring is put on.

And what we view, the area right here in the secondary O-ring, that gives some appearance of depression and raised area, and it also gives some appearance, and you can probably see it a lot better in what you've got, of the reduction in cross-sectional area of the O-ring.

Now, if I could have the next viewgraph.

(Viewgraph.) [Ref. 2/13-48]

MR. HARDY: We have a picture of one here that has been somewhat enhanced, and again, not too clear up here is the primary O-ring, and this is the land between

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the two grooves, and this is the secondary O-ring, and this is the area of interest, and here is the area which concerns me, indicating some reduction in cross-sectional area.

DR. WALKER: It looks like a gouge almost.

MR. HARDY: Possibly.

MR. FEYNMAN: What is that slightly white area? Do you mean that slightly white or black area in there?

MR. HARDY: In here.

Let me explain what we do know. What we do know is that there is, as I mentioned, this joint is greased rather heavily, and we are quite confident that this is grease that is smeared across here either at the time of application or installation of the O-ring. Again, without getting into too much of what you are going to hear tomorrow, after the O-ring is put in place, the operators with surgical gloves, with greased fingertips, do by procedure go around and push this O-ring into the groove, make sure it is fitting in the groove before they mate the joint.

So there is a question as to whether this represents grease smears also or whether it is some form of a distressed O-ring, and I use that term because I would not describe it as a twist; I would not describe

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it as, at this point in time, as a deformation. I will only say that without being able to totally interpret it at this time, it is a piece of data we are working with. We have some photo enhancement activities going on with expertise that we believe can give us more insight into whether or not that is grease smeared across the O-ring or whether that is in fact some form of defect in the O-ring.

GENERAL KUTYNA: Is this the flight motor or is this just a sample?

MR. HARDY: This is the flight motor.

GENERAL KUTYNA: Is this the flight joint?

MR. HARDY: This is the flight joint.

DR. WALKER: That particular one down there?

MR. HARDY: It is in the correct hemisphere. It is on the inside, and it is not in the same quadrant as the point where we see the blowing leak. All I can say at this time, it is in the right half of the motor. It is in the half of the motor adjacent to the external tank.

CHAIRMAN ROGERS: Can you tell us how this picture was taken? I assume—when? When was it taken?

MR. HARDY: It was taken as a matter of procedure when this joint was being made.

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CHAIRMAN ROGERS: What is the purpose of the picture?

MR. HARDY: It is what we refer to as closeout photos, and there are a number of operations here at Kennedy that require that closeout photographs be made. These closeout photographs can be used for many purposes. In some cases they can be used as a quality assurance validation, and in some cases they can be used in anomaly investigations.

CHAIRMAN ROGERS: What about this case?

MR. HARDY: In this particular case, Horace, I will let you explain it.

MR. LAMBERTH: These were closeout photos sir. We had photos that covered the entire 360 degrees of the putty lay-up and so on.

CHAIRMAN ROGERS: Somebody looked at this ahead of time but didn't notice it?

MR. LAMBERTH: That's right.

DR. COVERT: The inspector did not call attention to this change in the gap?

MR. LAMBERTH: No, Sir.

DR. COVERT: Well, how much could you see on that without having to call it? What is the spec on that?

MR. LAMBERTH: We did not have a spec.

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MR. RUMMEL: There appears to my eyes to be a ridge, a small ridge around the secondary O-ring. Is that——

MR. HARDY: We believe that is the grease that it picked up when it was rolled over the edge of the groove. That is our best guess.

MR. LAMBERTH: George, all of the engineers and all of the techs and the QC and advisors looked at this photo still feel that this is a result of the grease streaks and shadows, but nothing was written on any notes or anything when we actually made the close-out photos and made the joint.

CHAIRMAN ROGERS: Do we know who it was that signed off on this?

MR. LAMBERTH: Yes, sir.

CHAIRMAN ROGERS: Is that person—does he say he didn't notice this?

MR. LAMBERTH: We haven't interviewed that particular person formally yet. On all of the notes, they make notes of anything that they notice or anything, and none of the notes—all of the notes have been reviewed, and no notes specify anything.

CHAIRMAN ROGERS: Is there more than one person who does this?

MR. LAMBERTH: Yes, sir.

CHAIRMAN ROGERS: In other words, is there

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somebody who looks at this and signs off on this, and then somebody checks him?

MR. LAMBERTH: It is about four or five techs that put the O-ring in, and then they go around with gloves like George said, and QC goes around and makes a circle and looks.

CHAIRMAN ROGERS: But I am talking about the picture afterwards. You take the picture, and the picture is taken in order to see if——

MR. LAMBERTH: The picture is not part of the logoff records. Sometimes the picture does not get looked at.

CHAIRMAN ROGERS: Why is that?

MR. LAMBERTH: Well, there is a requirement to make these photos and document that they have been made and log them. The buy-off is the actual visual inspection at real time. The photos are for records.

MR. FEYNMAN: You talk about the streaks on the metal. For the moment I am not concerned with that on the O-ring, but the streaks like that—are streaks like that on the metal very common?

MR. LAMBERTH: Yes, sir.

MR. HARDY: The grease streaks would be, yes, sir.

MR. FEYNMAN: They look more or less like

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that.

MR. HARDY: Yes, I believe that is the case.

MR. FEYNMAN: Thank you.

DR. WALKER: Could I raise a question?

Shouldn't you expand your scenario backward, because suppose there's a low point in the putty and the putty doesn't seal properly? That could be a problem as well.

MR. HARDY: We do have the putty in the failure scenario. One particular aspect is the putty is cold, and how does cold putty affect the performance of the seal?

DR. WALKER: But suppose there is a low point in the putty here which leaves a gap? Is there some inspection of the putty? Does somebody measure the uniformity of the putty?

MR. HARDY: Yes, it is, and I need to point out to you, I don't know the exact dimensions, but as you see that putty there, it is up I'm going to say three-fourths of an inch or so, or in the neighborhood of three-fourths of an inch, and that is compressed into I would say a third or less of that dimension.

DR. WALKER: But there could be a gap in there.

MR. HARDY: We do believe, however, that the

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primary O-ring erosion occurs when we have what we refer to as a blow hole through the putty. So we can concentrate hot gas on the primary O-ring. So weak places in the putty, weak relative to other places in the putty where the gas would go through first could be a contributor to primary O-ring erosion, the primary O-ring erosion that you heard about yesterday or the day before, I have forgotten which now, in Washington, which we believe is a limiting failure mode.

CHAIRMAN ROGERS: I would like to go ahead with this.

When was the picture taken prior to launch?

MR. LAMBERTH: We stacked in December the 7th, so it was right about that timeframe.

GENERAL KUTYNA: On the back of the picture it says 12/7/85.

CHAIRMAN ROGERS: Now, you said sometimes these were looked at for safety purposes and sometimes they weren't.

How do you make that decision?

MR. LAMBERTH: Well, usually these are placed on record, and many times they are looked at by a group of people just looking at closeout photos, and review might occur a week or several week afterwards. The procedure does not require a review and a verification

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of the closeout photos. It requires a verification from the staff that the closeout photos are taken.

CHAIRMAN ROGERS: Was that practice changed? Did you used to do that? In other words, did you use to take the picture and then look at it before you signed off?

MR. LAMBERTH: No, sir.

CHAIRMAN ROGERS: It has always been that way?

MR. LAMBERTH: Yes, sir.

MR. HARRINGTON: The corresponding requirement for this is for postflight analysis of an anomaly.

MR. LAMBERTH: This is for questions that might come up later in the paper or anomalies.

MR. RUMMEL: A picture is taken at the time of the visual inspection, prior to the accident

MR. LAMBERTH: Yes, sir. We have a requirement that after we put the joint in the configuration that you see it here, from the time we start this joint to the time we mate is 24 hours, so the picture and the work all gets done within that period of time. We actually did this one in about ten hours.

CHAIRMAN ROGERS: Just because you are going to be asked eventually a lot of questions about it, what is the purpose of keeping the picture if you are not

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going to look at it before launch?

MR. LAMBERTH: Just like we are doing here now, sir, so that if some question comes up on the paper later that some anomaly or some question——

MR. HARRINGTON: A record of the condition that gets closed up by the flight configuration that you can look at later to examine it in the case of an anomaly, which we are doing.

CHAIRMAN ROGERS: But it seems illogical if you are going to save it for a record to show the failure.

MR. HARRINGTON: Well, you see, if we didn't know today there were two O-rings in there, and we didn't have a picture, we would have a difficult time ascertaining that somebody did put two in. We would have to take the word of the paperwork trail and the inspector. In this case we have a picture that says they definitely were there.

CHAIRMAN ROGERS: I just don't follow that. Why is it better after the fact to look at it than before the fact?

MR. LAMBERTH: Well, let me clarify that. Real time we have a buyoff by the technician that does the work, by a contractor inspector and a NASA inspector that this job was done properly and all the inspections

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were made, that is, by visual and buyoff procedure.

CHAIRMAN ROGERS: Let me press that point just for a moment.

So you have two humans or three humans that look at it.

MR. LAMBERTH: Yes, sir.

CHAIRMAN ROGERS: And then you have a record, which is even better, because it shows it.

Now, if you don't compare the record ahead of time with what the humans have done, and now you have a record that the humans failed because they didn't see this, and assume that's a fact, now, what is the point of having the picture?

I mean, it would seem to me that this may explain the failure after the catastrophe?

MR. FEYNMAN: Might I make a suggestion? Just as a suggestion as to what, it might be a very logical reason for doing this, whatever procedure you use, no matter how many inspections you use with people and whatever, then you have to decide sometime to close it up. It would be very handy to have a record to look at later in case there is some kind of a thing that you didn't know was important because you find some kind of a flaw, let's say blow-by. Later on you want to discover what did you do. You discover that every time

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there is a blow-by, there is a little extra grease mark that you hadn't realized was of any importance, and therefore the record would be very useful when looking at the thing later to discover whether something that you are not considering as important, which you are allowing to pass. That is, let us suppose that the usual rule is that when an ordinary human being looks at this picture he doesn't think there's anything wrong with it, which as a matter of fact, I do think there is nothing wrong with it. So it would pass everything, but the real thing is you would like to get a record of what it looks like so that in case later on you discover there is some other condition that too much of this grease or something like that, or a special color grease that you never knew or that you changed or something like that becomes of importance, but you hadn't realized that when you were putting it together.

And I see therefore some logical reason to have such records.

MR. CRIPPEN: Mr. Chairman, I guess that is our standard practice for keeping records, not for inspection, and in fact, I would submit that probably somebody—if somebody had inspected this picture while it was being put together, at a later point, and we had not had a problem, that it would have probably gone

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through and been looked at because it is still not obvious to even all of the experts looking at it today that that is really a problem area. It is just something that we—since we know we had a problem in this area, that looks a little bit different and people are out exploring.

DR. COVERT: In fact, Mr. Chairman, the fact that there is no specification calling for more than a minimum acceptable gap or a maximum acceptable gap suggests what Mr. Crippen is saying is so. If there is no spec to measure it against——

CHAIRMAN ROGERS: Well, I guess I understand. I am not convinced. I think it is going to be difficult to explain if you have—maybe the answer is that you didn't think you could find anything by looking at your pictures, maybe that is a better answer.

MR. RUMMEL: I think there might be some validity to that because the human eye looking at that object in three dimensions I think usually is far more accurate than photographs because of the perspective.

CHAIRMAN ROGERS: That is a better answer to me. It seems to me the human inspection is better than a visual inspection, a photographic inspection, but I'm not sure people are going to be convinced by that.

VICE CHAIRMAN ARMSTRONG: May I ask, the

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yellow, is that the putty or is that the insulation, the yellow material?

MR. LAMBERTH: That is the putty.

CHAIRMAN ROGERS: In this case, do we know who the ones were who looked at it visually and signed off on it?

MR. LAMBERTH: Yes, sir.

CHAIRMAN ROGERS: How many were involved?

MR. LAMBERTH: It is four people who usually handle the O-rings, and then we have a NASA QC and a contractor QC, and the technicians.

DR. COVERT: Could I ask a different question that has to be asked at this point, not directly relevant. In this organization, does QC report directly to the Director independent of the manager?

MR. LAMBERTH: Yes, that is correct.

MR. SMITH: Let me clarify the point. There is a quality organization within the Shuttle Directorate, the Shuttle Operations, under Bob Sieck, that does report to him. I have a center quality organization that is a procedures and holding function. That does not do the detailed inspections. The detailed government inspectors in this case do report to Mr. Sieck.

DR. COVERT: Is there the possibility—and I only raise this as a devil's advocate viewpoint, but is

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there a possibility that there may be a mild conflict of interest here because the guy on the one hand who is responsible for quality control and safety of the Space Transportation System reports to you, and you are also the man who is responsible for maintaining schedule and all of these sorts of things? And I don't mean to imply you are putting any pressure on your safety people or anything like that.

DR. SIECK: Well, that is a tough one to answer.

DR. COVERT: I did not mean to say when did you stop beating your wife?

MR. SMITH: Gene, I have gone through that process several times since I have been here. I have asked that question, I have talked with people. I have in my own mind been absolutely

convinced that we do not have a conflict of interest, primarily because the first charge is a safe launch and not schedule. And I know at least 300 times I went through and made sure to my personal satisfaction that that was not a conflict because I recognized the apparent conflict.

DR. COVERT: You understand——

MR SMITH: Absolutely, I understand the question.

DR. COVERT: Thank you, Mr. Chairman, for that.

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CHAIRMAN ROGERS: Going back to the picture for a moment, do you have pictures of, a lot of previous pictures of the O-rings from previous launches?

MR. LAMBERTH: Yes, sir.

CHAIRMAN ROGERS: And do you now, looking back at it, find other suspected areas?

MR. LAMBERTH: I am not prepared to answer that, sir. I don't know how many we have looked at. I am not aware of that. I need to check how many we have gone back and looked at.

CHAIRMAN ROGERS: How did you find this one?

MR. LAMBERTH: This was the inspection. We formed a special team to go back and re-review all of the procedures involved in this specific launch and review the closeout photos and all documentation, and it was reviewing these closeout photos where we picked up this.

CHAIRMAN ROGERS: But that was just 51-L that you reviewed?

MR. LAMBERTH: Yes, sir.

CHAIRMAN ROGERS: Thank you.

GENERAL KUTYNA: The team that stacked—this was a restack of this particular segment, is that correct? Didn't you stack it once and take it down and restack it?

MR. HARDY: No. I think that was incorrect

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information. There was another segment that was initially designated to be stacked here, and there was a redesignation of segments.

MR. LAMBERTH: George, we are going to cover that in our briefing, and we will go through that specifically, but the answer is no, sir.

MR. HARDY: Let me just say this about the picture. I cannot interpret the picture. I do want you to know, however, that we have engaged and are in the process of engaging what we believe to be the best photographic enhancement and photographic interpretation assistance that is available, and that should be going on, if not in fact today, within the next day or two.

CHAIRMAN ROGERS: Could I just make this one comment?

I appreciate, as Chairman of the Commission, that you showed us this photograph and enhanced it and made it available to us. I mean, it is the kind of cooperation that I think is very important, and you are to be commended for it.

MR. HARDY: Let me just mention one other thing that we are also going to do in the task of trying to interpret this picture is that we have set up on the full scale segment to try to simulate as nearly as possible what we see here, and prepare the joint, put

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the secondary O-ring on, the primary O-ring, and we will do at least three things that we can think of and anything else that we can think of around that O-ring, that we will twist it, we will smear grease on it, trying to match that pattern of grease as nearly as we can. We will indent it, deform it. We will do the things that we think might simulate that.

Then we plan to take photographs as best we can matching the camera angles, the lighting conditions, the distances and so forth, to see if we can make any reference photographs which would lend any support to interpreting what we see there.

DR. COVERT: George, just one other thing, and I hate to always go back to what Larry Mulloy said, but he led me to believe on Tuesday that in fact people did measure the diameter of the O-ring at the time that the O-ring was taken from its envelope, that there was essentially a receiving inspection procedure?

MR. HARDY: I am sure Larry knows.

DR. WALKER: That was only every two feet.

DR. COVERT: But if there was a ding in it——

MR. HARDY: I know Larry knows how this is done, and maybe we have a miscommunication here, but the O-ring is measured at Thiokol. It is lubricated at Thiokol. It is placed in a sealed bag, and it is

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delivered here certified flightworthy, and the inspection here is to ensure that the bag has not been opened, to make sure there has been no tampering with it.

But there is no requirement on KSC to verify the O-ring diameter. In fact, with the grease on it, it would be difficult for them to do.

DR. WALKER: So Thiokol measures the O-rings.

MR. HARDY: Yes.

DR. RIDE: Has that always been the case, that KSC hasn't verified the diameter?

MR. HARDY: It has not always been the case that KSC received it unlubricated, and I don't know whether you verified diameter or not, Horace.

MR. LAMBERTH: There was a time that we did do inspection of the O-ring, and I am going from recollection. I think part of the diameter checks, the interval was part of that.

DR. RIDE: When you used to do that, did you always find that they were within spec as measured by Thiokol?

MR. LAMBERTH: I asked for that record, Sally, and I know we had some questions about the O-rings, and there had been some that were, say, not usable, but I'm not for sure what was the reason. But what George says,

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today, we receive them in the bag, we inspect the bag, the bag is intact and sealed, and unless we see something very obvious when we take the O-ring out of the second bag, it is a plastic bag inside of a paper bag, and unless we see something obvious, we just install the O-ring.

MR. MOORE: Our task force is going back to ask for those records Sally.

CHAIRMAN ROGERS: Assuming this is some kind of an anomaly and not just a distortion of grease on the photograph, that there is some kind of a defect here, is it possible that the defect would be on the part of the manufacturer or the contractor, or would it be always in connection with the installation?

MR. HARDY: I think it could be either one.

DR. LUCAS: The same contractor does both.

MR. RUMMEL: Could it have occurred during the shipment or storage, where the O-ring is wound up and compressed? That sort of thing.

MR. HARDY: I think that is possible.

MR. RUMMEL: I think it would be normally expected if, in fact, that is the way it is stored.

CHAIRMAN ROGERS: Thiokol is responsible for that, too?

MR. HARDY: Yes, sir.

MR. SUTTER: Is there a lot of equipment that

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is inspected by whoever makes it, and then it is just put on the vehicle without a receiving inspection here?

MR. HARDY: I'm not aware of a lot of that equipment, but I could defer to Mr. Lamberth at KSC.

MR. LAMBERTH: I think the answer is no, there is not as lot. I think the purpose—it is my understanding when this decision was made, it was a time consideration, that the process we go through in putting up the putty and everything, you know, you do have a requirement to try to lay the putty up and put the O-rings in and make the joint in 24 hours, and I think that the time to try to inspect the O-ring to that depth and lubricate it and everything was why the decision was made to go ahead and lube it and ship it in that way.

MR. SUTTER: It seems to me the guy who is lubricating the O-rings shouldn't be fooling around with the joint.

Was this considered to be a noncritical item, or it just seems to be a false economy.

MR. HARDY: It has always been a critical item, and one of the reasons, too, was to have it lubricated at Thiokol, the manufacturer, the motor manufacturer's plant, was because the lubrication of the O-ring is a critical operation.

MR. SUTTER: Doesn't Thiokol join this thing?

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MR. HARDY: Yes, sir, the same contractor.

DR. WALKER: I thought Parker manufactured this.

MR. HARDY: The rubber is from DuPont or 3M. It is molded by Parker-Hannafin, and then it is joined together and made O-rings out of it by a company called Hydropack, which delivers it to Thiokol.

Just one other thing that did have to do with the decision to have it packaged at Thiokol was that there was some experience—and Horace, you may remember this—that this O-ring is a very large O-ring with a large diameter, and there was experience here at Kennedy of getting it contaminated when putting grease on it, getting contamination on the grease. This was having to be done in the Vertical Assembly Building where it was not contamination controlled, and so it was judged that it could be processed at Thiokol, we could ensure that we got the right grease on it, and we could deliver it without any contamination in that grease.

DR. COVERT: Is it inspected by a NASA inspector at Thiokol prior to bagging and shipment?

MR. HARDY: Yes, sir. There are mandatory inspection points requiring government inspector buyoff.

Now, if I can leave the O-ring, that suspect

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O-ring for a moment, the other observation that is of great interest to us regarding the part of th scenario that says the secondary O-ring leaked is the launch environment and the temperatures.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-49]

MR. HARDY: This shows the temperatures at the various nodes indicated here around the SRB, the right hand SRB, and this goes back roughly 70 hours, I believe it is, from the time to

launch, and the arrow here is at or very near the time of the launch, and this shows what the temperatures did through the night and where we were at the time of the launch.

MR. FEYNMAN: Is this calculated because there is a delay in cooling the steel? Otherwise you would have no way of predicting where it was going to be—the delay of the arrow is the inertia, the thermal inertia of the steel?

MR. HARDY: The model is just set up to run it for this period of time, and we just picked the actual launch time up here, but you can see, I believe, that the coldest temperature was in this area here. At the time of launch it was around 25 degrees. The other side of the booster was at near 47 degrees.

DR. COVERT: Does the sun shine on that side?

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MR. HARDY: I believe this side here is exposed to the sun, right in this area.

GENERAL KUTYNA: And where do we postulate the leak?

MR. HARDY: In this area here, this general area, well, first of all, somewhere on this side of the booster, and the very evident leak at the point in time, about the 58 seconds, is in this general area here.

Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-50]

MR. HARDY: The question was asked this morning about the gradient in the propellant. This is 51-L's curves. This shows the propellant next to the case, the insulation, and then the outer propellant, the outermost propellant and the inner propellant, and I believe that is about 19 degrees.

Of course, the mean bulk, which is the average temperature you see here, and you can tell that over the temperature excursions of this type, the temperature changes practically not at all.

The next viewgraph will show you that.

DR. COVERT: Could I have a copy of that, George?

MR. HARDY: Sure.

The next viewgraph, please.

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(Viewgraph.) [Ref. 2/13-51]

MR. HARDY: Just for information, I have shown on a previous flight, this was flight 51-C, the outer propellant temperature and inner propellant temperature on that particular flight was about the same, about 18, 19 degrees difference. The mean bulk temperature on this flight was a little bit lower than on the previous flight, a little bit lower on 51-L.

Could I have the next viewgraph, please?

DR. COVERT: What caused that cycling of the temperature?

MR. HARDY: It is the hourly cycling of the ambient temperature.

DR. COVERT: The time scale I didn't read carefully.

MR. HARDY: If these viewgraphs are not in your handout, I will make them available.

GENERAL KUTYNA: They are not.

MR. HARDY: Then I will make those available.

(Viewgraph.) [Ref. 2/13-52]

MR. HARDY: In summary, from what you have seen off of the curves, just putting it on a table here with the model, using the coldest ambient temperature and the relative humidity, the wind direction you can see there, and the cold sky for radiation, this is the

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cold case, the nominal case using the more nominal sky temperature for radiation, the aft segment in the coldest case. We predict the case temperatures at launch would be around 25 degrees at one location, and around the other side, about 47 degrees.

A more nominal prediction you see wouldn't be a vast change in that. There wouldn't be a vast change in that, and for the forward joint, the forward segments, the joint temperature on the right hand booster would be about the same.

Now, we are going to talk about these temperatures. I just wanted to show you that the forward segment on that side and the aft segment temperatures were essentially the same.

Could I have the next viewgraph, please?

MR. ACHESON: And we can assume the O-ring temperatures would have been the temperatures corresponding with those numbers?

MR. HARDY: Yes, very, very close.

MR. FEYNMAN: What did you mean by forward?

MR. HARDY: The forward field joint, these are these two joints here, and these aft field joints on the right hand booster at approximately the same clock orientation, or about the same place.

(Viewgraph.) [Ref. 2/13-53]

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MR. HARDY: Now, this is a busy chart, and I have it in your handout, and there was some interest expressed in this, I believe, on what are the dimensions of the joint, and I will not try to go through all of these, but I will point out some of the more significant ones having to do with the establishment of squeeze on the O-ring.

This is—well, first of all, this is a sealing surface on the tang, and this is the orient locations, and this is a picture that shows this just a little bit bigger, these radial dimensions on this sealing surface, and this interclevis leg sealing surface, dimension C and dimension D on 51-L were as shown here, 144.574 inches, and 144.566 inches.

DR. COVERT: That is an average of a bunch of readings around the ring?

MR. HARDY: Yes. This is done on the lathe.

VICE CHAIRMAN ARMSTRONG: Is this at a specific temperature?

MR. HARDY: At ambient. This is done at the case manufacturer, which is Rohr, Incorporated, in San Diego, and it is done on the lathe, and it is done, Neil, at just an ambient temperature.

DR. WALKER: It is not done after they are flown and recovered?

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MR. HARDY: No, it is not.

DR. COVERT: So this is as delivered to Thiokol for loading?

MR. HARDY: That's correct, the first time. Yes, specifically dimensions C and D.

Now, I am going to talk about some other dimensions that are made after every use, but this is the dimensions, the regular dimension on these sealing surfaces, and that is done the first time on a lathe at delivery, and in fact, that could only be done on a lathe. These steel, relatively thin-walled steel cases with the large diameters they have, there is no way you could actually make those—you could make some pie tape checks on it, but it is just not round. It never sits round, perfectly round. There are, however, requirements on the contractor Thiokol that if they have any occasion to repair any of the sealing surfaces—and there are repair procedures—that in the event there is any salt water effect—and I mentioned where you have the grease on it to

prevent any corrosion—but if there is any pitting corrosion, there are specifications that allow repair to remove that pitting corrosion. There are specifications, and I don't have the numbers, that tell what the allowable depth of the pit is that could be repaired, which can't be repaired,

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but in any case, where the sealing surfaces are repaired, the contractual requirement on Thiokol is that they still have to remain within the specification limits.

Now, this is the spec limit for this dimension, 144.373 plus or minus .004. You can see where 51-L was. You can see that if for any reason that segment had to be repaired, it could be repaired, but it could never violate that dimension, even after repair.

The other dimensions of interest are dimension A, which is specified as a minimum, although there is a maximum, too. That is the tang width of 51-L dimension was .789. The spec allowable is—this should be reversed—is .792 to .777. The other dimensions of interest is the gap of the clevis on 51-L that was .841. The spec allowable—and this should be reversed, too, is .842 to .827. You can see that this was on the high side within spec, and this, of course, was just about, I guess, close to the middle of spec.

Some other dimensions of importance is the O-ring groove width, and that is .310 to .305, max. The other is this O-ring groove depth, and that is .216 to .209. 51-L was .211. And when I say 51-L, I am talking about 51-L aft field joints specifically.

The other dimension which was specified was—

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is this gap here between the outer leg—that is, the gap between the outer leg of the clevis and the tang, and that is specified at .032, and that is controlled by the thickness of the shim.

Remember, we said in every case after the pin is put in, around each pin is put a shim. So that is a well controlled dimension.

So then in the use of these dimensions, one calculates a maximum gap, and in the case of 51-L, that static gap was .020.

Now, let me just make one other point, and then if anyone cares to pursue the details of this, there is an actual formula that is used to calculate that. Let me just mention one other thing, that unpressurized, the calculations are made for a gap size, a minimum sealing gap size and a maximum sealing gap size. The calculations are made assuming that the tang portion is not concentric. In other words, it assumes that on one edge I am up against this leg here, and the only thing I have got separating me is .032, and then all the rest of the dimension can gather such as the other side, is the maximum opening.

So that assumes that that piece is eggshelled to the maximum extent possible to give you the maximum gap possible with the dimension that you have here.

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The 51-L O-ring squeeze—and this assumed a minimum O-ring diameter—the O-ring is specified .280 minus .003/plus .005, and we assumed in all of our analysis and calculations on 51-L that it was minimum O-ring size, .277, but the static squeeze at room temperature for the O-rings for 51-L was .0395, and that is 14.7 percent, and calculated at 26 degrees Fahrenheit, which accounts for shrinkage of the O-ring, it was .0356, or 13.4 percent squeeze.

Now, the minimum spec squeeze allowed is 7.54 percent. I would only point out that 51-L, in terms of actual dimensions, aft field joint, would be somewhere I would say in the average dimensions that we could stack up on these things or slightly better than average.

Could I have the next viewgraph, please?

DR. COVERT: Wait a minute. Stop, please.

MR. SUTTER: Have you ever measured these dimensions when you put, say, the flight loads on it or put torsion and bending on it?

MR. HARDY: Yes. Go back to that viewgraph, please. It is not shown here, but remember the leak check port that goes through here? In the structural, static structural load test that we ran, and also in the hydroproof test, each one of these segments is hydroproofed to about 10 percent or 5 percent, I believe, it is at maximum expected operating

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pressure, we have on a number of those units taken that plug out because it didn't enter into the test that we were running, and put a dial plunger, a little device that has got a spring-loaded plunger that rests up against this leg here, and we zeroed that condition in, the static condition, and then as we brought pressure up to the ignition transient, up to max pressure, we measured the increase in that gap. We plan to do some more of that. We want to—well, first of all, let me say that what that does is measure the deflection of this joint as a result of the pressure load, and that is essentially a line load that is pulling through the bulkhead on that end, and it is pulling through the bulkhead on that end because the joint is tending to, or this clevis is tending to rotate around where the pin would be here, and that puts forces this way and this way to tend to spread it.

We have also done testing in the static structural test program where we had the two segments joined together in the test facility, and we pressurized those segments, and then we applied at this point here the bending load and the loads at the back side here, the design loads, and then we measured the deflections of the joint the same way, and we also, of course, measured and sealed any leak if we had any, and I might say that

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in that test program, which went over a number of months, the joint was pressurized, I can't remember how many times, but I'm going to say in the tens if not in fact a hundred cycles, and we never changed the O-rings. We left the O-rings in there.

So that is not, of course, the way we fly. We change the O-rings after every flight, but the O-rings are rather tenacious to cyclic loading.

Now, we are not satisfied totally today with the data that we have in hand, so in the pursuit of this failure analysis, we are continuing to measure this deflection or have a test program to measure this deflection where we are going to go around at eight different locations and we are going to drill seven more holes to correspond with the one hole where we have the pressure port, and put eight gauges in there, and measure it circumferentially.

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DR. FEYNMAN: I believe we have seen some pictures that were shown to us of the gap changing from .042 to something like .061. Is that what we were talking about?

MR. HARDY: .042 to .061, yes.

DR. FEYNMAN: Is that a result of those experiments you're going to try to repeat?

MR. HARDY: Yes.

DR. FEYNMAN: What does that mean? When I look at these numbers here, what would that mean when the pressure is put on the gap increased by what—excuse me, decreased by what, et cetera? How do I use those numbers?

DR. HARDY: I will show you that on the next viewgraph.

DR. COVERT: Before we do that, George, I was going to ask: How much does the change in the inner radius, when you take the temperature of this thing and drop it from say 70 degrees down to 25 degrees?

MR. HARDY: That calculation has been made, but I don't remember it, Gene. I don't have a recollection. You're talking about the basic shrinkage from temperature?

DR. COVERT: Right.

MR. HARDY: Of course both sides are going to

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shrink some. There may be a differential there.

DR. COVERT: One side goes into compression, the other side gets stretched.

MR. ACHESON: Can we assume that when you stack the two segments, that it is really impossible to push the O-rings out of the groove by some accident? And even if you did, it would probably just produce a tighter seal?

MR. HARDY: Yes. Could you go back to the viewgraph on the right, please? I have to have that viewgraph to show that.

(Viewgraph.)

If you notice, and I don't know if this would show too well or not here, but the O-rings are in the groove here, and you will notice that the tang is designed so that this portion of the tang can pass the O-ring without any engagement, and in fact this tapered surface here will contact here (indicating) before anything gets to the sealing surfaces. And it's designed such that as this taper here rides in on this taper to avoid any scrape across the O-ring, now I wouldn't say that couldn't happen, because if in fact you had an O-ring that was not pushed back in the groove, that could possibly happen to you. You could in fact engage the O-ring and damage it.

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MR. RUMMEL: Doesn't that assume perfectly concentric mating? Why can't that be a little bit off center so that in fact you, through the holes, take the pins? Why wouldn't you have a possibility of shearing part of the O-rings? And as it slides in, if it slides in metal to metal, if it isn't exactly concentric——

MR. HARDY: I see what you're saying. You're saying it is not concentric, and this does come into where it comes in right here.

MR. RUMMEL: It would be a masterful job to hold anything this big perfectly concentric.

MR. HARDY: If I could, I would like to defer the answer to that question to the presentation that is planned for you on the mating of this joint. That is in fact the primary reason that it is necessary, when mating, when I've got this segment, if you rotate that 90 degrees and I've got this segment on a crane, and I'm lowering it into position where I can go into this crevice here, the procedure requires that that be halted at some point there, and reference dimensions be made to ensure that it is round enough that that is not going to occur. But the details of that will be explained to you tomorrow, and I would like to defer that, if I could.

If I can answer another question, I would be

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happy to do it, but I think you are going to actually see a demonstration.

MR. RUMMEL: The only question would be that if indeed there was a metal-to-metal slide, then theoretically you could in fact pare the O-ring at that point. Is that correct?

MR. HARDY: That is correct. Could I have the next viewgraph, please?

(Viewgraph.) [Ref. 2/13-54]

MR. WAITE: Have you ever done any pressure checks, and then pulled them apart and had an O-ring cut as a result of this?

MR. HARDY: I believe there has been some experience of what's referred to as nibbling, or damage, and I'm not speaking here at Kennedy. I don't know that they have disassembled—well,

they have disassembled some, too. But at Thiokol in the test programs there have been some occasions where they have taken them apart after they've been assembled, and they see some nibbling, or damage, on the O-rings.

Now that has been—and then there's a big question: Did that happen when I put them together? Did that happen when I took them apart?

MR. WAITE: But they did pass pressure checks?

MR. HARDY: They did pass pressure checks,

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yes. If I could, I would just like to explain a little bit of the dynamics of this seal, and I will relate it first of all to the generic type description of how the seal operates, and then I will give you some information, albeit preliminary, because we are still trying to refine the analysis on 51-L.

This picture here represents step one, and that is at time zero, pressure zero, and that is a static condition. The primary O-ring is somewhat exaggerated here, but it is back against this side of the groove. [Ref. 2/13-55]

The second area O-ring is against this side of the groove, and that is because of the pressure test that I mentioned earlier. We put 200 psi in there.

DR. FEYNMAN: When you put the 200 psi, do you often have some leakage temporarily, at least, through the primary? Do you make bubbles in the putty?

MR. HARDY: I can't recall any cases recently. There may be some in the early test program when we assembled these horizontally. We had a few more problems with them, as you can imagine, and we have had to take some segments apart. When they were assembled on the test stand, they are tested horizontally, and we have been unable to pass a leak test, and had to take them apart and put them back together.

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But in any case, the flow starts across here. The first step is that this O-ring starts what we call pressure actuating, or moving toward the other side. Now the distance, you recall, that I told you that it could be depending upon dimensions could be 15/ to 30/1000, roughly. I'm going to talk about the gap here in a few minutes.

As flow continues, this O-ring moves completely to this side. And then, as the pressure builds up, it actually forms what we refer to as an extrusion seal. Now this is an exaggeration here a little bit. I don't think it goes to a square in that corner, but we do believe that there is a slight dimpling, if you will, of this O-ring into that gap.

Now that makes the gap very important.

DR. COVERT: George, is that edge of the O-ring groove broken? Or is that a fairly sharp—

MR. HARDY: It has a radius on it, and I don't remember the dimensions.

DR. COVERT: The important thing is it is not square.

MR. HARDY: We ran some tests, for instance I think it might be of interest here, on whether or not we stretch, rupture the O-ring, and we run tests pressurizing these O-rings up to 3000 psi, and we use

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them in the neighborhood of 900 and 950 psi. And also we have done that at various temperatures. And we also have, as I mentioned in the structural test article, recycled the O-rings around 1000 psi for 50, 60, 70, 80 times.

DR. FEYNMAN: Again, when the ring is moving because of the flow and hasn't finally set in, is there some chance of a little bit of leak? The reason I ask this is because Mr. Weeks no-

ticed that the motion in the nozzle ring is 110, and not just 15/1000ths of an inch, which is much more. And he was trying to account for the fact that the nozzle has more blow-by, almost 10 times, as the field joints, and I wondered what your opinion was, whether the actual fact that you have to move it further would weaken possibly the seal and account for the difference between the nozzle and the field joints?

MR. HARDY: The nozzle joint is different, as you know, than other joints. It's a great deal stiffer. But I do believe that the fact that the nozzle joint primary seal groove is wider by several thousandths and the fact that when you pressure actuate the seal and have to move it over a greater distance does account for the somewhat higher occasion of blow-by in the nozzle joint.

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But in any case, these are the steps. Now the design is such that if for some reason this fails to seal, then the secondary O-ring should take the pressure and essentially form the same type of O-ring or the same type of seal.

Now critical in this process is the rate of application of pressure, the squeeze on the O-ring and the change in the gap here. I would like to point out the fact that I believe the squeeze on the O-ring is important. To maintain pressure behind the O-ring while it is pressure-actuating, the gap dimension is important in the ability to form the extrusion seal and maintain that.

Now the dynamics of this seal is very important to us in the failure investigation, and we have a number of tests structured for this, and I will talk about them in just a few minutes. But a preliminary analysis as to what's happening in the squeeze on the O-ring and what is happening to the gap on 51-L dimensions, that is from the time we started at static zero pressure, static zero time, which is down here in this corner (indicating), until we build up in pressure with time, what is this gap doing? And also, what would be the effect of temperature on that sealing, little sealing dynamics?

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Well, the effect of temperature is still in analysis and will be subject to tests. We believe we have to actually run the dynamic tests. But just to look at some of the things that are not affected, as is for instance the O-ring by temperature, as the pressure goes up—and I've plotted here 10 milliseconds. In fact, the pressure is about 25 psi.

At 80 milliseconds, it's around 60 psi. And then at 150 milliseconds, it essentially hasn't gone up. What happens there is that the igniter in the head end of the motor provides the first initialization or pressure vise, and then as the crane ignites and the pressure starts going up, then the pressure starts going up very fast. But the deflection, if you will, and the gap change is represented by this curve.

So at 200 milliseconds, I'm at 250 psi and the gap is changing approximately 4/1000ths. Now what's important to understand is the seal performance under those dynamic conditions, and in the temperature area of interest.

So we have a number of tests designed to evaluate the performance of the O-ring under temperature-pressure dynamic conditions. These tests, some of them will be scale tests. Some of them will be portions or representative of full-scale tests. We will

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be taking the joint down to ensure that we have the O-rings at the temperatures predicted that 51-L would be. We will set up the initial conditions like we had on 51-L, and we will apply the pressure to the appropriate pressure profile.

Our objective is to see do we get blow by the primary O-ring, and if we do, will the secondary O-ring seal under those conditions?

MR. SUTTER: On that curve, I don't know whether you carried it out to, what, 800 seconds?

MR. HARDY: To 600 milliseconds.

MR. SUTTER: What is 600 milliseconds?

MR. HARDY: 6/10ths of a second.

MR. SUTTER: That says that the squeeze goes from .038 and drops down to .018? Am I reading the chart right?

MR. HARDY: That would say at 600 milliseconds the gap, the delta, the gap is 20/1000ths. And if I started with a 36/1000ths squeeze, and I had immediate response of the O-ring to fill the gap as it opens up, I still have a 16 mil squeeze.

But what is of interest to us is how will the O-ring respond at reduced temperatures.

MR. SUTTER: So the O-ring could be sitting there with very, very little squeeze on it and its

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performance has got to be a lot lower.

MR. HARDY: I would just change your words a little bit. The O-ring is going to be sitting there with the gap opened, and its response to the gap opening, its resiliency could be hampered by temperature.

GEN. KUTYNA: If you had zero resiliency, it wouldn't spring back at all. You could have a gap as much as 20/1000ths.

MR. HARDY: At this time, that is correct. Now unless something else has happened, unless something else has happened, I am long overdue for actuating the seal. Now this happens here at about 25 psi, 25 to 50 psi. This happens at 100 to 150 psi. And so at 600 milliseconds, I am at 700 psi plus. So the seal should actuate. Nominally the seal should actuate in this timeframe down here.

GEN. KUTYNA: Let me ask you, do you start time at the time that you light the solids, 7 seconds prior to that? Or is it 7 seconds when you light the liquids?

MR. HARDY: Right.

GEN. KUTYNA: And you go through an excursion of the bending moments on that chart, even though it is not very bad, what happens to that gap as a function of those bending moments in those 7 seconds?

MR. HARDY: I cannot quantify that. I can

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tell you that the dynamic model shows—the gap opening characteristics shows that the vast majority of the load that causes that gap to open is the pressure load. Now the bending load does have an effect on it.

GEN. KUTYNA: Are you going to run a test?

MR. HARDY: Yes. We're going to run a test, and we're also including in our model the effects of bending, and we will in fact incorporate that in the gap opening at these various times.

DR. FEYNMAN: So a preliminary estimate or kind of a guess when thinking about it, which way the gap goes at the initial bending, whether it's on the back there, does it close down first and then open on account of the pressure? Or does it open on account of the bending and then open still further?

MR. HARDY: It would tend to close, compress in this area, and open over here. Now again, our area of interest is maybe somewhere in between the compression and the tension. It is closer to the compressive load than it is the tensile load.

MR. ACHESON: What does the gap curve look like in other flights?

MR. HARDY: This would be—and I can be more specific with the data—but this would be near nominal, or I would say a little better than average.

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MR. ACHESON: Why does it drop back there at 80?

MR. HARDY: Well, of course the gap primary response is to the pressure. The highest pressure we see here is really the igniter. The igniter in this motor is about the size of the third stage of a Minuteman. So it is a big igniter. And that pressure causes the initial rise right here, and then the main grain ignites, and then it catches about here, and then it shoots it straight up.

VICE CHAIRMAN ARMSTRONG: Did I understand correctly the pressure measurements on that graph come from the transducer, and that the gap comes from calculations?

MR. HARDY: That is correct. The gap measurements come from actual measurements made in the pressure test that I referred to, and they also in that analysis we used strain measurements that we put on. In static firings, we put strain measurements around these various joints. So we know the strain that is going in just from the pressure around each one of these joints. But the gap is calculated based upon actual dimensions, and the model which has been validated by actual tests that we ran.

MR. RUMMEL: Did those calculations assume

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perfectly concentric mating of the components?

MR. HARDY: No. They assume the worst case that you could get, nonconcentric worst case that you could get. And let me explain that one more time.

That says that I've got two circles here, or I've got a circle nonconcentric, and then I've got another circle nonconcentric, and I put—at one point I put those just as close as I can possibly get them so that at another point I've got them as far away as I can have them, and that is what is referred to as the max gap.

MR. SUTTER: Could I ask one more quick question? A vendor can change these O-rings, or change materials? Has there been any change in the manufacturing process? Has anybody checked those records?

MR. HARDY: The primary vendor, as I mentioned, Parker-Hannafin, molds them and Hydropack that cuts them measures them, dimensionalizes them, and fuses them together, has not been changed to my knowledge. We have not changed the vendor for the rubber, although there are two sources, and we are in the process right now—and some way along on this—tracking back to the ore in the mine. We're tracking back to the rubber, the batch of rubber that was

1020

manufactured for these very O-rings that went into 51-L, what the chemical analysis and constituency of that rubber is. We are comparing. We're looking to see where else in the program it was used, if it has ever been used before.

We're doing the same thing by O-ring lots. So we are going back to the origin of the rubber to make that distinction.

MR. SUTTER: These O-rings that are checked out at Thiokol and put in bags, are they serialized so a given O-ring goes into a given segment? Or are they just in a store and you take out so many of them out of storage?

MR. HARDY: I'm not positive of that. Horace, do you know?

MR. LAMBERTH: I'm not positive of that, George. I will check.

CHAIRMAN ROGERS: George, just in consideration of our reporter, maybe you ought to pause a little bit. It's pretty rough for him. It's been a long session. Why don't we take just a few minutes break.

(Recess.)

MR. HARDY: Mr. Chairman, if I might proceed, I think I have maybe one more chart here and I will be

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finished my presentation. I would like to just mention one thing to make sure that I gave a correct answer, or you have a correct understanding regarding the inspection and certification of the O-rings that is done at Thiokol and delivered here certified.

It is, however, at Thiokol that government inspection is mandatory on the inspection of those O-rings, so it is done by Thiokol and government inspection. I just wanted to make sure. I thought I made that clear, but I wanted to make sure.

The only other thing I want to make clear before I go to the last chart, and then I will conclude, is with respect to the launch environment, cold temperatures. In addition to the effects of temperatures on the O-rings and the O-ring response and sealing performance capability, we are also running some tests and some analysis to see if it is possible that water in the joint creasing could in any way upset or effect this secondary sealant capability.

If you go around the joint here in the crevice, the water would be in this area here (indicating), and we are doing the analysis and tests to determine if there would be any effect of ice in the joint affecting the sealing capability. If I could go to the next chart, which I believe is my last one.

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(Viewgraph.) [Ref. 2/13-56]

This is proceeding on with the scenario that said that after primary O-ring blow-by, either the secondary seal leaked or because it was defective, or because it was affected by the temperature, or because it had ice in the joint, or whatever reason, or the leak checkport leaked, then the next event that occurs is at 58 seconds, approximately, where we know we have leakage out of the aft field joint.

These two events show up in every scenario because they in fact happened. So in every case we have to get back to this event. Now as mentioned before, and I won't belabor it anymore, we are experiencing analytically a great deal of difficulty sustaining a leak from at liftoff, or near liftoff, through the joint, the primary and secondary O-ring, and having it wait until 58 seconds before it shows itself as a blowing leak.

In fact, if we do the simplistic analysis and just simply assume that the entire joint by dimension has the complete ability to vent—that is, there is no choke points, no flow blockage or anything like that, it would indicate that we would be leaking profusely all the way around that joint in about 12 to 15 seconds.

Now we do know that there would be flow

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blockage in certain areas. It could be upstream, it could be downstream. So we are refining that analysis. But there is difficulty in maintaining the picture that we see and that is, let me say, a limited leak at best for 58 seconds, which grows into a bigger leak for that period of time, to the thermal flow analysis. But we are continuing to analyze that.

The next question would be the structural capability of that joint, and the structural capability of the joint assuming maximum flow conditions through there would also be degraded to the point that we would question the ability for the continuous leak. Emphasize again, those analyses have to be refined.

We have to run some tests to more properly characterize the flow conditions and complete those. That does not of course attack a scenario like this, because a small leak through this port could wait until about 58 seconds to manifest itself in the secondary O-ring, nor does it attack the scenario that says I've got limited leakage for some number of seconds, and I won't try to quantify that right now.

I did damage to the primary and secondary O-ring, but it sealed. And then when I came to max Q and got the maximum deflections on the joint, the degraded seal could not hold the seal. And so we are

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studying all of those scenarios.

I just wanted to tell you at this point at least, in a scenario that said that we started a leak through the primary and second O-ring at ignition, and that leak continued for 58 seconds is difficult to control it the way the photograph tells us it must have been controlled up to that time. But that doesn't rule out all of the possible scenarios for that event.

And I think that just about brings me to the end, except to say that in many of these scenarios that I've talked about here, particularly those that have to do with the joint dynamics, the sealing characteristics, the dynamic effect of transverse loads, et cetera, is a fairly comprehensive test program that we have got set up and in process.

Some of these tests will be conducted in Huntsville. Some will be conducted in Utah, and some will utilize test fixtures that have been in place. Some are requiring special test fixtures that are in fabrication now. We will be starting tests, and in fact are scheduled to start some of those tests at the end of this week, or just about in the now timeframe, and I expect they will be going on for the next two weeks, two or three weeks. And most of these tests are subscale tests, or component level tests. And as we learn from

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those tests, a need to get to a higher fidelity, and maybe even a full scale, we will define those tests as we proceed.

But the objective of those tests is to test these scenarios for any event they're in, and either credit it or discredit it, prove it or refute it. One other thing, as we continue and particularly in the area of dynamics and loads, we will develop any appropriate new scenarios. We will change the scenarios as they may have to be changed, and we will interconnect them, if that is indicated.

That concludes what I had prepared.

CHAIRMAN ROGERS: Thank you very much. May we go off the record for a moment?

(Discussion off the record.)

CHAIRMAN ROGERS: Let's go back on the record.

DR. WALKER: I had one question on the last presentation. Could someone say something more about the infrared picture evidence?

MR. HARDY: If I could, I would like to call on our thermal analyst who is with us from Marshall, and he has been working with the PSC people over the last several days to try to get some understanding or resolution of the difference between this and the metals at issue.

51-L FAILURE ANALYSIS

STATUS REPORT

O FAILURE SCENARIOS

O POTENTIAL FAILURE MECHANISMS/CAUSES

O WORK IN PROCESS

G. HARDY
MSFC
2-12-86

[Ref. 2/13-32]

51-L FAILURE ANALYSIS STATUS REPORT

- FAILURE SCENARIO ASSESSMENT MATRIX

EVENT 1 → EVENT 2 → EVENT N

- 51-L OBSERVATIONS/DATA
- ANALYSES/DATA
- TESTS/DATA
- EXPERIENCE BASE

SUPPORT/REFUTE

MOST PROBABLE FAILURE SCENARIO

- FAILURE MECHANISMS/CAUSES
  - 51-L HARDWARE PEDIGREE REVIEW
  - EXPERIENCE BASE
  - DESIGN
  - LAUNCH PROCESSING/LAUNCH/FLIGHT
  - ANALYSES
  - TESTS

- MOST PROBABLE FAILURE MECHANISM/CAUSE

[Ref. 2/13-33]

51-L FAILURE ANALYSIS

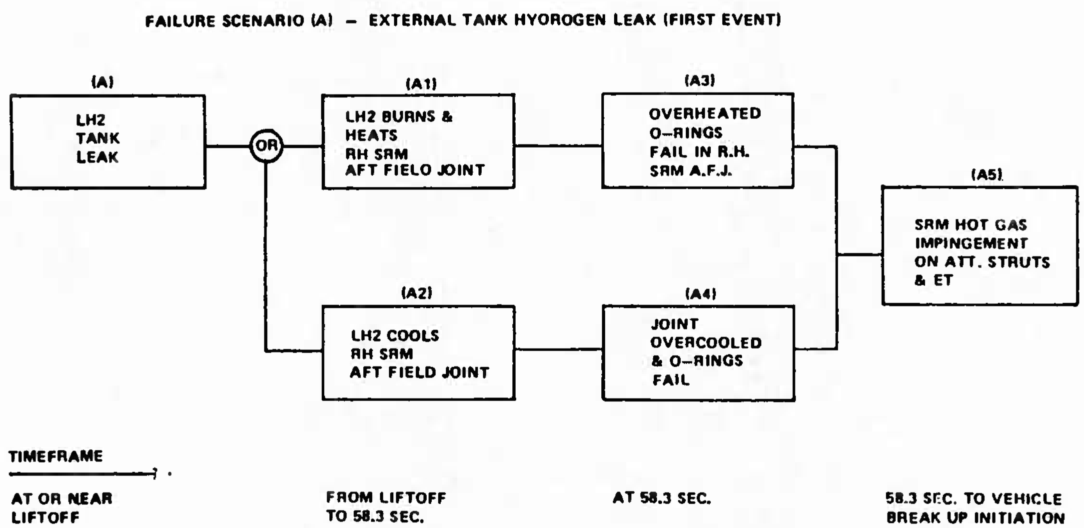
STATUS REPORT

FAILURE SCENARIOS
(FIRST EVENTS)

- O EXTERNAL TANK HYDROGEN LEAK
- O RIGHT HAND SOLID ROCKET MOTOR AFT FIELD JOINT LEAK
- O UNANTICIPATED VEHICLE LOADS/DYNAMICS

[Ref. 2/13-34]

51-L FAILURE ANALYSIS STATUS REPORT



[Ref. 2/13-35]

51-L FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO (A) - EXTERNAL TANK HYDROGEN LEAK (FIRST EVENT)
EVENT (A1) - LH_2 BURNS AND OVERHEATS RIGHT HAND SRM AFT FIELD JOINT

- o 51-L OBSERVATIONS/DATA

(A1.1) ANOMALOUS "SMOKE" STARTING AT (OR NEAR) LIFTOFF COULD BE ET TPS BURNING (STARTING AT OR NEAR LIFTOFF)

- o ANALYSES/DATA

(A1.2) 4 LB/SEC LEAK (0.81" DIAMETER HOLE) WOULD NOT BE DETECTABLE BY TANK ULLAGE PRESSURE MEASUREMENTS

(A1.3) STRUCTURAL INTEGRITY OF THE LH_2 TANK IS MAINTAINED WITH A HOLE GREATER THAN 0.81" DIAMETER

(A1.4) FILM ENHANCEMENT FOR H_2 BURNING AT LIFTOFF/EARLY IN FLIGHT

[Ref. 2/13-36]

51-L FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO (A) - EXTERNAL TANK HYDROGEN LEAK (FIRST EVENT
EVENT (A3) - OVERHEATED O-RINGS FAIL IN RIGHT HAND SRM AFT FIELD JOINT

- **51-L OBSERVATIONS/DATA**

  (A3.1) SRM HOT GAS BLOWING LEAK EVIDENT AT RIGHT HAND SRM AFT FIELD JOINT (58.315 SECONDS)
  (A3.2) RIGHT HAND SRM P_c STARTS DIVERGING (59.000 SECONDS)
  (A3.3) SRB AND SSME THRUST VECTOR CONTROL EXCURSIONS (61 - 63 SECONDS)

- **ANALYSES/DATA**

  (A3.4) HEATING RATE/MAX TEMPERATURE AT JOINT BY 58.315 SECONDS
  (A3.5) CORRELATION OF JOINT LEAK WITH TVC EXCURSIONS

- **TESTS**

  (A3.6) O-RING "FAILURE" AT HIGH TEMPERATURE

[Ref. 2/13-37]

51-L FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO (A) - EXTERNAL TANK HYDROGEN LEAK (FIRST EVENT)
EVENT (A2) - LH_2 COOLS RIGHT HAND SRM AFT FIELD JOINT

- 51-L OBSERVATIONS/DATA

  (A2.1) DOES NOT EXPLAIN ANOMALOUS "SMOKE" AT (OR NEAR LIFT OFF)

- ANALYSES/DATA

  (A2.2) 4 LB/SECOND LEAK (0.81" HOLE) WOULD NOT BE DETECTABLE BY TANK ULLAGE PRESSURE MEASUREMENTS
  (A1.3) STRUCTURAL INTEGRITY OF LH_2 TANK IS MAINTAINED WITH A HOLE >0.81" DIAMETER
  (A1.4) FILM ENHANCEMENT FOR H_2 LEAKING/FLOW

[Ref. 2/13-38]

51-L FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO (A) - EXTERNAL TANK HYDROGEN LEAK (FIRST EVENT)
EVENT (A4) - JOINT OVERCOOLED AND O-RINGS FAIL

- 51-L OBSERVATIONS/DATA

  (A4.1) SRM HOT GAS BLOWING LEAK EVIDENT AT RIGHT HAND AFT FIELD JOINTS (58.315 SECONDS)
  (A4.2) RIGHT HAND SRM P_c STARTS DIVERGING (59.000 SECONDS)
  (A4.3) SRB AND SSME THRUST VECTOR CONTROL EXCURSIONS (61 - 63 SECONDS)

- ANALYSES/DATA

  (A4.4) COOLING RATE/TEMPERATURE AT JOINT BY 58.315 SECONDS
  (A4.5) CORRELATION OF JOINT LEAK WITH TVC EXCURSIONS

- TESTS

  (A4.6) O-RING "FAILURE" AT LOW TEMPERATURE

[Ref. 2/13-39]

51-L FAILURE ANALYSIS
STATUS REPORT

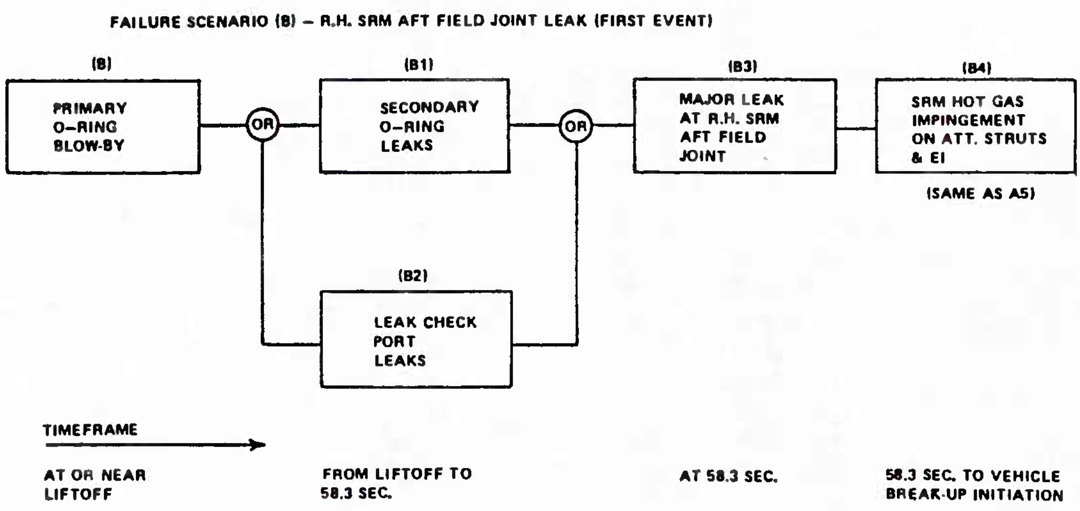
FAILURE SCENARO (A) - EXTERNAL TANK HYDROGEN LEAK (FIRST EVENT)
EVENT (A5) - SRM HOT GAS IMPINGEMENT ON ATT. STRUTS AND ET

- 51-L OBSERVATIONS/DATA

  (A5.1) DECREASED H_2 TANK PRESSURE RISE RATE (67 SECONDS)
  (A5.2) H_2 TANK PRESSURE DECREASES WITH 2 FCV'S OPEN (72 SECONDS)

[Ref. 2/13-40]

51-L FAILURE ANALYSIS STATUS REPORT



[Ref. 2/13-41]

51-L FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO (B) - RIGHT HAND SRM AFT FIELD JOINT LEAK (FIRST EVENT)
EVENT (B) - PRIMARY O-RING BLOW-BY

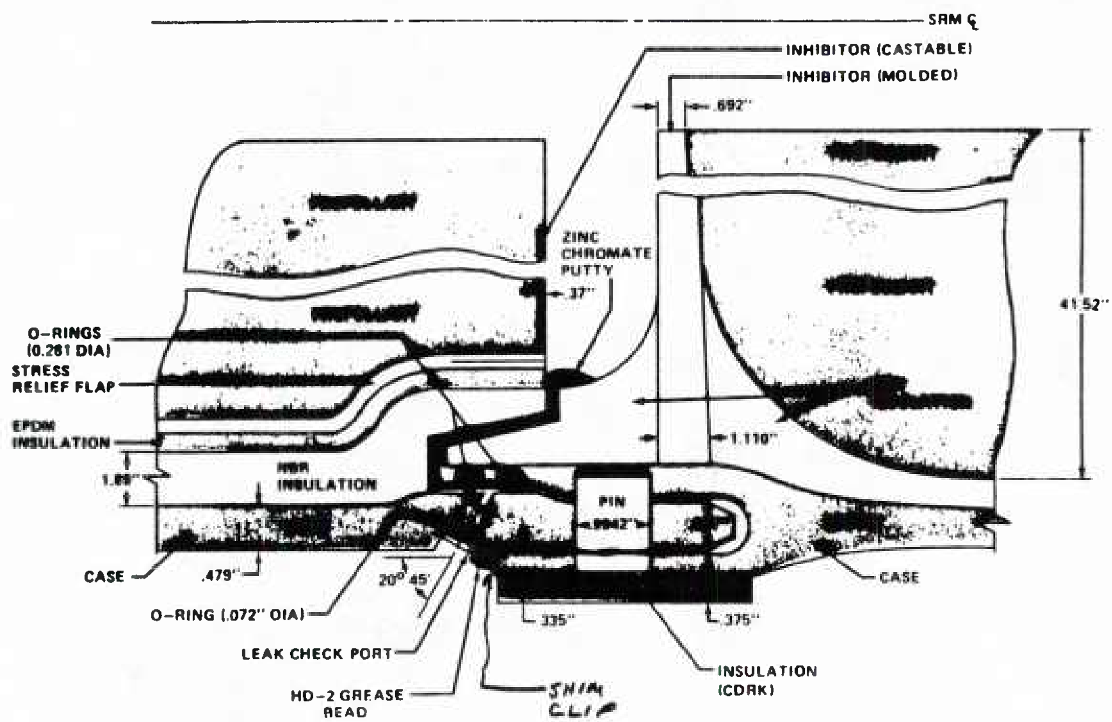
- ANALYSES/DATA

(B.1) EXPERIENCE SHOWS JOINT/SEAL DESIGN CAN RESULT IN BLOW-BY WHILE PRESSURE ACTUATING PRIMARY SEAL DURING EARLY PART OF IGNITION TRANSIENT

EFFECT OF BLOW BY LIMITED IN PREVIOUS EXPERIENCE BY SECONDARY SEAL

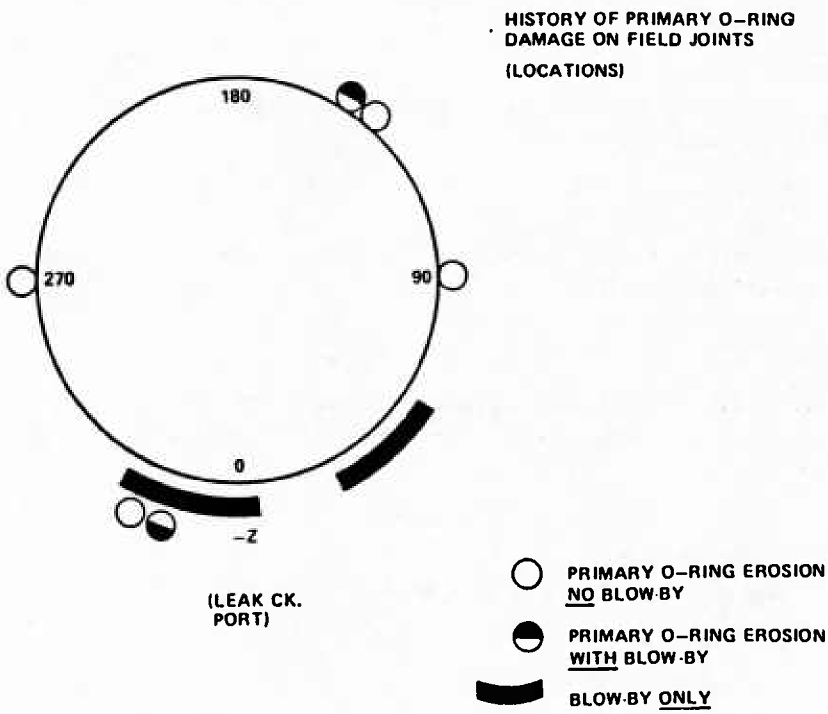
[Ref. 2/13-42]

AFT SEGMENT/AFT CENTER SEGMENT FIELD JOINT CONFIGURATION



[Ref. 2/13-43]

51-L FAILURE ANALYSIS STATUS REPORT



[Ref. 2/13-44]

51-L FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO (B) - RIGHT HAND SRM AFT FIELD JOINT LEAK (FIRST EVENT)
EVENT (B2) - LEAK CHECK PORT LEAKS

- **51-L OBSERVATIONS/DATA**

  (B2.1) ANOMALOUS "SMOKE" STARTING AT (OR NEAR) LIFTOFF COULD BE LEAK FROM LEAK CHECK PORT

- **ANALYSIS/DATA**

  (B2.2) LEAK CHECK PORT THERMAL/FLOW CHARACTERISTICS
  (B2.3) SECONDARY O-RING DEGRADATION DUE TO LEAK THROUGH CHECK LEAK CHECK PORT

- **TESTS**

  (B2.4 LEAK CHECK PORT FLOW CHARACTERIZATION TEST

[Ref. 2/13-45]

51-1 FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO (B) - RIGHT HAND SRM AFT FIELD JOINT LEAK (FIRST EVENT)
EVENT (B1) - SECONDARY O-RING LEAKS

- 51-L OBSERVATIONS/DATA

  (B1.1) ANOMALOUS "SMOKE" STARTING AT (OR NEAR) LIFTOFF COULD BE FROM RIGHT HAND SRM AFT FIELD JOINT
  (B1.2) SUSPECT SECONDARY O-RING INDICATED IN CLOSE-OUT PHOTOS
  (B1.3) LAUNCH ENVIRONMENT/COLD TEMPERATURES

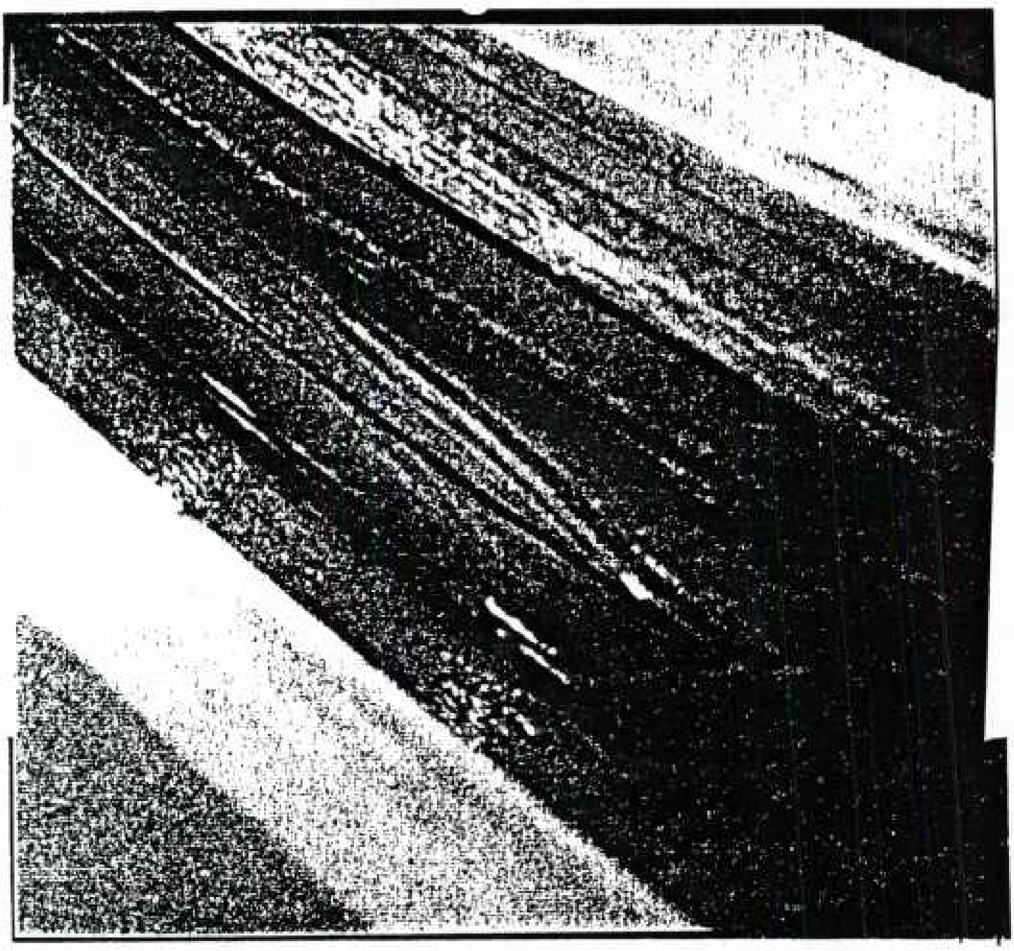
- ANALYSES/DATA

  (B1.4) SUSPECT O-RING PHOTO ENHANCEMENT
  (B1.5) JOINT THERMAL ANALYSIS
  (B1.6) JOINT/SEAL DYNAMICS

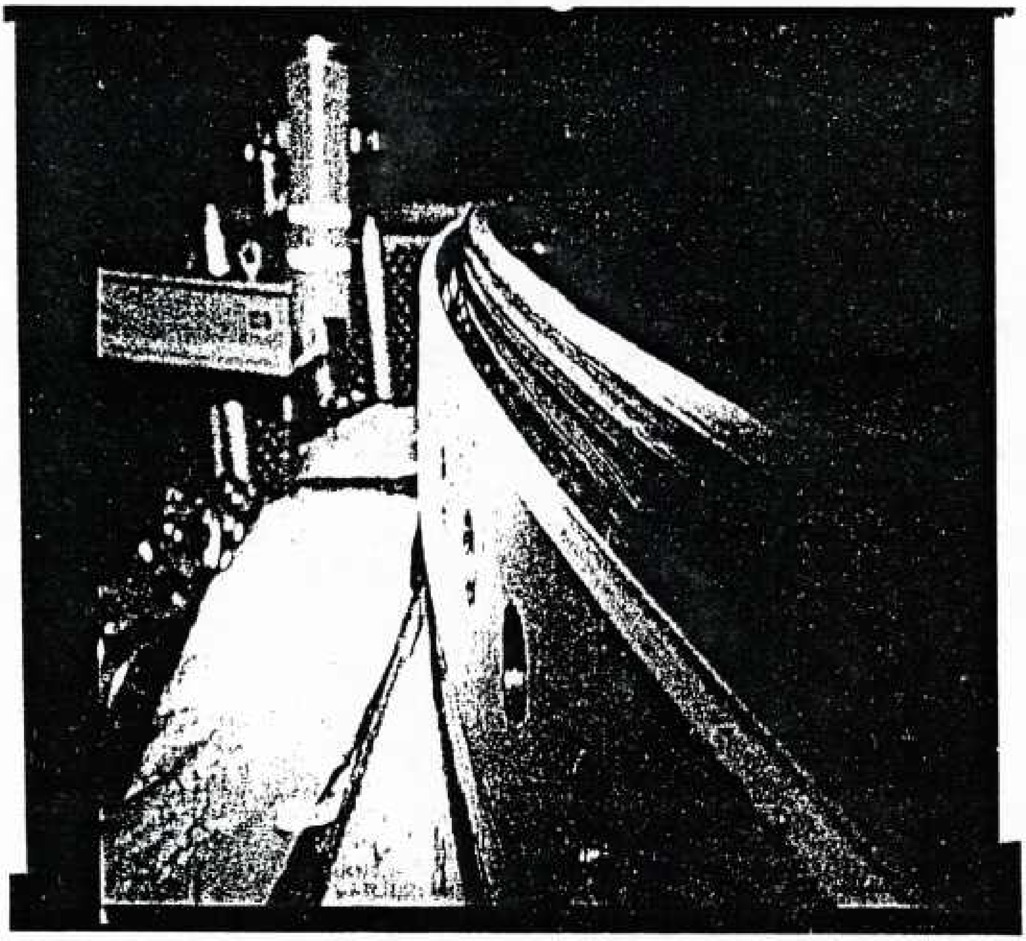
- TESTS

  (B1.7) SUSPECT O-RING SIMULATION/TESTS
  (B1.8) JOINT/O-RING THERMAL & DYNAMICS TEST

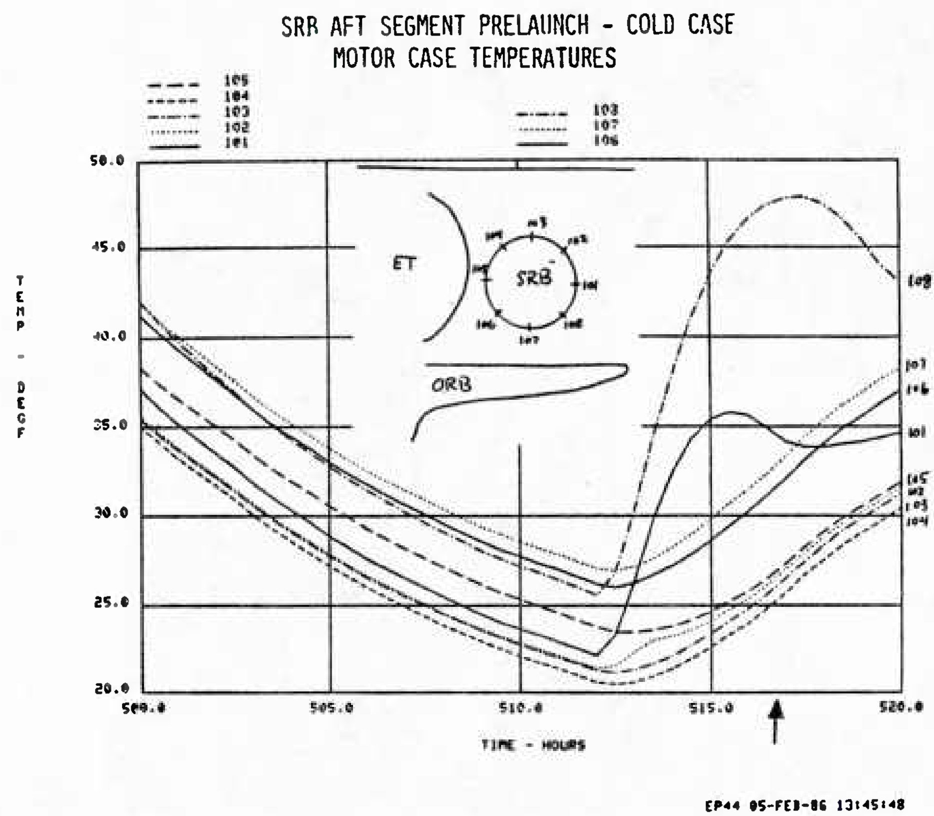
[Ref. 2/13-46]



[Ref. 2/13-47]



[Ref. 2/13-48]


SRB AFT SEGMENT PRELAUNCH - COLD CASE
MOTOR CASE TEMPERATURES
TEMP - DEGF
TIME - HOURS
ET
SRB
ORB
EP44 05-FEB-86 13:45:48

[Ref. 2/13-49]

PRELAUCH THERMAL PREDICITONS

PRELAUCH CONDITIONS

- COLDEST AMBIENT TEMP (0700 EST) = 23 to 24 °F
- AMBIENT TEMP AT LAUNCH (1130 EST) = 36 to 37 °F
- WIND 5 to 10 Knots from 250° at 0700 EST
- RELATIVE HUMIDITY 60% to 65% at 0700 EST

SRB TEMPERATURE PREDICTIONS

	AFT SEGMENT COLD CASE	AFT SEGMENT NOMINAL	FWD SEGMENTS NOMINAL
CASE TEMPS @ 0700 (coldest time)	21° to 27°	25° to 28°	25° to 27°
CASE TEMPS @ 1130 (launch)	25° to 47°	29° to 48°	29° to 47°
PROP MEAN BULK TEMP @ 1130	54°	56°	56°
AVG PROP TEMP AT WALL @ 1130	33°	37°	37°

COLD CASE: Min Sky Temp
Min Wind
ET Loaded 34 hr

NOMINAL: Nom Sky Temp
Nom Wind
ET Loaded 10 hr

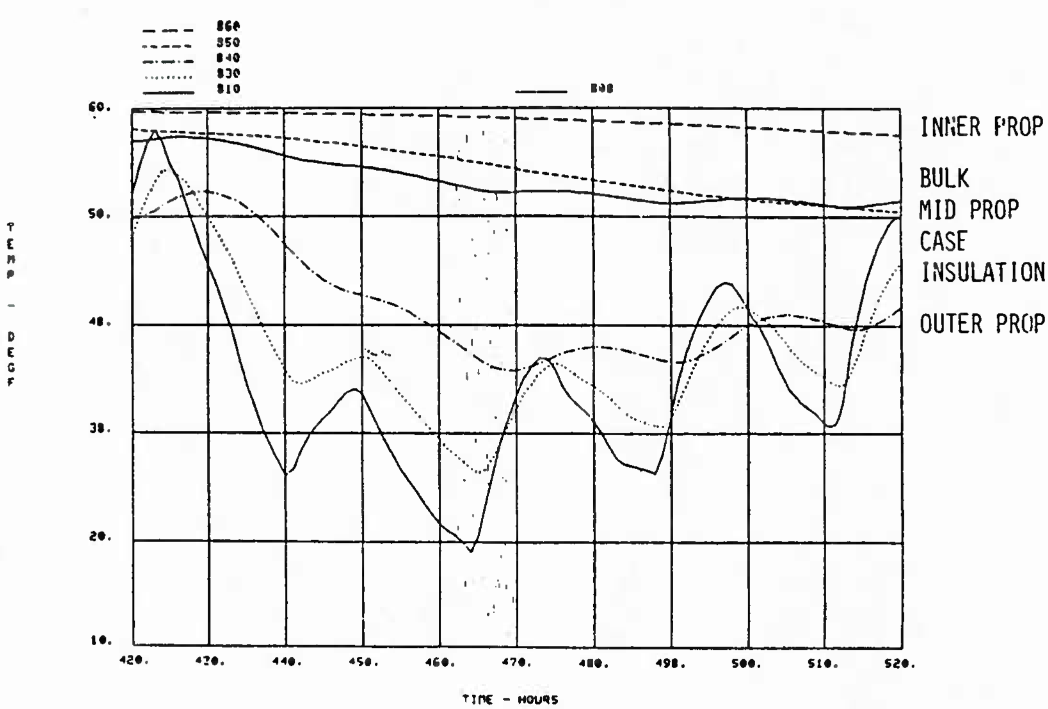
EP41 10-FEB-86 17:44

[Ref. 2/13-50]

STS 51-C

SRB AFT SEGMENT PRELAUNCH - COLD CASE

CASE AND PROPELLANT AVERAGES

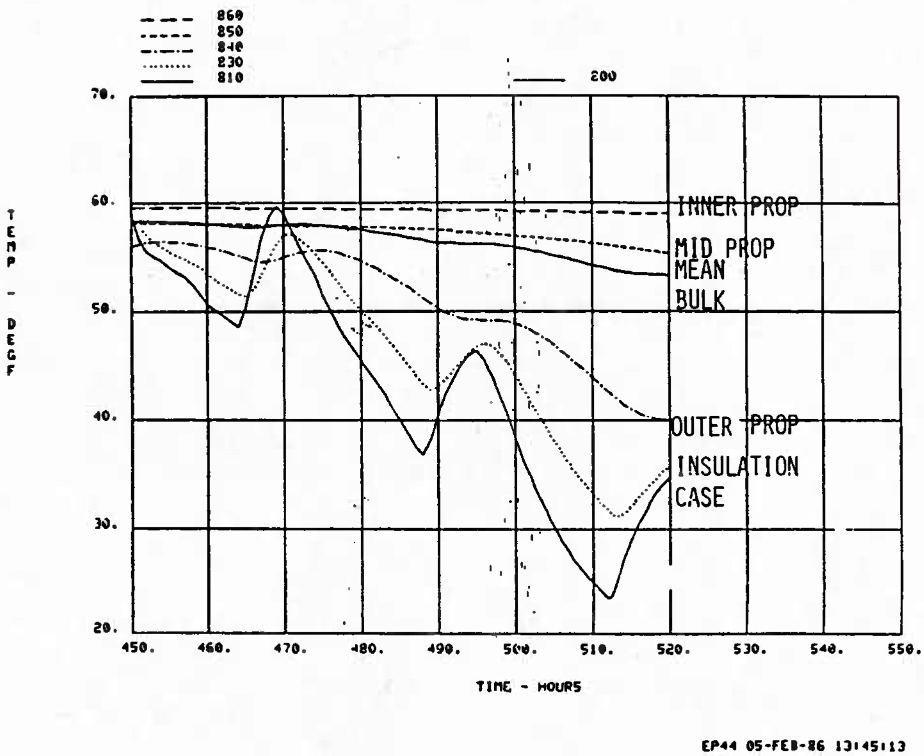


EP44 11-FEB-86 08:08:13

[Ref. 2/13-51]

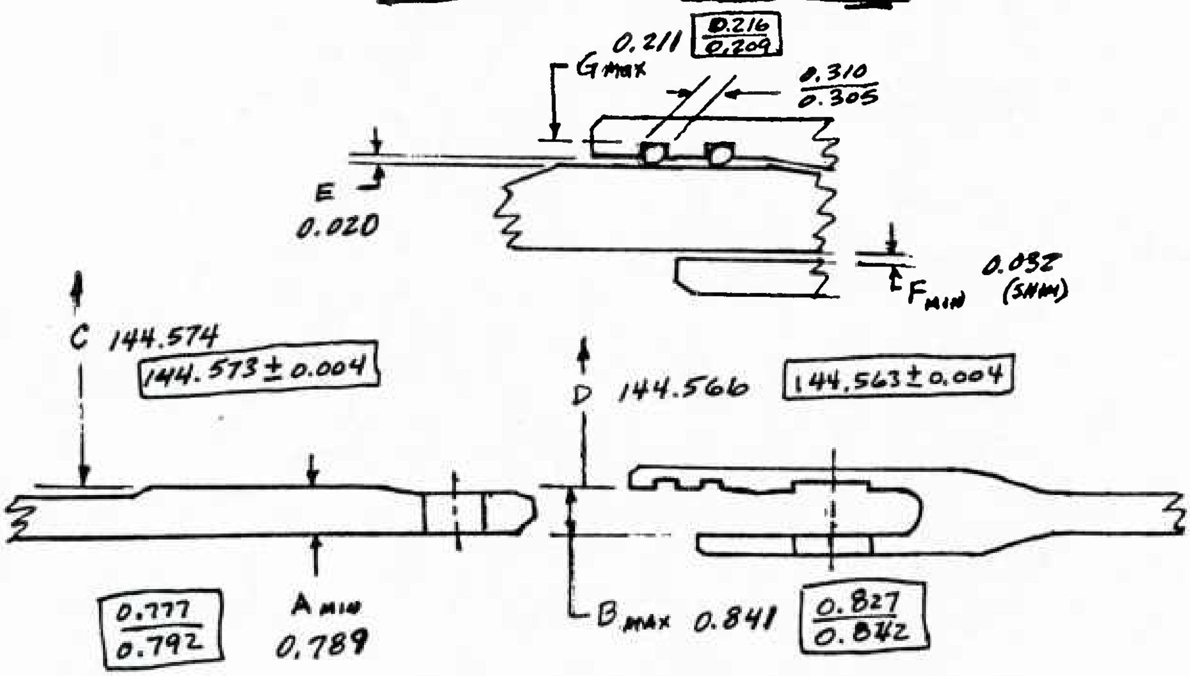
SRB AFT SEGMENT PRELAUNCH - COLD CASE

CASE AND PROPELLANT AVERAGES



[Ref. 2/13-52]

51-L R.H. SRM AFT FIELD JOINT (NTS)

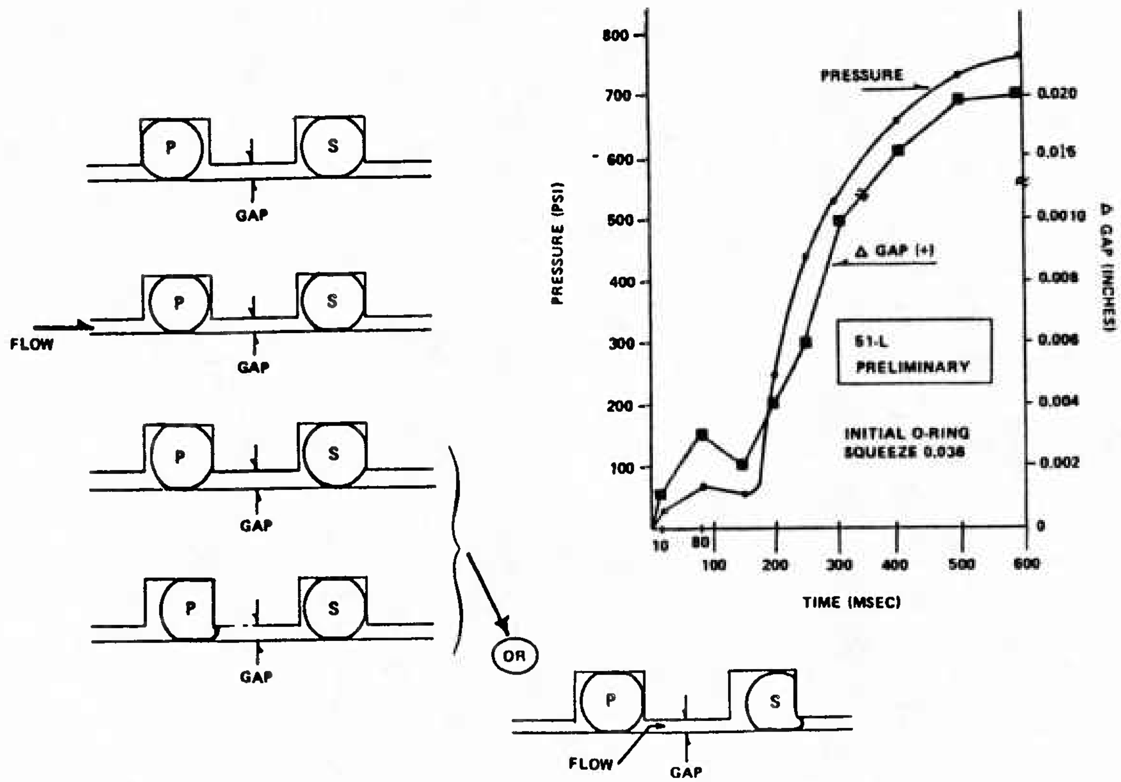


51-L O-RING SQUEEZE (ASSUME MIN. O-RING DIA.)
0.0395 (14.7%) - R/T
0.0356 (13.4%) - 26°F
MIN. O-RING SQUEEZE 7.54%

[Ref. 2/13-53]

51-L FAILURE ANALYSIS STATUS REPORT

JOINT O-RING SEAL PERFORMANCE CHARACTERISTICS



[Ref. 2/13-54]

[Ref. 2/13-55]

51-L FAILURE ANALYSIS
STATUS REPORT

FAILURE SCENARIO(B) - RIGHT HAND SRM APT FIELD JOINT LEAK (FIRST EVENT)
EVENT (B3) - MAJOR LEAK AT RIGHT HAND SRM AFT FIELD JOINT

- o 51L OBSERVATIONS/DATA

  (B3.1) SRM HOT GAS BLOWING LEAK EVIDENT AT AFT FIELD JOINT (58.315 SEC)
  (B3.2) RIGHT HAND SRM P_c STARTS DIVERGING (59 SECONDS)
  (B3.3) SRM AND SSME TVC CONTROL EXCURSIONS (61-63 SECONDS)

- o ANALYSES/DATA

  (B3.3) JOINT/O-RING THERMAL/FLOW VERSUS TIME
  (B3.4) CASE STRUCTURAL INTEGRITY VERSUS TIME
  (B3.5) CORRELATION OF JOINT LEAK WITH TVC EXCURSIONS

[Ref. 2/13-56]

MR. BACHTOL: Rick Bachtol from Marshall. As George presented the minimum temperature we predicted on the SRB was on the order of 21 degrees, and that was a cold case. That is where we assumed the coldest sky, a minimum wind, et cetera, to try to see if we could get down

to the measurements that were being made on the pad. Now what was made on the pad was 9 degrees on the righthand SRB.

We estimated there was a 4 or 5 degree error in that.

DR. FEYNMAN: Which way?

MR. BACHTOL: Well, I mean the measurement was too low by 4 or 5 degrees. And the problem there is, as the gentleman was shooting up at the SRB, the night sky was reflecting off of the SRB. The SRB is about .85 incidents, and we went out this morning and made some tests, and it looks like that error may be as much as 6 to 9 degrees, and the reason being it is a little more is because the instrument is only sensitive in the 8 to 12 micron region by design. That is also a window in the atmosphere as far as water vapor is concerned, so the night sky looks a lot colder to the instrument than it really is.

So we estimate there's about a 6 to 9 degree error on the low side, and a 9 degree heat measurement

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which gets it up to 15 to 18 degrees. And so we still have a small discrepancy between the 18 that he measured, and we put that 9 degree error on it in the 3 degrees that we calculated, and we're still looking at that to see if we can resolve that. But that is where we stand right now.

DR. COVERT: Why is there a difference between the right and the left side?

MR. BACHTOL: Well, the righthand side has a good view of the night sky, so we would predict it is colder. The lefthand side, when he is shooting up at it, he is reflecting the——

DR. COVERT: It is because of where he is standing?

MR. BACHTOL: Right. And he is reflecting the fixed surface structure, and we have proved that this morning when we were out there.

DR. COVERT: I am touched by your faith in your model, and I guess my experience with these things are such that—well, in what way have you calibrated this thing down at these kind of temperatures.

MR. BACHTOL: Let me say the model probably has, as far as surface temperature is concerned, plus or minus 4 or 5 degree error, but what I have done is biased down so that I feel like I've got pretty much

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minus zero. So I have oriented everything toward the low end to try to see how cold I can really get. For example, I assume the sky in my model was minus 30 degrees, which is a lot colder than a lot of people would think.

DR. COVERT: But still and all, have you ever really honest to God validated your model at temperatures at say below 30?

MR. BACHTOL: Well, for example, the day before we were within 2 degrees.

DR. COVERT: What was the temperature the day before?

MR. BACHTOL: The SRB temperatures were in the 30s.

DR. COVERT: So you're in a sense trying to validate the model and use it at the same time? Good luck.

(Laughter.)

MR. CRIPPIN: I guess I would like to make one point. Even though we're still trying to understand those measurements, this was the first time that we had ever seen that kind of a difference between the left and righthand SRB.

MR. BACHTOL: That's true. We've never seen that before.

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DR. COVERT: In the future, will you have him walk around and look at it?

DR. WALKER: So these measurements usually agree fairly well with the model?

MR. BACHTOL: They usually agree with the model in the right and lefthand SRB. They usually agree with each other unless one of them happens to be in the sun.

DR. FEYNMAN: Of course the primary question is not whether it agrees with the model or not. The primary question is whether what the temperature is that you measured, that is to say, what is wrong with the instrument, if I may put it that way, or how much specular reflection from the sky is going to produce, you mentioned 6 degrees. Is that figure also pushed for some reason, assuming the night sky is colder than you would have ordinarily thought?

MR. BACHTOL: No.

DR. FEYNMAN: To the normal night sky and normal reflection, not trying to push that?

MR. BACHTOL: This morning we demonstrated a 6 to 9 degree error, and you are right. We were interested, we are interested in the measurement. Because if I can only predict 21, and we actually had 9 degrees on the pad, then there is something else that is

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causing it that we have to look for.

DR. FEYNMAN: When you're pushing, you should push on your model, but you shouldn't push on the instrument.

MR. BACHTOL: That is why we went out and ran the test.

CHAIRMAN ROGERS: Okay, if we may now go to Mr. Moser.

MR. MOORE: He will talk about some work he has been doing on the failure scenario team.

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STATEMENT OF T. MOSER, HEAD, FAILURE SCENARIO TEAM, JOHNSON SPACE CENTER

MR. MOSER: I am going to talk to you this evening primarily about the failure scenario teamwork that we have had going on at Johnson Space Center.

(Viewgraph.) [Ref. 2/13-57]

There is a bit of other work that I want to review with you briefly. It is, and you can think of this entire report as a status report, and in light of what you just discussed about releasing information, I think it is true of everything I will show you here. We have tried to show you in a status everything that we know to date, and a lot of it is very preliminary, but it is our best shot today of the way we see things, and it can change.

So I will point that out now, and I will point that out as we go through some of the analyses that are being refined, and we will continue to do that. So I think the entire system needs to recognize that. If I could have the next charts, please.

(Viewgraphs.) [Ref. 2/13-58]

A little bit of background. What we did at the Johnson Space Center from the day, the morning of the incident, we formed a team that was consistent with

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our contingency plan at Johnson Space Center, signed by the Director also. It was comprised primarily of program and project office personnel in addition to discipline experts throughout the center, and in addition to a staff. It was formed as you see here.

The purpose of this organization was to, number one, ensure that we captured all the data. We are controlling that. We established a focus for this team, shown up here as a MER. That is our Mission Evaluation Room, which is an engineering focus for all of our activities in support of shuttle missions. All of our technical information flows through this in support of Mission Control Center. That was our focus for this activity.

We had developed an investigation plan for each one of the teams. We had established a time line, which you have seen, by Mr. Kohrs today. That is a dynamic time line that is changing as we interpret the data, I might add. And then we have daily tankups to exchange information between us.

The failure scenario team, which I am currently heading, was formed several days afterwards, once we began to get the data. To date, here is a one-page summary of what all of those other team efforts are, other than the failure scenario team. We have gone

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through all of the orbiter systems. We have gone through the main propulsion system—and by the main propulsion system, I mean the engines, the lines in the orbiter up to the interface between the orbiter and the external tank.

We have looked at the cargo, and the environments associated with the flights, both the natural and the self-induced environments; the dynamic loads; the thermal effects during ascent. We did not—we have let Marshall do primarily the work on the temperatures on the pad.

We do not see any indications at this time of any one of these systems contributing to this incident. We have not concluded our work there, however. We are spending most of our team efforts right now on an ascent reconstruction of the loads in those environments, and that is quite an extensive effort.

For that, the Johnson Space Center is responsible to the total system to define the loads, the vibration, the thermal effects on this entire stack configuration of all the elements. If I might take a moment, what we do for this particular configuration, we are going back and looking at the predicted thrust as measured from the solid rocket boosters, the mass properties of the system.

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We are doing a complete dynamic multiple response of the systems so we get the dynamic loads, we get the static loads, and we combine all of those so that we will provide to Marshall for their detailed structural analysis of this joint, or anything else that they want to analyze, an overall complete compatible set of dynamic loads. That is a significant amount of the work.

It will be using as best predicted winds at the time of the event. We have talked about max Q alpha, the dynamic pressure versus the angle of attack of the vehicle as measured as predicted. We will be filtering all of—or factoring all of that into these loads.

GEN. KUTYNA: Without extending the evening, much of the team has not seen the twang the Shuttle does on liftoff. Could we get a quick movie of that tomorrow? It takes about 15, 20 seconds. Have you got a good illustrative one?

MR. MOSER: I would say yes, but I can't speak for Kennedy. I know it is on every flight. All we've got to do is find the right camera. Looking primarily from this angle, you can really get the response. We are continuing with the major focus of our effort to the failure scenario team, and I'm going to spend most of

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this afternoon in my discussion with you on that.

The visual team has an effort and tried to enhance photos. We're doing a lot of work in that area. We're looking at the spectral analysis of the photographs, trying to understand what is causing different things that we see. We are looking at temperatures, trying to use calibrated pyrometers to look at photographs to see if we can get temperatures from that, in addition to the analysis that we're doing.

DR. FEYNMAN: How can you look at a pyrometer with a photograph?

MR. MOSER: We're going to try some schemes we have in our minds to see if we can calibrate.

DR. FEYNMAN: Do you have color photos? Do you have many different filters?

MR. MOSER: Yes. We have to look at it. I don't know if it's possible, but we're going to give it a shot. We're trying to do everything we can.

DR. FEYNMAN: I just wanted to make sure it wasn't utterly impossible.

MR. MOSER: Okay. The next charts, please.

(Viewgraphs.) [Ref. 2/13-59]

Now let me spend some time with you on this bullet here of the failure scenario team. The team is

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comprised of a group of senior, and I will underline "senior", experts in the areas that you see in that viewgraph. We use that team to focus and guide the activities for the failure scenario team, which is probably several hundred engineers that are doing some of the analyses that I will share with you today.

Our process through this organization is we have taken the output from the orbiter, the main propulsion system, and those things, looked at that, at evidence of what appears to be anomalous, or what conditions could exist which would contribute to a failure scenario much like George Hardy explained to you. And in addition to that data which is being documented at this time—and I might add that we do have—we have provided to Jess Moore and to Arnie Aldrich about a one-inch thick status documentation of the activities of the other teams.

We are using that as our findings. We are using visual data, much the type that you've seen today. In addition to that, we have spent a lot of time looking at other missions, corresponding times of what is occurring during the ascent phase of the mission, comparing apples and apples for those two to help give us clues.

The physical evidence we're just beginning to

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factor into our scenarios, and we are using the timeline bits. From that we have constructed failure scenarios. We have four primary failure scenarios that we are working now. I am going to touch in quite some depth on the one, and just tell you briefly about the other three.

The major part of our effort now is verifying each one of the steps in the failure scenarios, much as Marshall is doing.

DR. FEYNMAN: Is this an independent effort from what they're doing here?

MR. MOSER: Yes. I was just getting ready to say that. We have done this totally independent of Marshall. We met with them last week to begin to exchange information and ideas. We have been passing information back and forth about our failure scenarios, and those activities are independent. We are considering now joining efforts, however, to make sure that we get to the right depth.

I think once we establish some baselines, that would perhaps be prudent in our use of resources in order to try to get more depth, but we haven't done that yet. So everything to date has been independent. So then we close the loop. If we have something here we need back from that team, or more photographic data,

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then we close the loop through that process. And I don't want to belabor the process a whole lot.

I'm going to leave this chart up, because this is a failure scenario for an SRB leak, a solid rocket booster leak. We have other failure scenarios where the hydrogen leak has been the ignition source, and oxygen leak being the initiating force, but let me talk to this one to give you some feel for the status of our analysis.

This one, compatible with the timeline event Mr. Kohrs showed you this morning with an SRB leak being observed, had about a little bit less than half a second. The plume from the SRB expands in this theoretical model up to the time about 59 seconds. There is heating in the aft end of the solid rocket booster, and the external tank. There is a hydrogen leak detected, and I will substantiate all of this with more fact momentarily.

The righthand SRB moves with respect to the stack at about 72 seconds mission elapse time. We think then there is an overload at the forward end of the external tank spilling liquid hydro-

gen and liquid oxygen either by an overload here or some ruptures of the lines, and then we see the explosion.

I am going to walk you through this path and

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not spend a lot of time on these other things. We have closed, we think, some of these are closing out, like the large 17-inch disconnect for the hydrogen or oxygen line. We had that in our model, and we see data now which is not supporting that disconnecting.

The next charts, please.

(Viewgraphs.) [Ref. 2/13-60]

This is the way we have tracked through on our system. We establish a time, an observation, what we are seeing, what the data source is for which the observation is made, a premise which goes with that, and then a task which we identify and track, and then for each one of these tasks we have a detailed task description, a set of products, and a schedule which supports that.

I want to start here with the noticeable drop in the pressure of the righthand solid rocket booster on this screen, and I depicted that. You may not be able to see this because of the equipment here, but mission elapse time is just in excess of 60 seconds. We see the divergence. This is the righthand chamber pressure of the righthand SRB, and for the lefthand SRB up through that flight they had been tracking fairly closely.

DR. FEYNMAN: Just one moment, because you've got the same time of the divergence as they did. Is

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that done independently? Because it doesn't look like you could determine that time. How do you know you shouldn't go further on that?

MR. MOSER: Well, what we have done is we have compared our time lines, and we have tried to merge those. To the extent that——

DR. FEYNMAN: It's not the absolute time that I'm worried about. It is the fact that you picked that point to say those two curves are diverging. How do you know they don't diverge until later?

MR. MOSER: Pardon me.

DR. FEYNMAN: Let's say they do not diverge until later. You still have some time in which the things are the same distance apart as they were earlier.

MR. MOSER: If I'm interpreting you, there should be a tolerance on the start of the divergence, and that is correct.

DR. FEYNMAN: Well, it's remarkable that you come so close to each other.

MR. MOSER: No, I don't want to mislead you or give you any indication that we have not compared time lines. We did that probably about a week ago, trying to get a normalized and compatible set of time lines where we could reach agreement. And so for the purpose of exchanging data— —

DR. FEYNMAN: Carry on.

MR. MOSER obviously you can look at

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this. That could be interpreted to be several milliseconds, or hundreds of milliseconds probably on either side of that. And to further refine that, we are doing more analysis in this region to see if we can pin it down. On the other hand, I don't know that it is really that critical that we be that accurate for that particular one.

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We looked at that pressure drop. We have estimated the leak associated with that decaying pressure, assuming that the right hand should be tracking exactly with the left.

The next chart, please.

DR. COVERT: Tom, have you calculated what the thrust would be due to a leak of that size? Does that give you a torque that is compatible?

MR. MOSER: I'm going to touch on that in just a moment, but that is a good question, and the answer to your question is yes, okay.

DR. COVERT: Good.

(Viewgraph.) [Ref. 2/13-61]

As George Hardy indicated, it is difficult to expand a leak from where we think we see a leak at less than half a second after the SRB ignition up until the time of about 60 seconds, where we can see a visual indication of a leak in that solid rocket booster. We're predicting that that leakage in the solid rocket booster is growing at a very fast rate from 60 seconds and beyond, based upon our analysis and trying to extrapolate back down to the time of ignition.

And when we look at a one-dimensional growth like an O-ring moving circumferentially around a solid rocket booster—and let me call that a one-dimensional increase in area—you get an increasing area about

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like the lower sloped curve I show you up here. If it is an area increase from the square of the dimension, then it's a characteristic slope like you see here.

We're still not meeting that type of increase in area. To us that is telling us that we're not only getting—we're getting more than an ablation increase or melting of the steel, that we are literally losing part of the steel case to meet that type of model.

The next chart, please.

(Viewgraph.) [Ref. 2/13-62]

That is an effort we are spending an awful lot of time on trying to construct that failure to get some bound of what is happening in this joint, and this would be an upper bound. We have modeled thermally, two-dimensional thermal analysis of this joint, letting the 5700 degree gas go from inside the solid rocket, out the labyrinth path, through the clevis and the tang, to see what temperatures are doing out there with the O-rings removed and with the nominal caps.

DR. COVERT: Where do you get the material properties for this combustion? Is that from the JANAF tables?

MR. MOSER: I'm sorry? Repeat that, please?

DR. COVERT: The combustion products from the chamber that's going out.

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MR. MOSER: What we're doing here is just using the gas temperature. We are not using——

DR. COVERT: You have to know something like CP and CK and all kinds of grubby things like that.

MR. MOSER: I don't know specifically where our thermal analysts got that. I can find that out.

DR. COVERT: And you have to know what the constituents are.

MR. MOSER: Yes, yes, we have that, and I don't know what the source is, but I will find that out for you. I don't know maybe Jack Lee or George Hardy could answer that. I think we probably relied upon Marshall to provide that to us.

DR. COVERT: Fine.

MR. MOSER: We will find that out for you.

Anyway, after one second of flow-through a path like this, we're seeing isotherms as depicted here: 2,000 degrees, 1,000 degrees. As shown after two seconds of flow—and this is what Dr. Lucas was talking about—we're beginning to see melting, as shown in the cross-hatched regions.

The next chart, please.

(Viewgraph.) [Ref. 2/13-63]

So our flow area is increasing after 3 seconds, and 12 seconds. After 12 seconds, we would

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show a complete violation of that region, again in a two-dimensional thermal analysis. And this is with the O-rings completely gone.

MR. WALKER: And this is all around the circumference?

MR. MOSER: Well, this is just two dimensional, okay. So just taking that heat over that section, we are doing the three-dimensional analysis right now. So this is the type of thing that we are using to help build our model, how this leak rate is changing versus time.

The next chart, please.

(Viewgraph.) [Ref. 2/13-64]

Another event that we saw in the time line was an unusual pitch motion of the entire vehicle at about 64 seconds. We looked at several things. One was wind response and, just to get to the bottom line, that appears based upon the reconstruction of the trajectory to be the best explanation of that diameter response.

Everything appears to be normal. You will see some wind activity at that altitude, and the vehicle responded to it, and we think that is in fact what caused that.

We also looked at the plume effect from the SRB to see if that could be a contributor to this. It

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is not very significant in the overall aerodynamic forces on the vehicle. Nor do we see that it is changing the aerodynamic flow across the vehicle significantly.

We also looked at the possibility that one of the separation motors or all four of the separation motors on the aft end had fired, and if that would result in the motion of the vehicle. It does not, based upon our preliminary analysis to date.

The next chart, please.

VICE CHAIRMAN ARMSTRONG: Excuse me. On that previous one, you said something about the simulate with SSFS?

MR. MOSER: The space shuttle flight simulator.

VICE CHAIRMAN ARMSTRONG: It would seem the time to gather such things as wind shear, thrust degradation, and nozzle motions, and various dynamics, it would be appropriate.

MR. MOSER: That is all in that model, Neil. It is all included.

VICE CHAIRMAN ARMSTRONG: All of the elements you can reasonably include, with the mechanisms you have available?

MR. MOSER: That is correct, and I can't think

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of anything off the top of my head that it doesn't have in it. It is quite detailed.

VICE CHAIRMAN ARMSTRONG: And at what point would you expect to get preliminary results out of that?

MR. MOSER: Well, our preliminary results have been completed. It does not show the vehicle responding according to that.

I would say within the next couple of weeks we will have the final results from that. I can look at a detailed task and give you that specifically. Let's see. Could you put the last—okay, all right, go ahead.

(Viewgraph.) [Ref. 2/13-65]

The next thing in the ascent that we see is the change in the ullage pressure of the hydrogen tank from measurements. We then tried to determine what the cause of that was. We have looked at thermal analysis of the tank, but before I say that let me show you specifically what I'm talking about again. Again, interpretation of the exact time line, probably measured to the nearest thousandth of a second.

But we see what the characteristics and the repressurization is relative to what it had been up to that point. It is not normal. We're not keeping up with the pressurization of the tank.

GENERAL KUTYNA: But that is after the

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appearance of the flame?

MR. MOSER: That is correct, that is after the appearance of the flame.

GENERAL KUTYNA: Eight seconds or so?

MR. MOSER: About six seconds.

So we looked at that and said, what are the potential causes of that. We've got plume impingement on the external tank itself and on the repressurization lines.

The next chart.

(Viewgraph.) [Ref. 2/13-66]

DR. KEEL: Do we not have these charts?

MR. MOSER: You do have those charts. What I've done is, any ones you see back up there in the back, and I will mesh them together to let the information flow as easily as I could.

DR. KEEL: Would it be possible, for the benefit of the reporter and the record, to indicate what page number, so that we know what we're referring to in the record.

MR. MOSER: Yes, I believe that I can do that. Let me check my system here. It is marked down here. I am now looking at charts 14, BU-14, and on the right screen or C screen, chart 7.

Okay, I'll try and do that. On screen A we have taken the plume fron the SRB

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and impinged it directly on the external tank to see if that could have penetrated the wall of the external tank with different boiling heat transfer coefficients of the liquid hydrogen inside and adjacent to the aluminum wall. We have taken, theoretically taken the insulation, the spray-on foam insulation on the tank, off.

We're looking at a thickness of about four-tenths of an inch in thickness, and with a heat transfer coefficient very high. We are showing peak temperatures of about minus 100 degrees with that plume impinging directly on it.

At a lower heat transfer coefficient from the hydrogen to the skin of the tank, we are showing a peak of about 200 degrees, still well below——

DR. COVERT: So this raises liquid hydrogen to the inside skin temperature?

MR. MOSER: That is correct.

DR. COVERT: What temperature does nuclear boiling take place in hydrogen?

MR. BACHTOL: Well, we usually plot it versus heat plus.

DR. COVERT: I know that. I want to get to that.

MR. BACHTOL: Well, I can tell you the burnout heat flux in about 10 Btu's per foot squared second.

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DR. COVERT: But this age of 10,000 then corresponds to what?

MR. BACHTOL: I'm not sure. I suspect that is BTU's per hour. I'm not familiar with this analysis.

DR. COVERT: You say 10 BTU's per square foot per second?

MR. BACHTOL: That's right, burnout heat flux.

MR. MOSER: This assumes boiling. With that analysis—and again, we were trying to bound it. We did not show a penetration of the tank. But we also have analysis proceeding which not only takes the heat, but takes the energy from the aluminum particles which are in the solid propellant and impinges it, to see if we can erode the tank away. We do not have the results of that as of yet. The other thing that we are doing——

MR. WALKER: There's enough thermal mass in the hydrogen to keep the tank pretty cold?

MR. MOSER: That's right. We've even looked at what it would be doing to change the pressure of the gas which is dissolved back into the liquid, and we do not ever show a pressure increase of significant magnitude there.

Again, this is about one week's worth of analysis, which we began on this scenario. The other

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thing that we're going is trying to look at this decrease in pressure by a structural failure which would be manifested in the form of a buckle in the tank. Even though we are not melting the tank and penetrating that way, there are some fairly high thermal gradients set up and fairly high thermal stresses which could cause local buckling, with the local buckling and a crack forming in the tank and then letting that leak.

The thing that flies in the face of that, though, to see those types of pressure decreases we have to be leaking liquid at about 133 pounds per second, which is about equivalent to what is flowing through the engine. And so we don't think that that model is really going to hold up. Or in a gaseous sense, about a little bit less than one pound per second of gaseous hydrogen, which better fits.

The next chart, please.

(Viewgraph.) [Ref. 2/13-66]

We looked at the gaseous hydrogen repressurization line with a low heat flux, and we're seeing that wall temperature getting out to about less than 150 degrees. For a higher heat flux—the next chart, please.

(Viewgraph.) [Ref. 2/13-67]

We're getting high enough temperatures to fail.

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But again, we're trying now to model the plume coming out of the SRB and get a better characterization of what heat flux we're really seeing there. In addition to heat, we're also having to look at erosion of that line. That looks like a very high possibility of the thing that is causing the increase in the pressure of the tank.

The next chart, please.

DR. KEEL: Again, could I make a plea with you, please ask you to give some reference title, or figure number to your charts.

(Viewgraph.) [Ref. 2/13-68]

MR. MOSER: I'm sorry. I'm now looking at chart BU-16 on screen A, and on screen B BU-15, and chart 7 on screen C.

MR. SUTTER: Is all of this work to try to find out why it finally went, after the SRB leak started?

MR. MOSER: Yes, sir, seeing if we can fit all of the data to this scenario, that the SRB truly had started there. Some of this analysis is applicable. However, even if the ET started, if everything doesn't fit the physical evidence that we have, we don't think we have concluded or will have a legitimate understanding of what caused this failure.

MR. SUTTER: It would seem to me if that SRB

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ever leaked, like it could have with what we know, after that it's too late.

MR. MOSER: Yes, sir. I agree with you but today we can't say the SRB leaked for sure.

MR. SUTTER: But this is all a lot of work assuming the SRB leaked.

MR. MOSER: Yes, sir. I don't know any other way to build all of the facts into and to put the pieces of the puzzle together and have a clear picture. If I had a one-piece puzzle I wouldn't have a problem, but I don't.

MR. WAITE: Could you consider some of the structural loads at the bottom attachment, like loss——

MR. MOSER: I'm going to get to that in just a moment.

DR. FEYNMAN: There's no way to make aluminum hydride or something when it gets hot, chemical reactions between the hydrogen and the aluminum?

MR. MOSER: We haven't looked at that. That is on the list to do. We have not considered—gotten any depth in that.

On chart A—now, I'm coming up to your question now. Later into the program, into the flight, we see a divergence in the right-hand solid rocket booster

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compared to the rest of the stack. That is the orbiter on the left hand, and that is depicted on screen A. What's shown here is the pitch rate of the right and the left, and you can see slightly beyond 72 seconds, you see a deviation. The same way with yaw.

The potential explanations for that is the plume effects and also the heating on the aft structural attachments of the external tank to the solid rocket booster down here.

The next chart, please.

(Viewgraphs 17 and 18.) [Ref. 2/13-69]

This is a picture—on screen A I have chart 17, and B is 18, and chart 19 is on screen C.

On screen A, the lower attachment strut is shown in the upper part of this view. The center screen shows a cross-section of it. Pyrotechnic is included in that attachment. We looked at the possibility of that pyrotechnic getting to sufficient temperature to detonate it.

Based upon the thermal analysis that we have conducted so far, we show that pyrotechnic being slightly over 100 degrees. I think that that has been shown to be able to get up to about 420 degrees from 60 seconds before the pyrotechnic would be detonated. And so we went one step further.

1055

Next chart, please.

(Viewgraphs.) [Ref. 2/13-70 1 of 5] [Ref. 2/13-70 2 of 5]

And so we looked at the load on that link. And on screen C, I now have chart 20, and on screen B, I have chart BU-15.

The load in that tank is about 80,000 pounds at the time of the event. It is designed for compression of about 290,000 pounds and tension of about 390,000 pounds. So it is well within the capability.

Next chart, please.

(Viewgraphs 22 and 29.) [Ref. 2/13-70 3 of 5] [Ref. 2/13-70 4 of 5] [Ref. 2/13-70 5 of 5]

However, when we consider the temperature, and on screen A I have chart 23, on B I have chart 22, and C, I have chart 21, for the record. Looking at a thinner section of that same lower link, which has less thermal mass, predicting what the temperature is there and the surfaces are for an exposure of a plume like we are seeing at the solid rocket booster, you can see that we are easily getting up in excess of 2,000 degrees.

Looking at the material properties on screen A of a typical Inconel, somewhere around 1200 degrees the ultimate strength of that material just plummets. So that looks like a feasible failure scenario of what has happened.

Next chart, please.

1056

(Viewgraph B-15.) [Ref. 2/13-70 2 of 5]

To further substantiate that, we looked at if that link fails. So now the right-hand SRB is attached at a forward point and it is allowed to hinge about the aft point. It now has a new hinge line which is skewed to the axis of the solid rocket booster. If that rotates up toward the orbiter, then the ratio of the pitch rates and the yaw rates about an axis of that rotation correspond to exactly what we measured. So that fits the model pretty well.

Next chart, please.

(Viewgraphs 24, 25, 26.) [Ref. 2/13-71 1 of 3] [Ref. 2/13 2 of 3] [Ref. 2/13-71 3 of 3]

I have charts 24, 25, and 26 on A, B, and C. With that, with the loads that are on that, on the solid rocket booster at that time, at about 72-1/2 seconds we would be predicting a rotational rate about that hinge line of 40 degrees per second.

Screen B shows pictorially what that view would be, and screen C shows how the SRB moves over and begins to contact the inner tank region on the solid rocket.

DR. COVERT: What was the roll rate that would indicate that?

MR. MOSER: I would have to go back and look at that. Those are yaw rates, the pitch rates that are

1057

in the handout there, and I can't remember which of those—what that magnitude was. Shall I go back there?

DR. COVERT: Well, this says relative roll rate.

MR. MOSER: That is the roll rate about the new hinge.

DR. COVERT: I understand that. I wondered what the data indicate. I can't put this back into a single thing in my head.

Lets go on, Tom.

MR. MOSER: It is shown on chart BU-16, that shows the right hand has a pitch rate at that time of about minus point—I mean, about .5 degrees positive. And it has a corresponding yaw rate—this is about the body axis now for the right-hand SRB—which changes drastically and goes up, goes from minus about .15 or 1-1/2 up to about 3-1/2 degrees per second. [Ref. 2/13-68]

Next chart, please.

(Viewgraph 28.) [Ref. 2/13-72]

I want to show you what I think is happening up in the forward end, and again just to put the other pieces of the puzzle together.

(Viewgraph 27.) [Ref. 2/13-73]

Now, on screen B I have 28 and on screen C I have 27.

1058

With a 12 degree rotation about that new hinge line, we bottom out the forward SRB attachment to that on the external tank. So now we're beginning to induce a bending moment in that carry-through structure between the SRB's, which is a load direction for which it was not designed for a very high magnitude, and I don't know that number right off the top of my head.

Next chart, please.

(Viewgraphs 29 and 30.) [Ref. 2/13-74] [Ref. 2/13-75]

What that is doing, it is inducing loads into the inner tank region, which is dumping them into the blocks, wall, and the bulkhead of that, and the same way with the hydrogen tank.

As the SRB continues to rotate—and on screen B, I have 29 and on screen C chart 30—the aft attachment does not encounter an interference until it has rotated through about 52 degrees. However, by the time it rotates through that angle, the forward end of the solid rocket booster has impinged into the inner tank region a significant amount, as seen in the crosshatch on chart C. And we are completing the analysis now to show what that would do as far as loading up the external tank, both LOX and hydrogen.

Next chart, please.

(Viewgraph.) [Ref. 2/13-76 1 of 3] [Ref. 2/13-76 2 of 3] [Ref. 2/13-76 3 of 3]

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From a CAD, the way it would appear to be, in screens A, B, and C, charts 31, 32, and 33. The back end of the solid rocket, right hand solid rocket, has moved up toward the elevons, and from the physical evidence that meets the model also, looking at the damage, in talking to the National Transportation Safety Board members today and looking at it myself.

The head-on view of that, you can see with that type of rotation how that right-hand solid rocket booster has gone up and probably contacted the wing for sure plus failed the tank.

CHAIRMAN ROGERS: Is this compatible essentially with what the Aviation Week story said?

MR. MOSER: I did not read that all the way through.

MR. HOTZ: It's pretty accurate, sure.

MR. RUMMEL: Isn't the impact point on the tank between——

MR. HOTZ: I don't think they had any contact with the wing in their story, but they had the rotation.

MR. MOSER: I saw it is USA Today and they referenced the Aviation Week article, and I believe that they did have it—show it going into the inner tank. It is probably the same thing.

1060

MR. RUMMEL: Could I ask, the impact point of the SRB with the tank is between the oxygen tank and the other tank?

MR. MOSER: Correct.

MR. RUMMEL: Is there any evidence of those tanks in fact failing?

MR. MOSER: Just the visual evidence, yes, sir.

MR. RUMMEL: There is?

MR. MOSER: In the visual evidence, there is a cloud of vapor at this corresponding time coming out on the tank.

CHAIRMAN ROGERS: If we recover the right booster, will the damage to it likely confirm this theory?

MR. MOSER: Yes, sir, I would think so. It should show some damage.

MR. WAITE: How long has this been available to you since the 8th of February, is that right?

MR. MOSER: Yes, sir, this has.

DR. RIDE: Do the photos show the SRB contacting the tank?

MR. MOSER: Pardon me?

DR. RIDE: Do the photos show the SRB contacting the tank?

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MR. MOSER: The photos, no, not so far. We can't tell that, Sally. We are trying to enhance it to see if we can, but to date you cannot see that it does, at least not in the observations that I have made.

MR. SMITH: But I guess, even though you can't see that, that would agree with the leakage that you're seeing in the topping off of the forward oxygen tank. I think that fits the story.

MR. MOSER: It fits a lot of the physical evidence, the visual evidence.

MR. WILLIAMS: Plus you've got a lot of burning in that area, too.

MR. MOSER: The next charts, please.

(Viewgraph.) [Ref. 2/13-77]

MR. MOSER: I'm not going to walk you through the same detail on this, but it is a scenario not too unlike George Hardy showed you. Assuming a hydrogen leak initially, on screen A, which is chart BU-5, 6, and 7, respectively, A, B, and C; which on screen B is an oxygen leak, following through that failure scenario tree; and then a structural failure on BU-7.

And I do not think that I have sufficient information to really show you the status of that today, but we are working that and we will continue to work it until we have closed out what we think is the most

1062

probable.

And that's it, Mr. Chairman.

CHAIRMAN ROGERS: Thank you very much.

Are there any questions?

(No response.)

Well, I guess we should adjourn for the day if there are no further questions.

MR. WALKER: Mr. Chairman, in the morning are we going to pick up with these other two presentations?

MR. KEEL: The intent, Mr. Chairman, is to start with the Thiokol temperature discussion, but then try to constrain that to an hour and 30 minutes, and then go directly from there to the assembly demonstration prior to the tour. And then we will couple the wreckage reconstruction into the visit to the logistics facility, where we actually will look at the wreckage.

MR. SMITH: Excuse me. Are we going to have the discussion on the field joint assembly prior to the tour? It probably would be beneficial.

MR. KEEL: Yes, I just said that. That will be at 9:30. We are redoing the agenda right now.

(Whereupon, at 7:35 p.m., the meeting was adjourned.)

NASA
Johnson Space Center

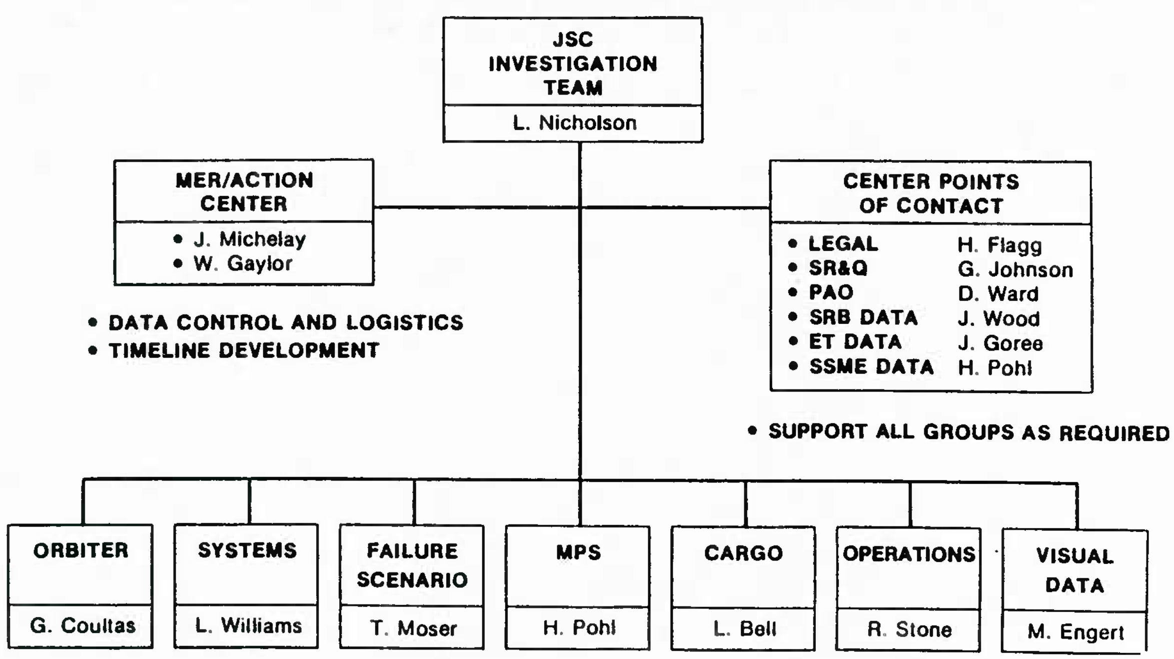
Failure Scenario Team

Thomas L. Moser
February 13, 1986

[Ref. 2/13-57]

NASA
Johnson Space Center

Organization



[Ref. 2/13-58 1 of 2]

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Initial Actions and Operations

- ENSURED THAT ALL ELEMENTS HAD TAKEN STEPS TO IMPOUND DATA
- ESTABLISHED MER AS FOCAL POINT FOR
  - DATA CONTROL
    - MAINTAIN LOG OF ALL RELATED DATA AND LOCATION
    - CONTROL FLOW OF DATA WITHIN TEAM AND TO EXTERNAL LOCATIONS
- DEVELOPED TEAM INVESTIGATION PLAN AND ROSTER
- COMPILED A SINGLE INCIDENT TIMELINE FOR ALL TEAMS AS REFERENCE
- ESTABLISHED DAILY TEAM TAG-UPS FOR COORDINATION AND TO PROVIDE INPUTS TO THE INTERIM BOARD

[Ref. 2/13-58 2 of 2]

NASA
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Failure Scenario

- **CHAIRMAN**
  - **THOMAS L. MOSER - EA - X6511**
- **MEMBERS**
  - **Phil Glynn, SCENARIO LEADER X2876**
  - **Chester Vaughn, SCENARIO LEADER**
  - **Gene McSwain, GUIDANCE & CONTROL**
  - **C. Tom Modlin, LOADS & DYNAMICS**
  - **Bob Ried, AERO & GAS DYNAMICS**
  - **Cecil Gibson, PROPULSION**
  - **B.J. McCarty, SAFETY & RELIABILITY**
  - **Bill Schneider, STRENGTH INTEGRITY**
- **TEAM FUNCTION**
  - **BASED ON THE EVIDENCE (DATA, VISUAL, PHYSICAL, RECORDS) CONSTRUCT FAILURE SCENARIOS WHICH COULD HAVE CAUSED THE FAILURE ON STS 51-L. EVERY ELEMENT OF THE SCENARIOS WILL BE VERIFIED OR PROVEN INCORRECT BASED ON EVIDENCE (DATA, VISUAL, PHYSICAL, RECORDS) AND SUPPORTING ANALYSES AND TESTS**
- **TASKS**
  - **TASKS IDENTIFIED BY FAILURE SCENARIO ATTACHED**

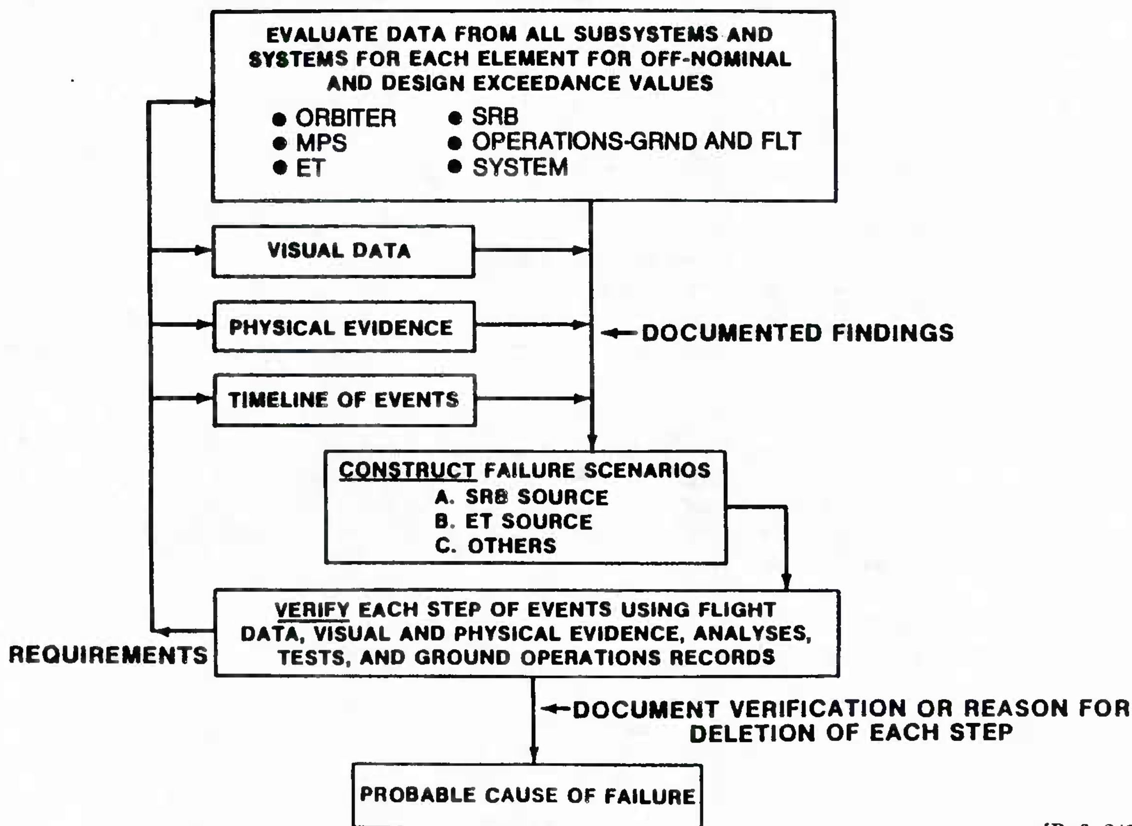
[Ref. 2/13-59]

NASA

Johnson Space Center

Failure Scenario

Failure Investigation of STS 51-L



[Ref. 2/13-60]

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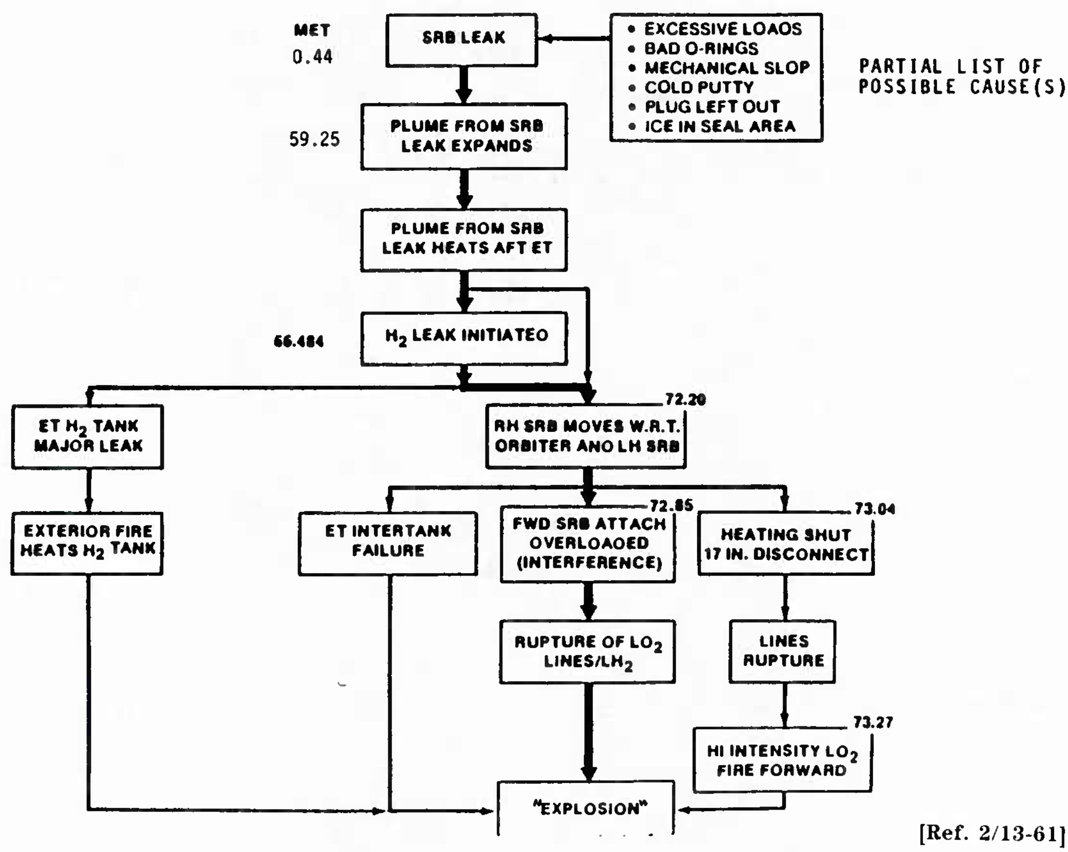
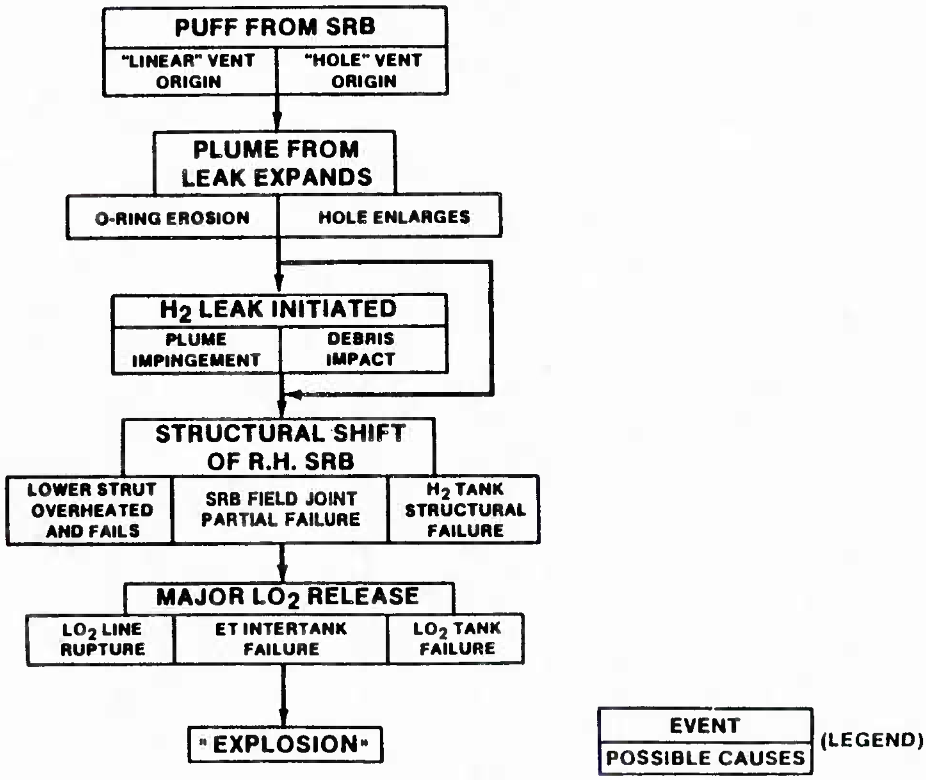
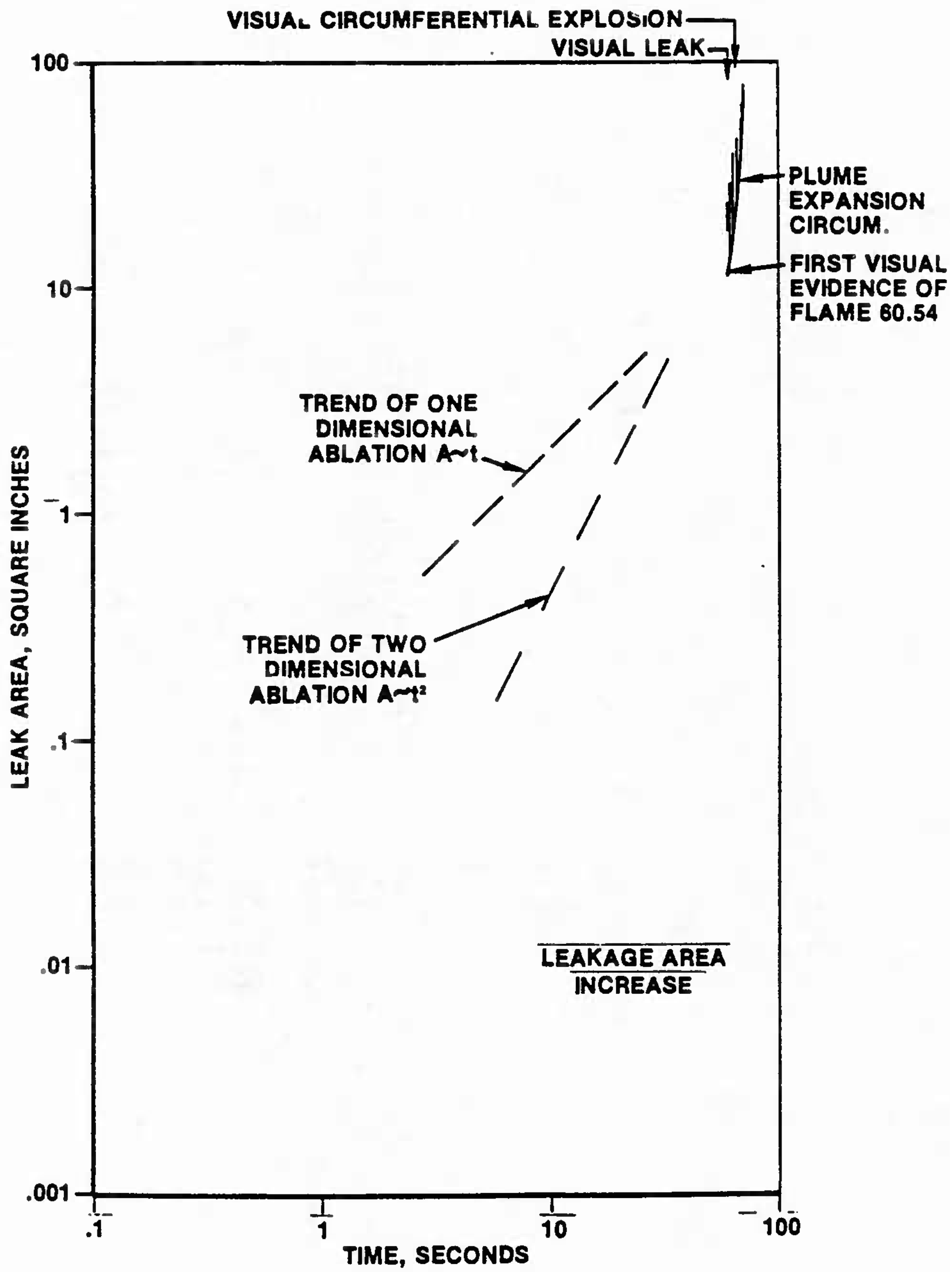
Failure Scenario
SRB Leak



[Ref. 2/13-61]


PUFF FROM SRB
"LINEAR" VENT ORIGIN
"HOLE" VENT ORIGIN
PLUME FROM LEAK EXPANDS
O-RING EROSION
HOLE ENLARGES
H_2 LEAK INITIATED
PLUME IMPINGEMENT
DEBRIS IMPACT
STRUCTURAL SHIFT OF R.H. SRB
LOWER STRUT OVERHEATED AND FAILS
SRB FIELD JOINT PARTIAL FAILURE
H_2 TANK STRUCTURAL FAILURE
MAJOR LO_2 RELEASE
LO_2 LINE RUPTURE
ET INTERTANK FAILURE
LO_2 TANK FAILURE
"EXPLOSION"
EVENT
POSSIBLE CAUSES
(LEGEND)

[Ref. 2/13-62]


VISUAL CIRCUMFERENTIAL EXPLOSION
VISUAL LEAK
100
10
1
.1
.01
.001
PLUME EXPANSION CIRCUM.
FIRST VISUAL EVIDENCE OF FLAME 60.54
TREND OF ONE DIMENSIONAL ABLATION A~t
TREND OF TWO DIMENSIONAL ABLATION A~t²
LEAKAGE AREA INCREASE
LEAK AREA, SQUARE INCHES
.1
1
10
100
TIME, SECONDS

[Ref. 2/13-63]

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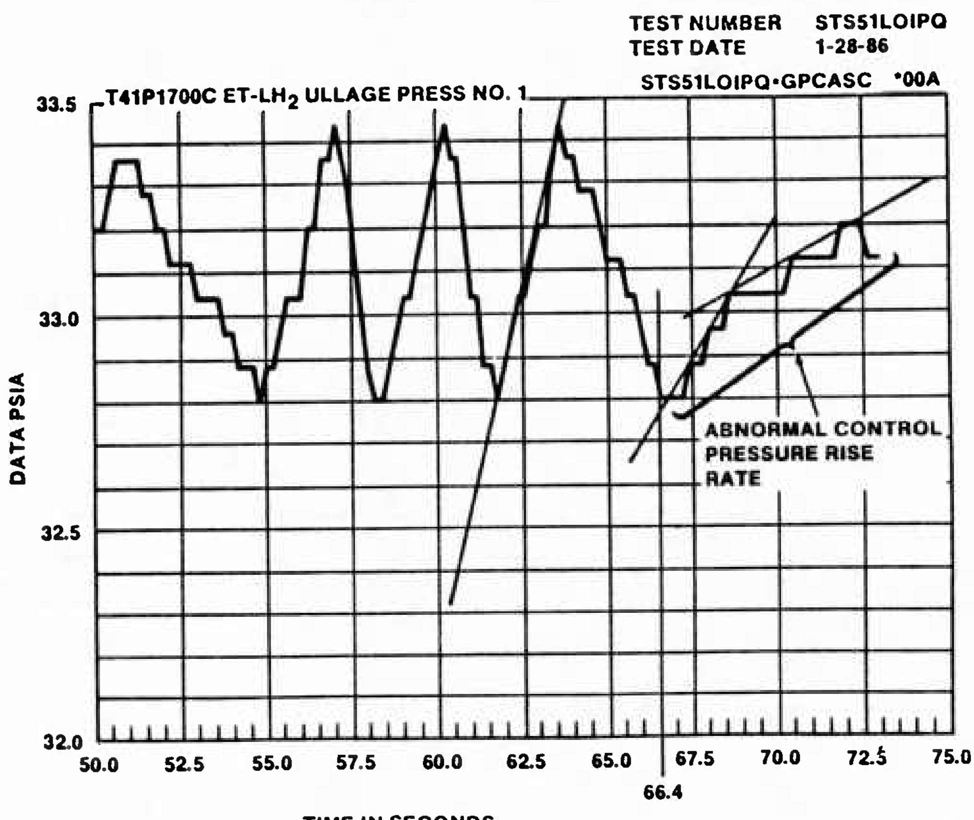
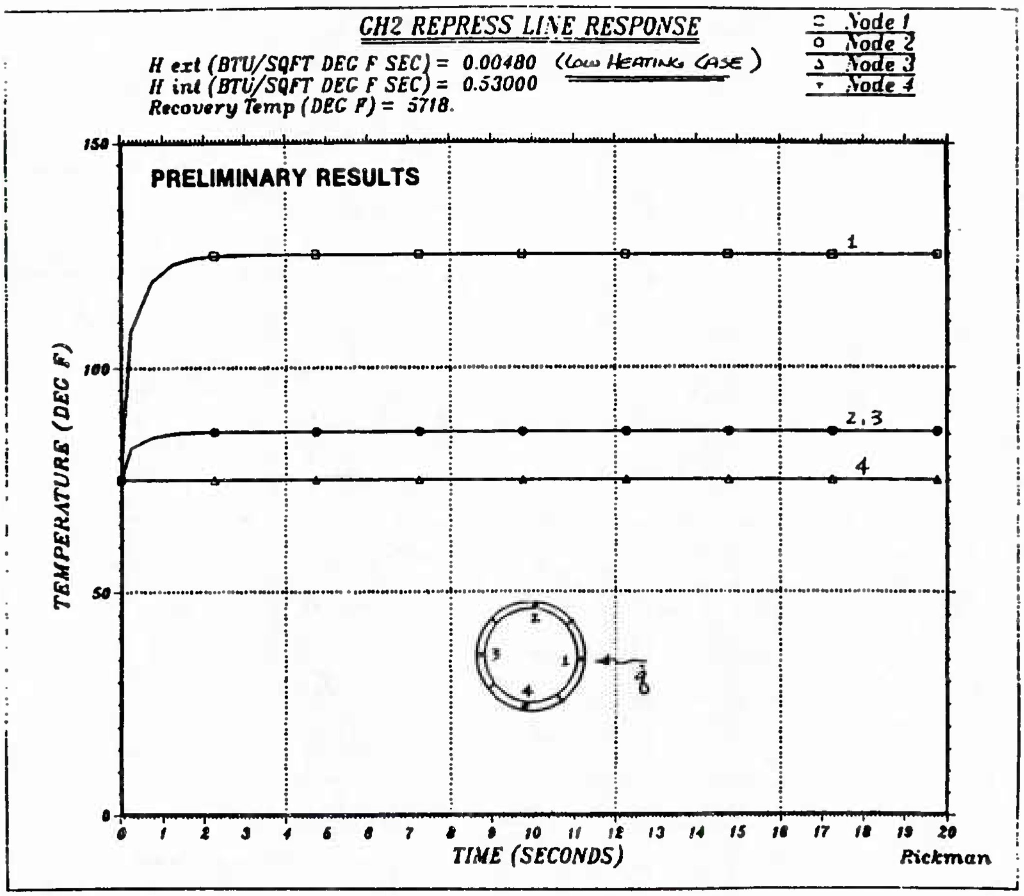
Failure Scenario
Summary Worksheet (Cont)

TIME (SEC)	OBSERVATION	DATA SOURCE	PREMISE	TASK	RESULT
T-64.6	UNUSUAL PITCH MOTION	NUMEROUS FLIGHT CONTROL MEAS#: V90R252CL	NORMAL WIND RESPONSE	TASK #: EH2-1 DUE DATE: 2/14/86 RESP: G. McSWAIN • ANALYZE RESULTS OF TRAJECTORY RECON-STRUCTION WITH BEST ESTIMATED WINDS	• RESULTS SUGGEST WIND RESPONSE IS PROMINENT "AS DESIGNED"
			CHANGE IN RH SRB PLUME INDUCES ANOMALY	TASK #: ED-3 DUE DATE: 2/21/86 RESP: R. WALLACE • ESTIMATE CHANGE IN AERO FORCES	• PRELIMINARY ANALYSIS SHOWS INADEQUATE FORCE WHEN CONSIDERED ALONE
			REAR RH SRB BSR MOTOR FIRED	TASK #: EH2-2 DUE DATE: 2/7/86 RESP: G. McSWAIN • SIMULATE WITH SSFS TO VALIDATE OR DISPUTE PREMISE	
T-66.30	SRB LEAK HAS EXPANDED IN PLUME SIZE AND EXTENT	CAMERA#: E206 PHOTO#: FR361	SRB HEATING IS CLEARLY SIGNIFICANT	TASK #: VT-2 DUE DATE: TBD RESP: VISUAL TEAM • LOCATE EXTENT OF SRB LEAK AND IMPINGEMENT AREAS - CAD/CAE FILM	

[Ref. 2/13-64]

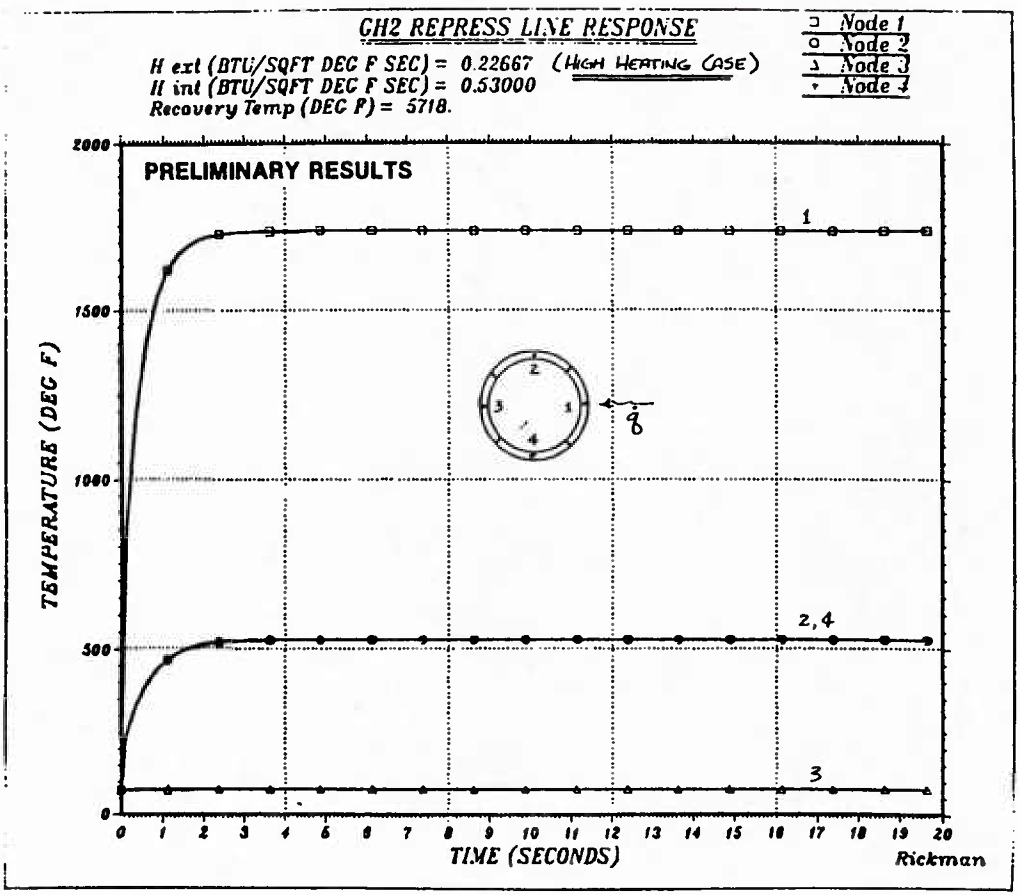

TEST NUMBER STS51LOIPQ
TEST DATE 1-28-86
STS51LOIPQ·GPCASC *00A
T41P1700C ET-LH2 ULLAGE PRESS NO. 1
33.5
33.0
32.5
32.0
DATA PSIA
50.0
52.5
55.0
57.5
60.0
62.5
65.0
67.5
70.0
72.5
75.0
66.4
TIME IN SECONDS
ABNORMAL CONTROL PRESSURE RISE RATE

[Ref. 2/13-65]

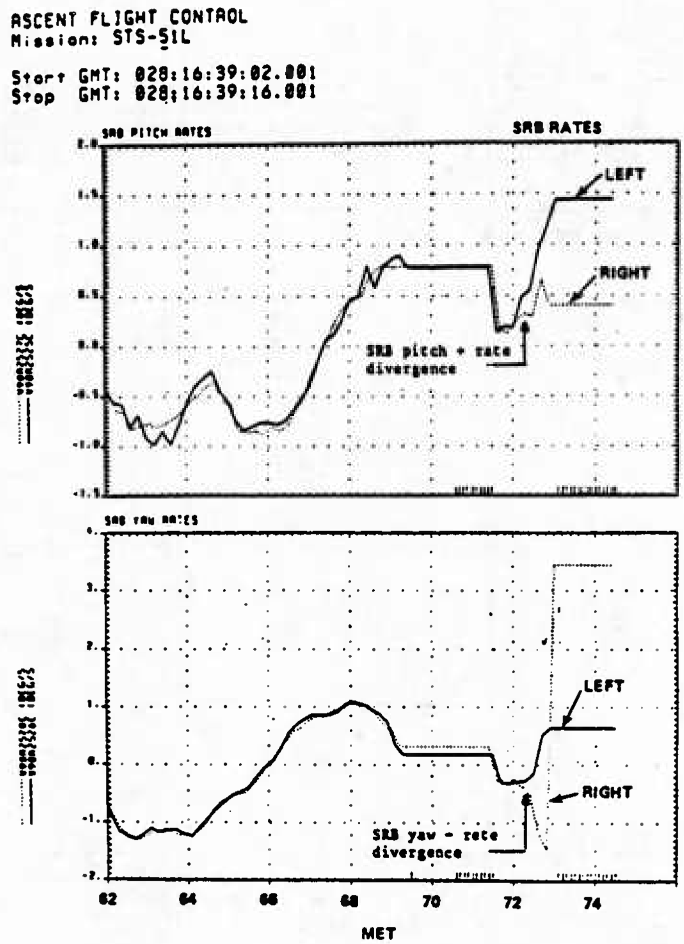


GH2 REPRESS LINE RESPONSE
H ext (BTU/SQFT DEC F SEC) = 0.00480 (Low Heating Case)
H int (BTU/SQFT DEC F SEC) = 0.53000
Recovery Temp (DEC F) = 5718.
Node 1
Node 2
Node 3
Node 4
PRELIMINARY RESULTS
TEMPERATURE (DEC F)
TIME (SECONDS)
1
2,3
4
Rickman

[Ref. 2/13-66]

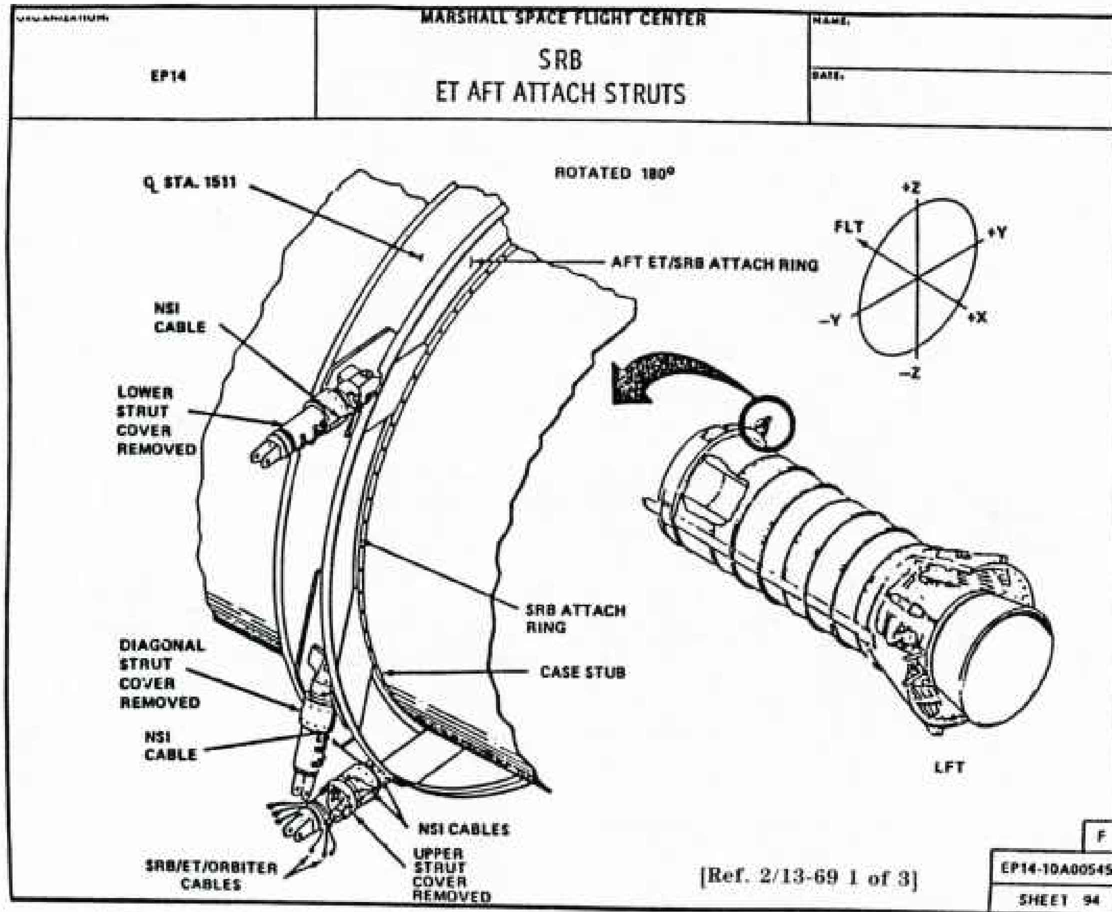

GH2 REPRESS LINE RESPONSE
H ext (BTU/SQFT DEG F SEC) = 0.22667 (HIGH HEATING CASE)
H int (BTU/SQFT DEG F SEC) = 0.53000
Recovery Temp (DEG F) = 5718.
Node 1
Node 2
Node 3
Node 4
PRELIMINARY RESULTS
TEMPERATURE (DEG F)
2000
1500
1000
500
0
1
2,4
3
q
TIME (SECONDS)
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
Rickman

[Ref. 2/13-67]

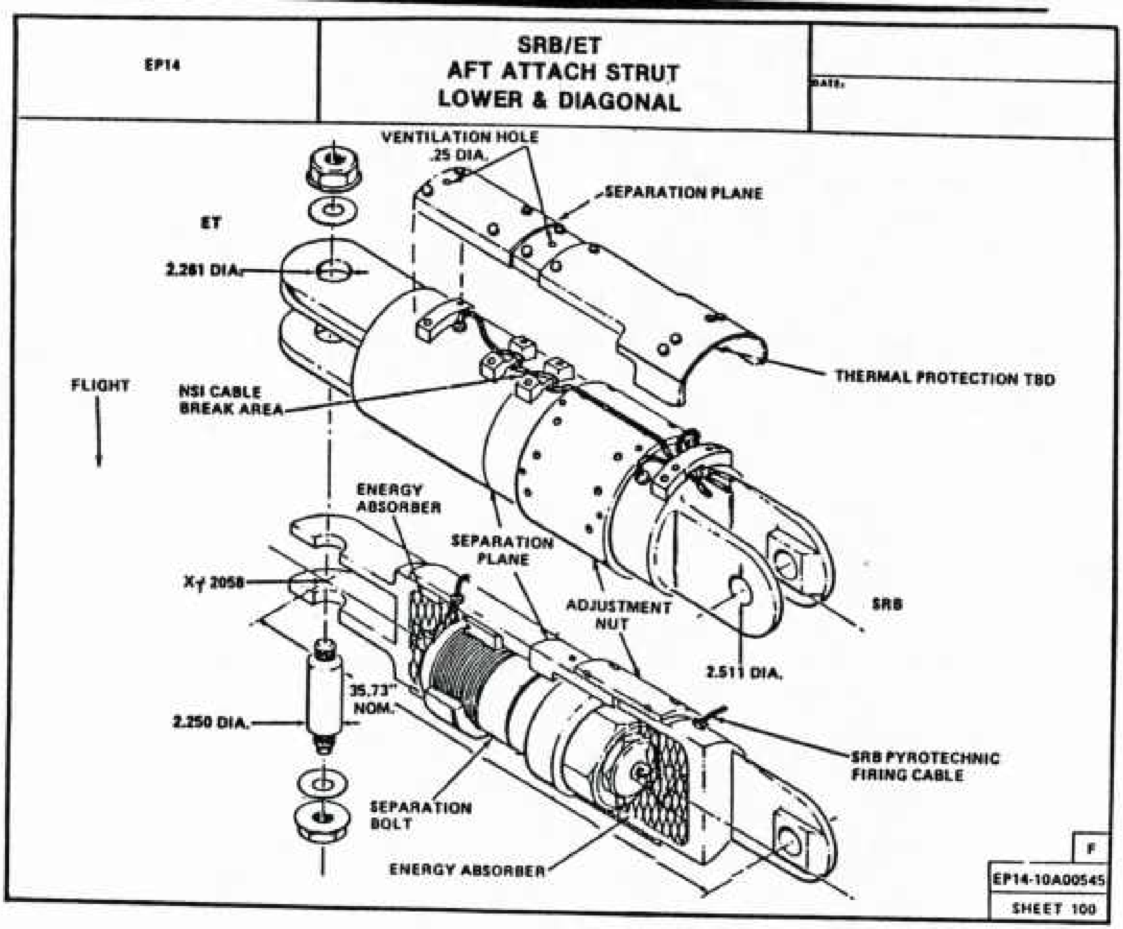


-SRB yaw and pitch rate divergence.

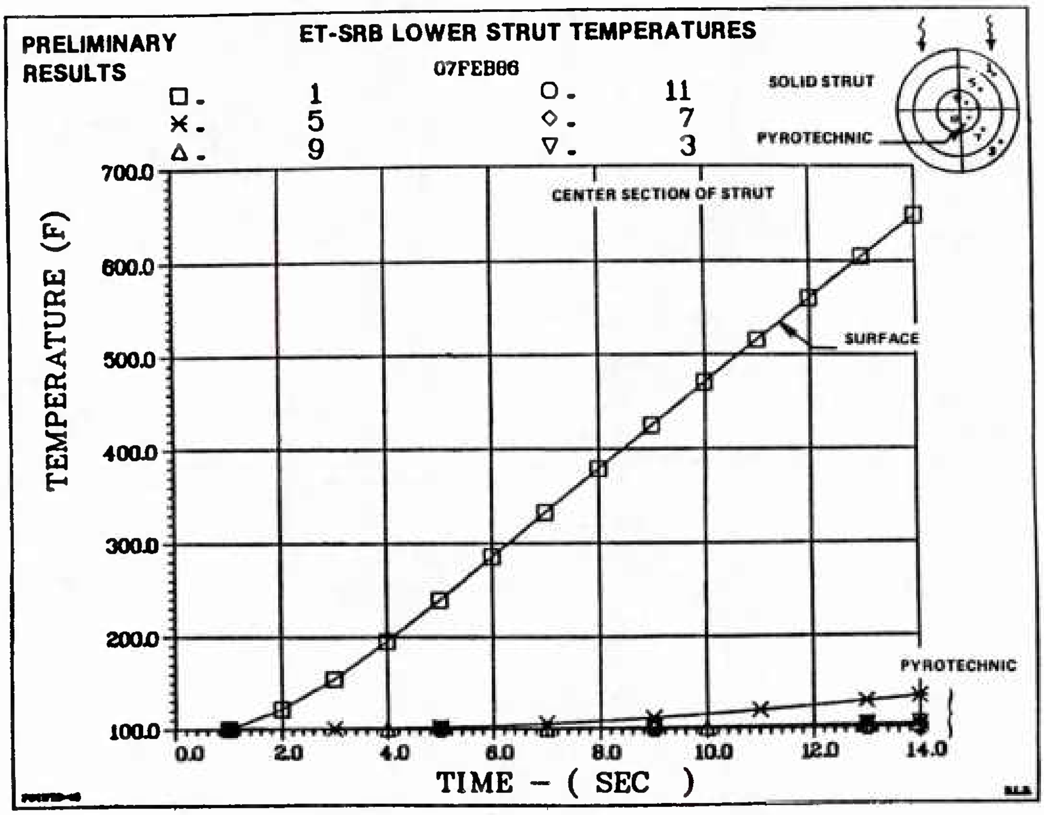
[Ref. 2/13-68]



[Ref. 2/13-69 1 of 3]



[Ref. 2/13-69 2 of 3]



[Ref. 2/13-69 3 of 3]

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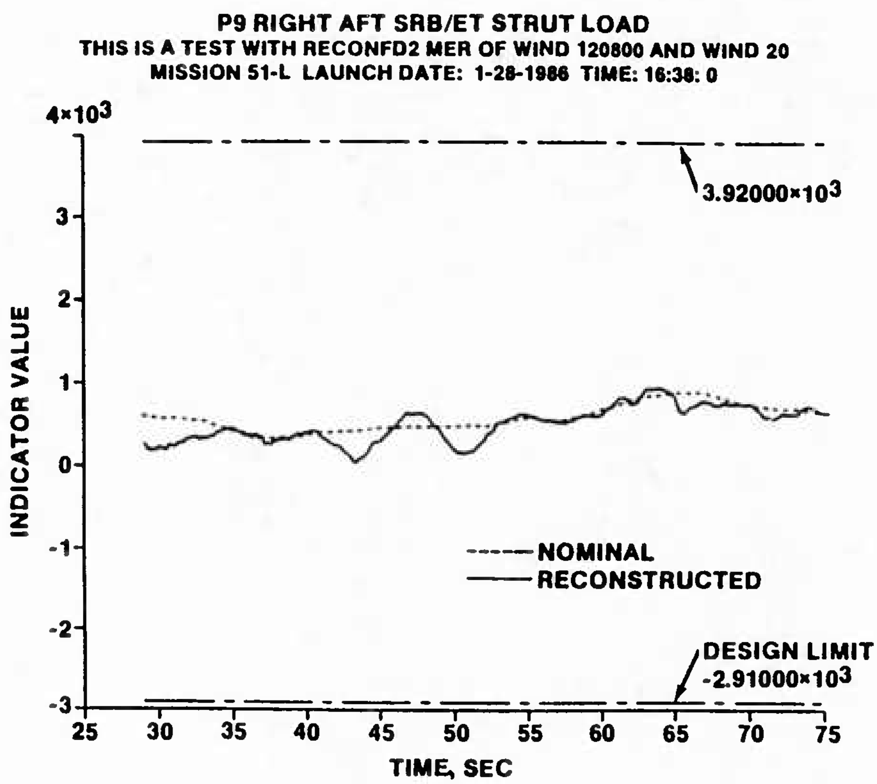

[Ref. 2/13-70 1 of 5]

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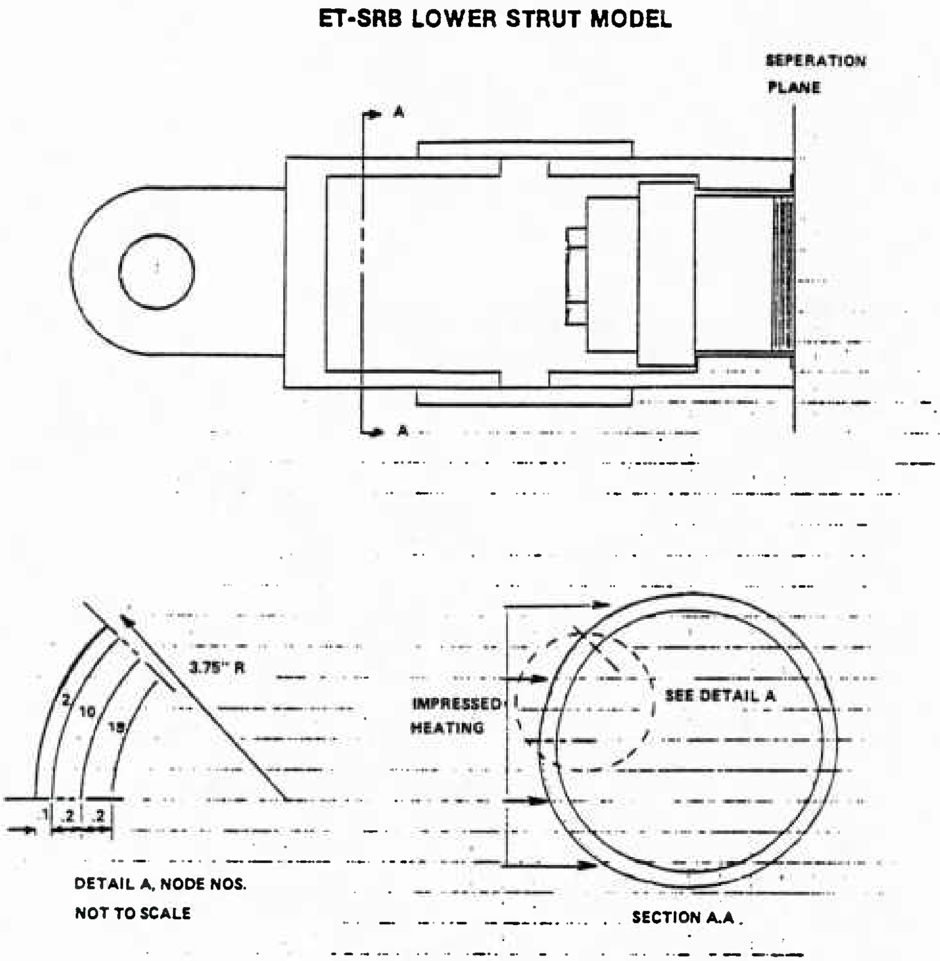
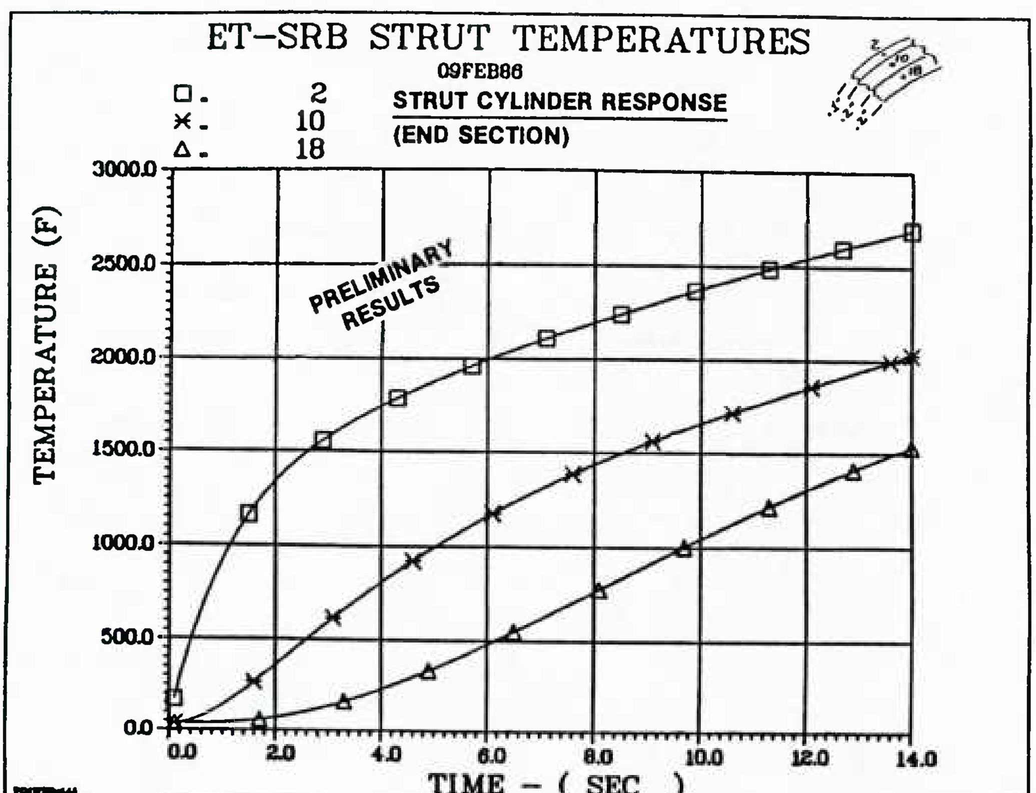
Failure Scenario
Summary Worksheet (Cont)

TIME (SEC)	OBSERVATION	DATA SOURCE	PREMISE	TASK	RESULT
T-66.484 (CONT)				TASK #: ES2-2 DUE OATE: 2/19/86 RESP: T. MOOLIN • OOCUMENT ALLOWABLE PENETRATION SIZES FOR LEAK NO-BURST PHENOMENA	•PRELIMINARY FRACTURE MECHANICAL ANALYSIS SHOWS EFFECTIVE LENGTH LESS THAN 1 INCH. HEATED HOLE TEST IN PROGRESS
T-72.141 THRU T-72.281	RH SRB RGA OIVERGES INCONSISTENT WITH STACK DATA NOTICEABLE DYNAMIC EVENT	RGA# V90R2527CR 25CL 28CR 26CL ACCEL V98A1581C	SRB PLUME HAS FIREO PYRO ON LOWER STRUT (10130-005) OR STRUT FAILURE OUE TO HEAT	TASK #: VT-3 OUE OATE: TBD RESP: VISUAL TEAM • VERIFY PLUME GEOMETRIES ANO IMPINGEMENT OYNAMICS CAD/CAE	
				TASK #: ES3-3 DUE OATE: 2/20/86 RESP: D. DONOHOE • PERFORM THERMAL ANALYSIS OF SRB STRUT	oPYRO SHOULD NOT HAVE FIRED(150°F) oMAX. TEMP. COULD FAIL STRUT
				TASK #: ES4-2 DUE OATE: 2/12/86 RESP: B. HOLDER • COMPUTE LOAD IN MEMBER AT OISCONNECT	
				TASK #: ES4-2 RESP: B. HOLOER • VERIFY RESULTING MOTION	•KINEMATIC ANALYSIS INDICATES THAT A FAILED LOWER SRB/ET AFT STRUT WILL RESULT IN THE RATIO OF PITCH-TO-YAW RATES WHICH WERE OBSERVED IN THE FLT DATA AT APPROX 72.5 SEC

[Ref. 2/3-70 2 of 5]



[Ref. 2/13-70 3 of 5]



[Ref. 2/13-70 4 of 5]

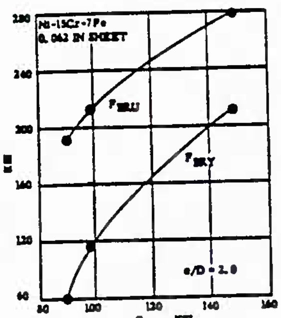


FIG. 2.025 RELATION BETWEEN BEARING PROPERTIES AND TENSILE STRENGTH OF SHEET (INCO 1958)

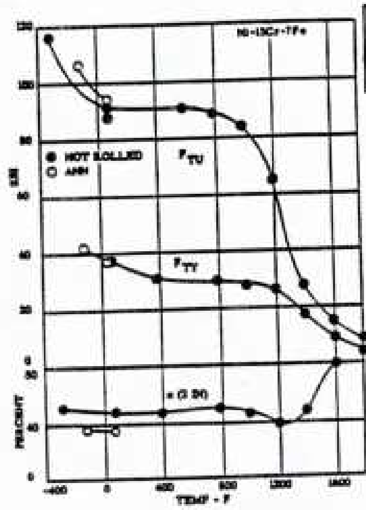


FIG. 3.0311 EFFECT OF TEST TEMPERATURE ON TENSILE PROPERTIES OF HOT ROLLED AND ANNEALED MATERIAL (INCO 1958)

	Ni
15	Cr
7	Fe

INCONEL

EXAMPLE OF INCONEL

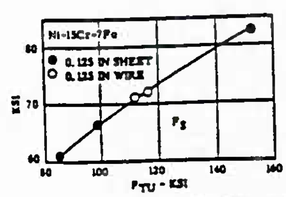


FIG. 3.026 RELATION BETWEEN SHEAR STRENGTH AND TENSILE STRENGTH OF SHEET AND WIRE (INCO 1958)

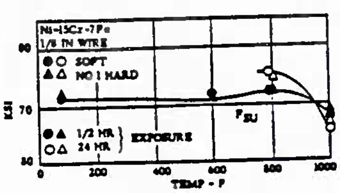
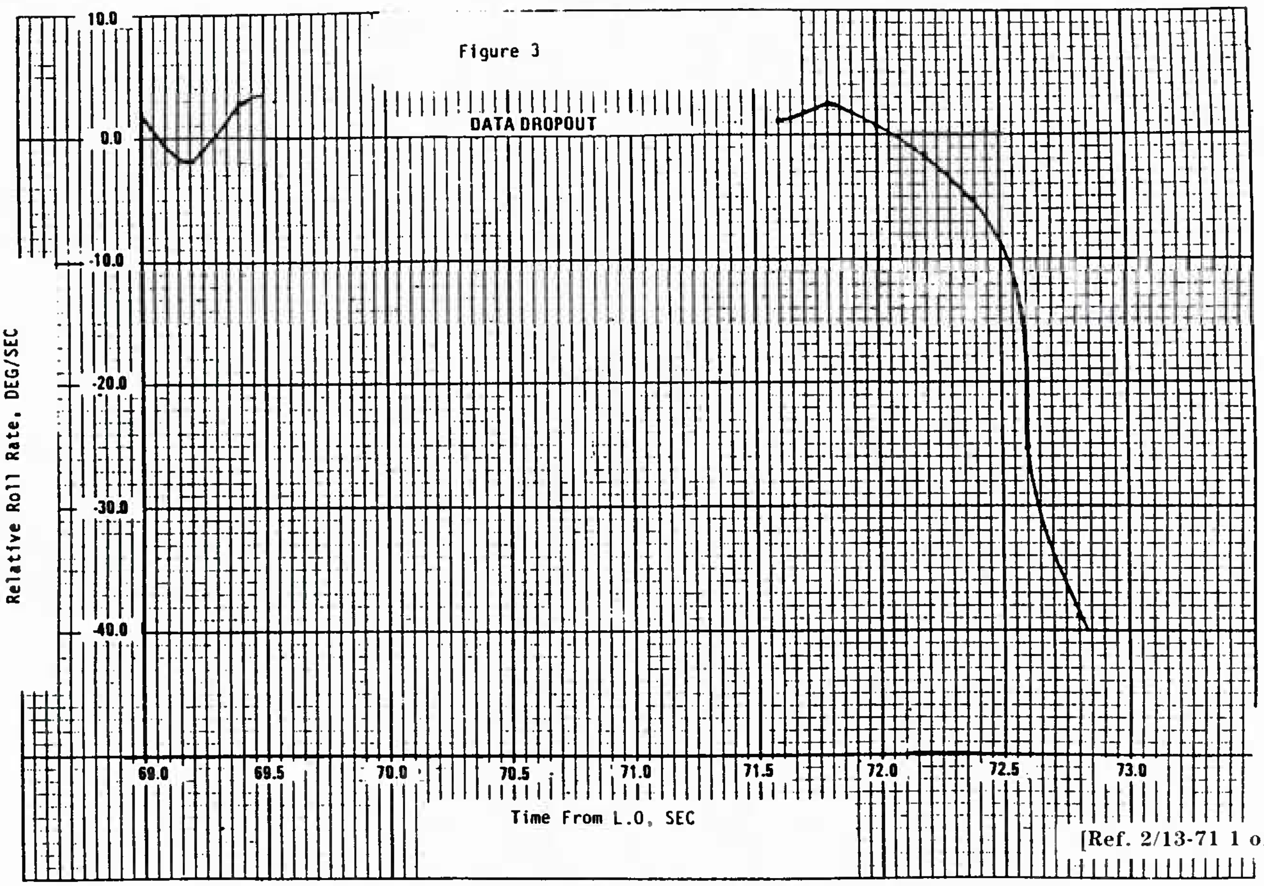
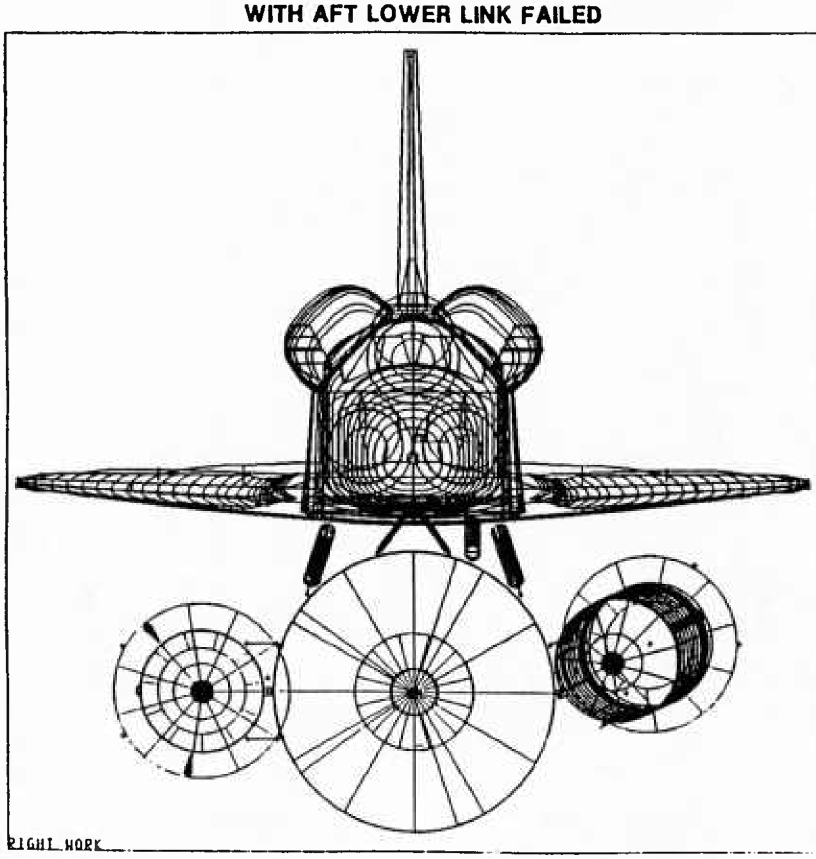


FIG. 3.0321 EFFECT OF EXPOSURE AND TEST TEMPERATURE ON SHEAR STRENGTH OF WIRE (INCO 1958)



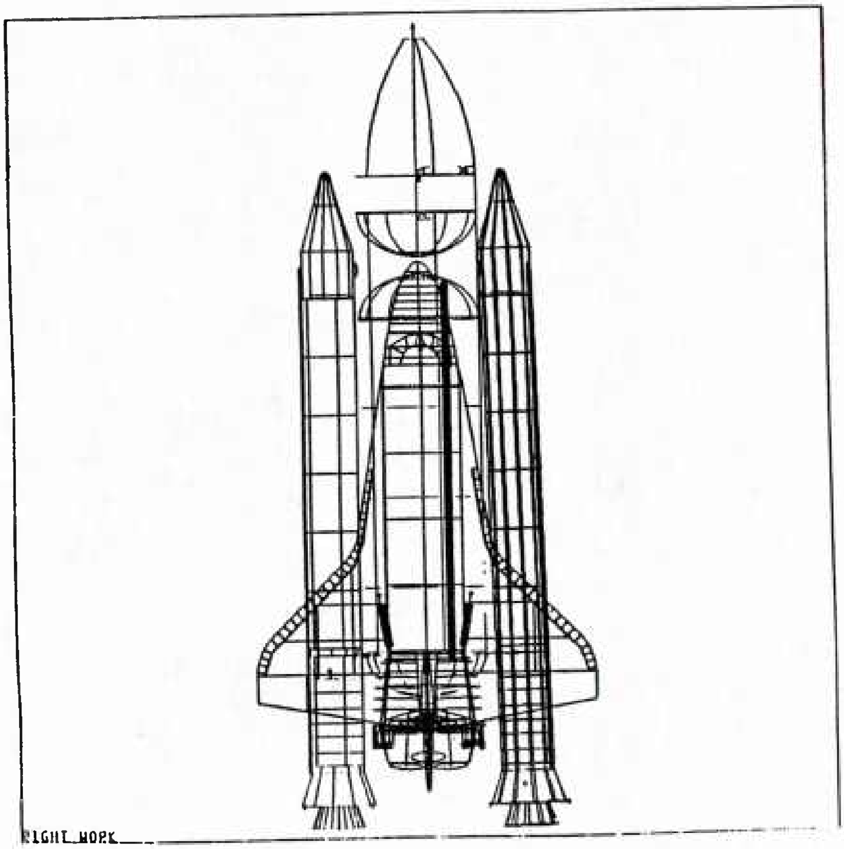
[Ref. 2/13-71 1 of 3]

RSRB ROTATES ABOUT FWD AND AFT ATTACH POINTS WITH AFT LOWER LINK FAILED



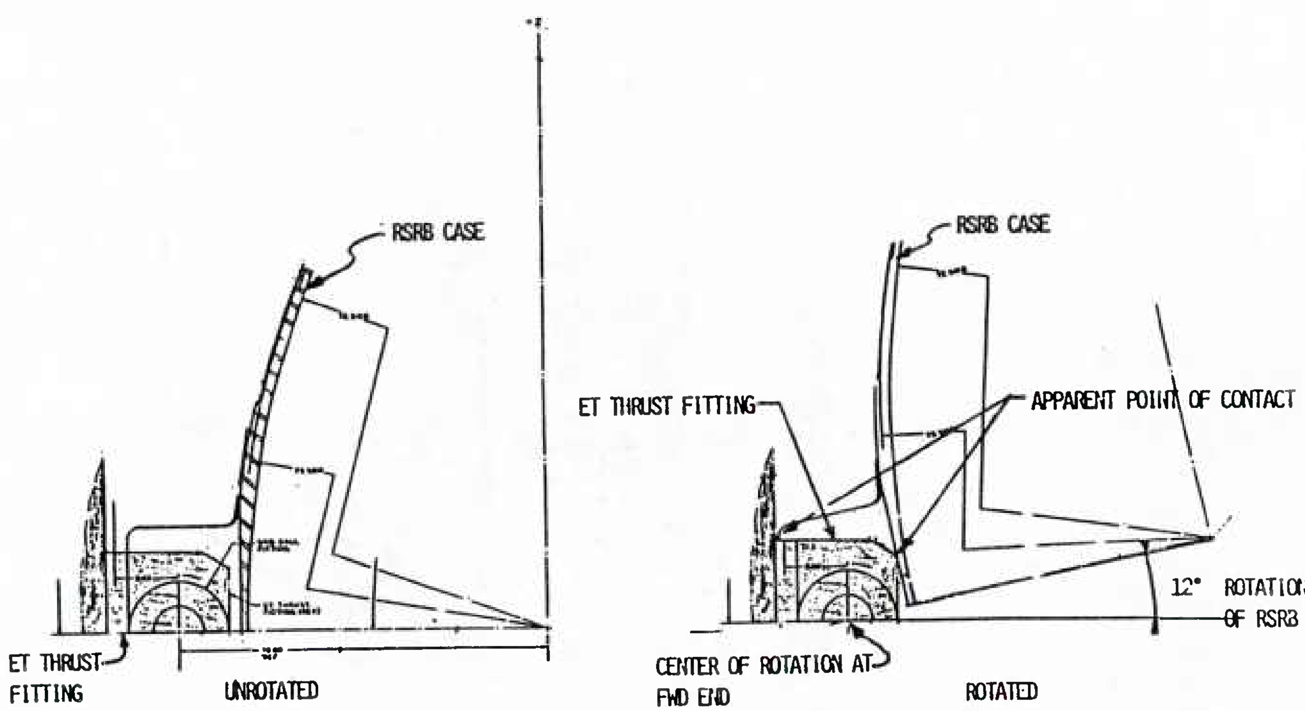
[Ref. 2/13-71 2 of 3]

RSRB IMPACTS ET INTERTANK



[Ref. 2/13-71 3 of 3]

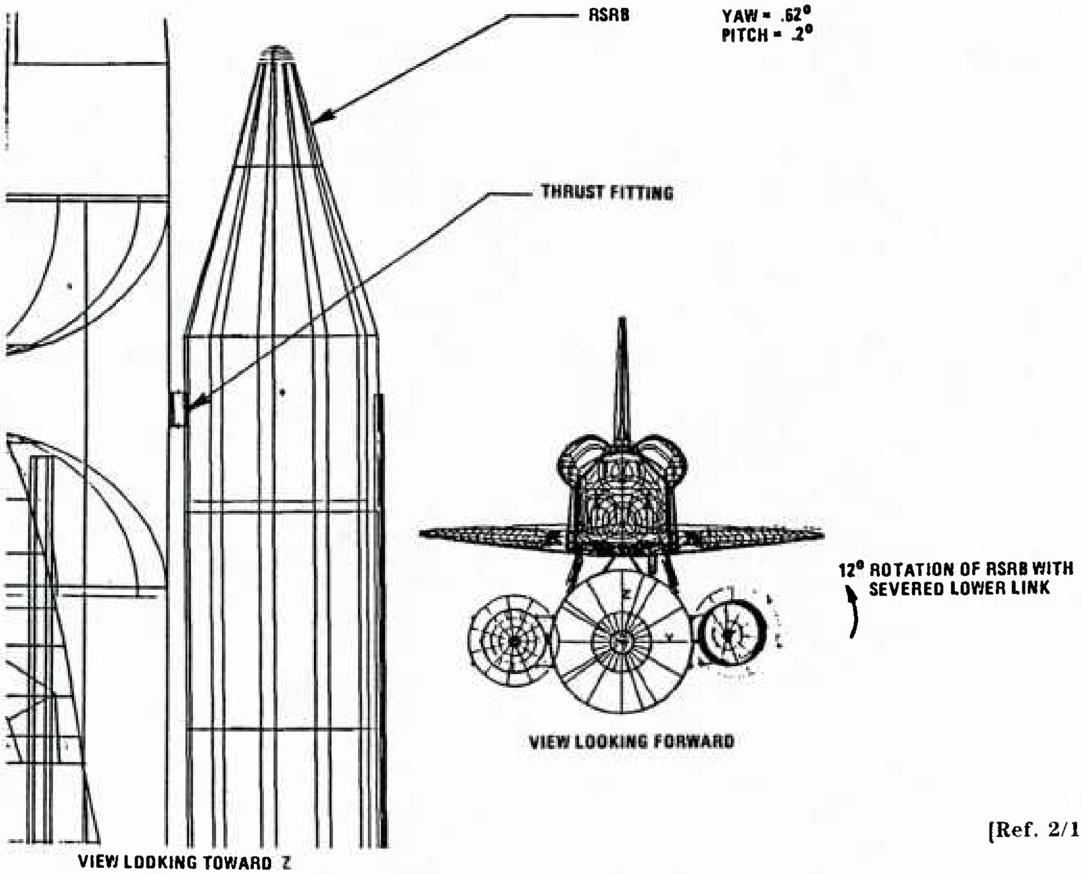
LAYOUT OF FWD ATTACHMENT ROTATION



[Ref. 2/13-72]

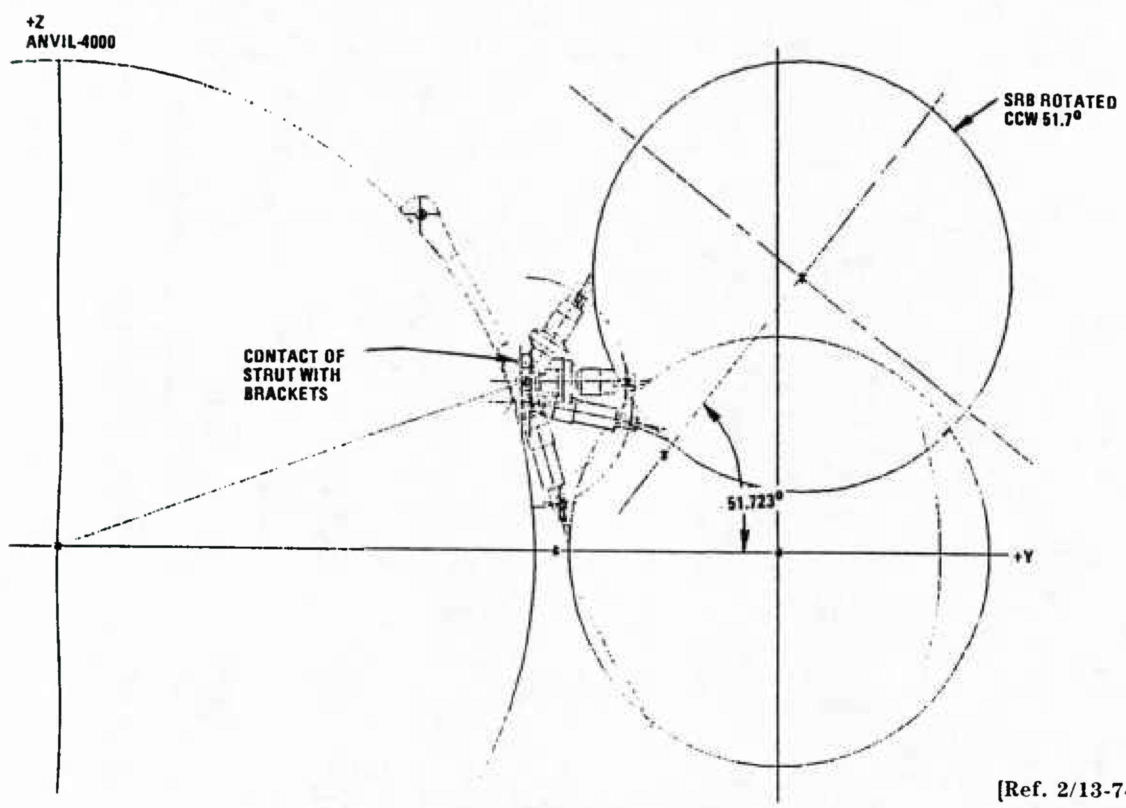
VIEWS LOOKING FORWARD

INITIAL RESTRICTION FOR RSRB ROTATION



[Ref. 2/13-73]

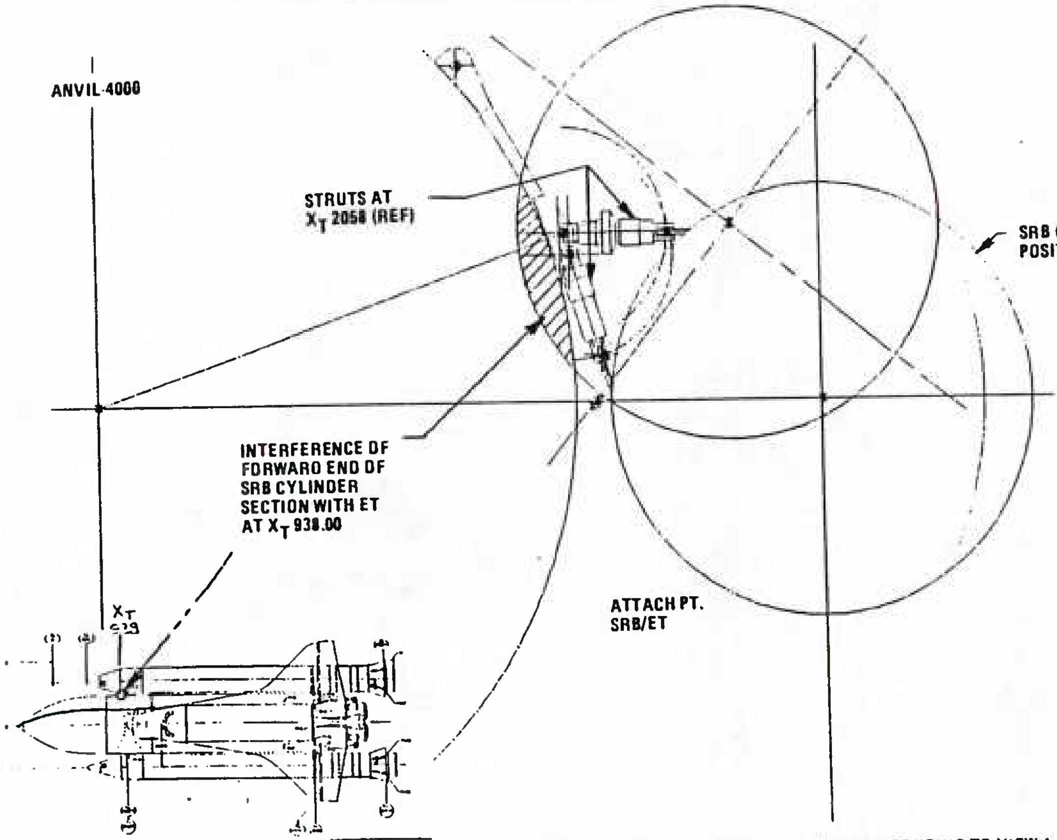
INITIAL RESTRICTION OF AFT RSRB ROTATION



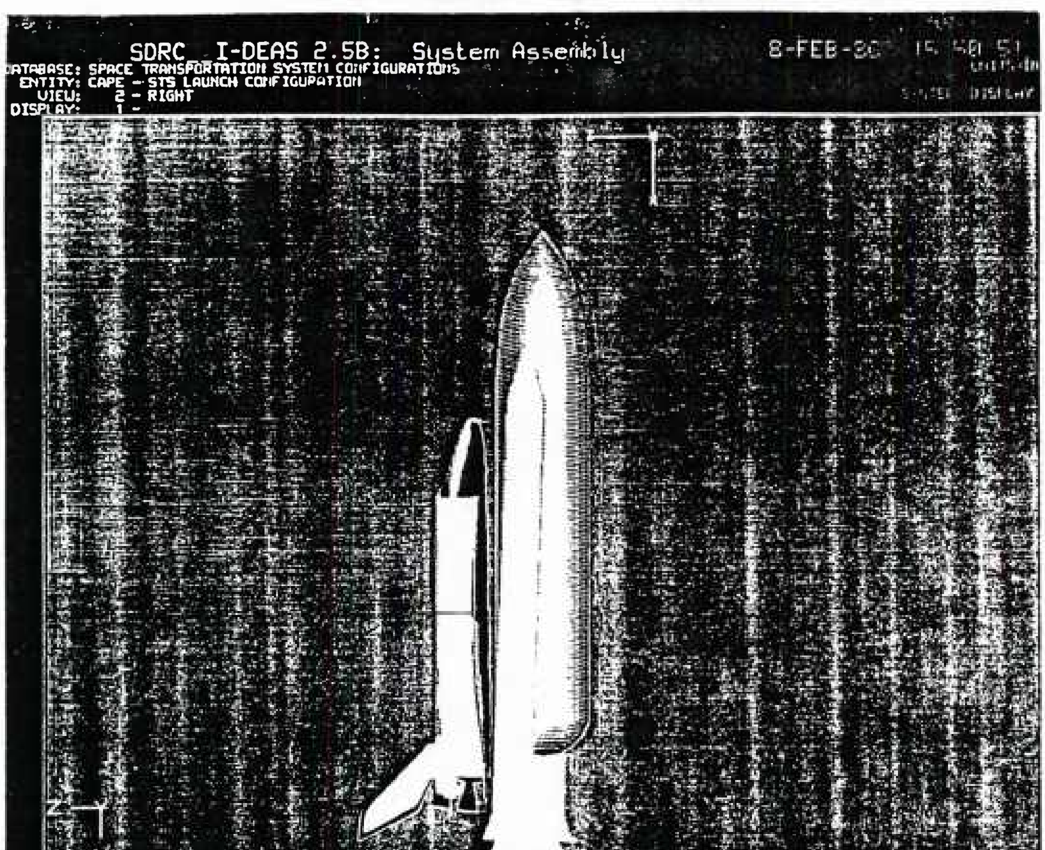
[Ref. 2/13-74]

VIEW LOOKING FORWARD AT X_T 2058

INTERFERENCE AT FWD RSRB AND ET WITH 51.7° ROTATION



[Ref. 2/13-75]



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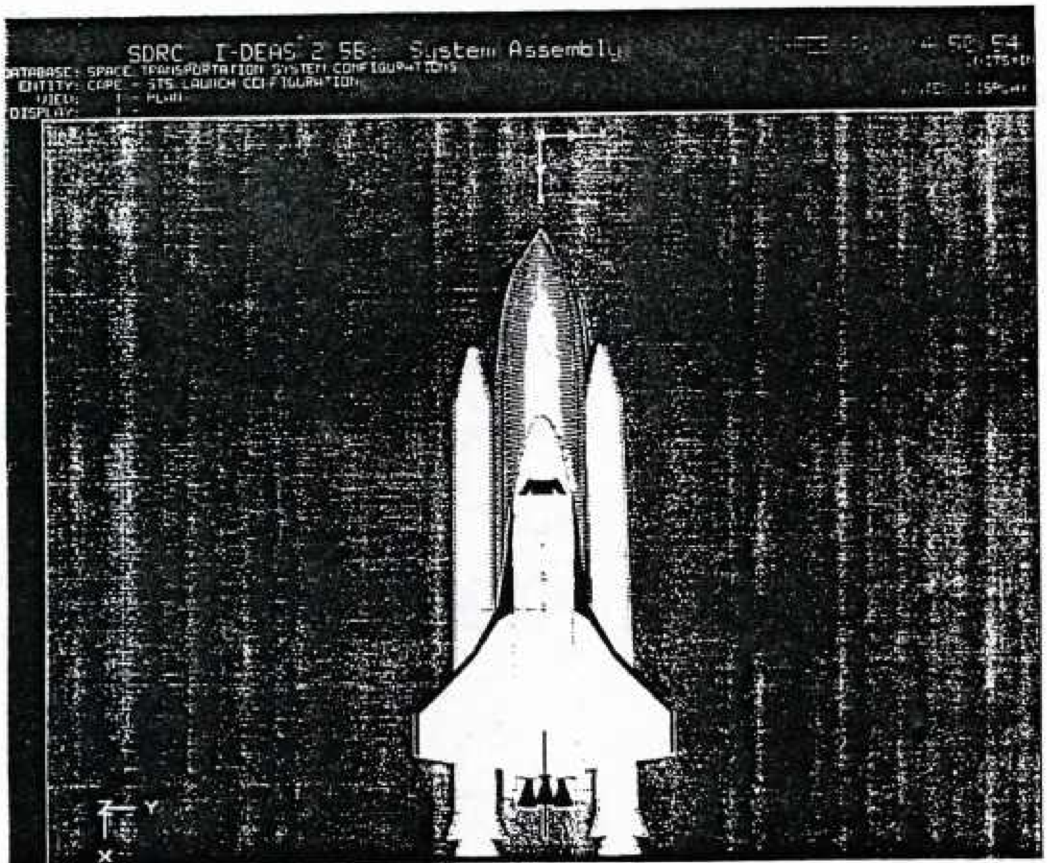

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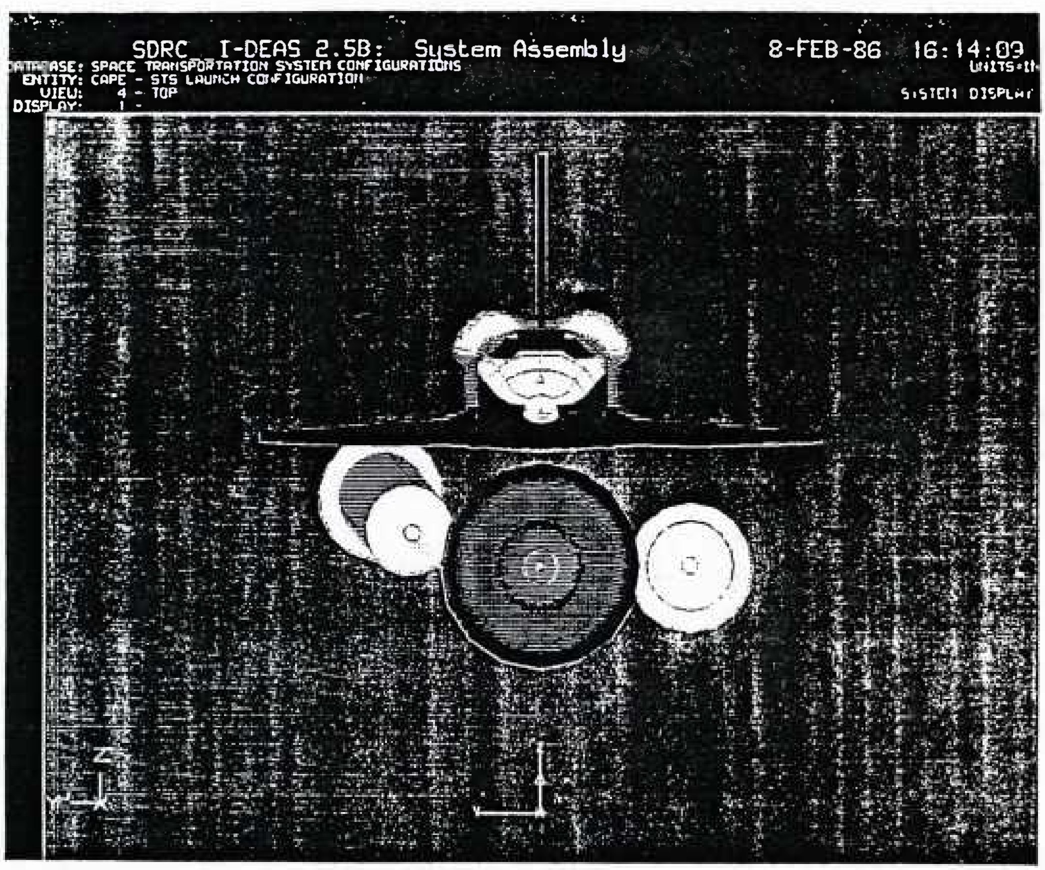
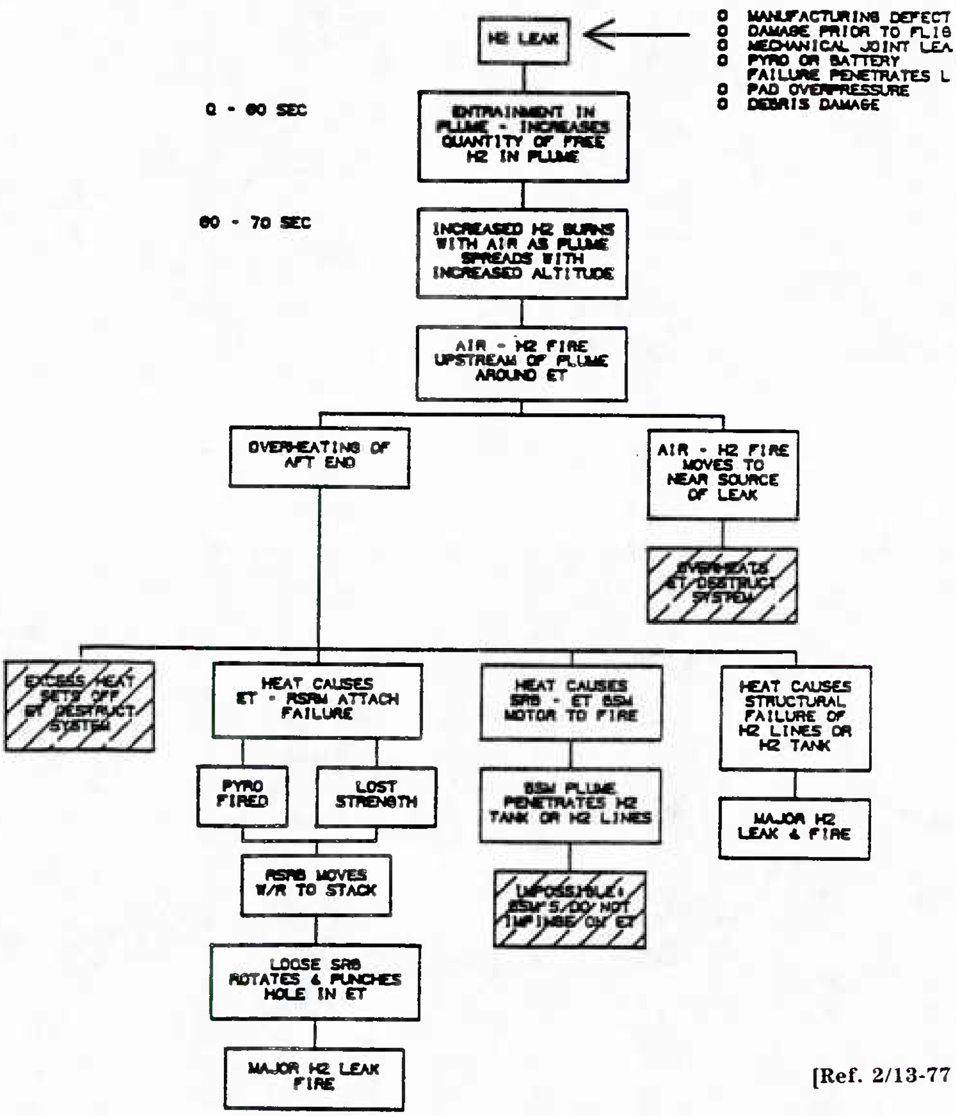


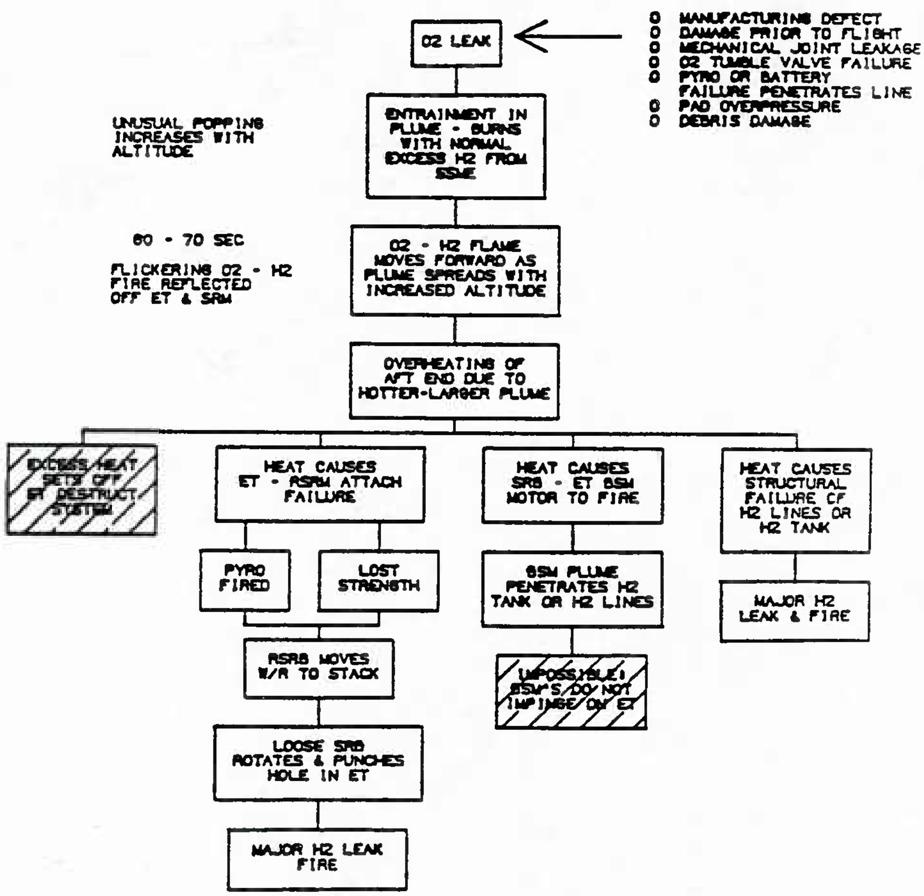
[Ref. 2/13-76 3 of 3]

SMALL/MODERATE HYDROGEN LEAK



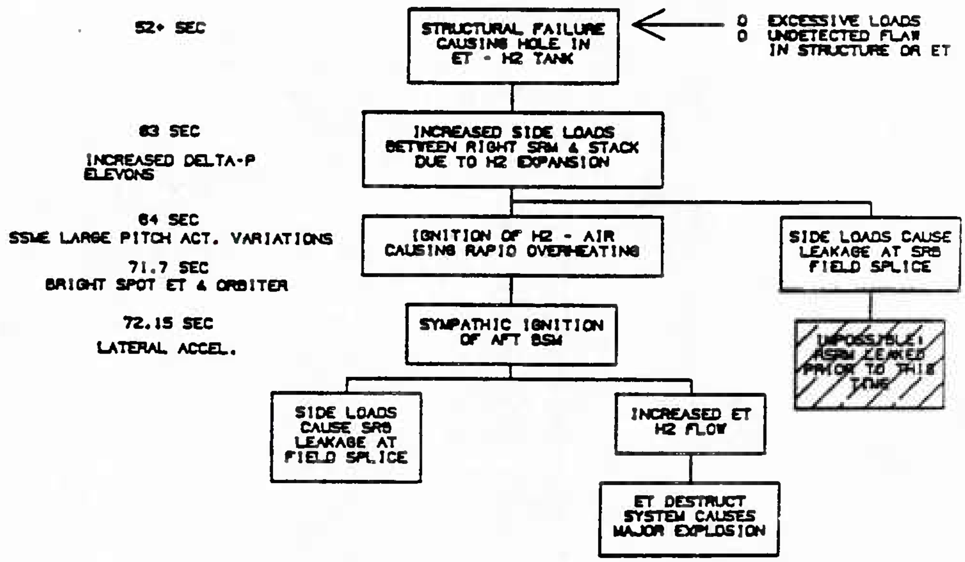
[Ref. 2/13-77 1 of 3]

SMALL/MODERATE OXYGEN LEAK



[Ref. 2/13-77 2 of 3]

STRUCTURAL FAILURE



[Ref. 2/13-77 3 of 3]

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PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—FRIDAY, FEBRUARY 14, 1986

Kennedy Space Center
Cape Canaveral, Florida
The Commission met, pursuant to recess, at 8:05 a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ALBERT WHEELON
ROBERT RUMMEL
DR. ARTHUR WALKER
DAVID C. ACHESON
DR. RICHARD FEYNMAN
MAJOR GENERAL DONALD KUTYNA
ROBERT HOTZ
DR. EUGENE COVERT
ALSO PRESENT:
AL KEEL, Commission Executive Director

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PROCEEDINGS

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CHAIRMAN ROGERS: This morning, the first item on the agenda is the Thiokol discussion, and I thought that in order to lay a foundation for the discussion, I might have Dr. Keel, who is our Executive Director, refer to comments that were made in our executive session, whenever it was—I've lost track of the date, I guess last week—by Mr. McDonald.

DR. KEEL: Yes.

CHAIRMAN ROGERS: I think this will give you a good starting point. We didn't ask the questions at that time. We thought we should have the principals here so we could ask them. So with your permission, we will do that, and you can summarize it, Al.

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DR. KEEL: Yes. I will just, Mr. Chairman, at your direction just—we have selected excerpts from this, and let the record show that this is excerpts from the closed session of Monday, Feb-

ruary 10th, before the Commission. I will begin—this is after Mr. McDonald first rose and explained the fact that there was a discussion prior to launch expressing concern about the temperature at the seal and the performance of the O-rings at that temperature.

CHAIRMAN ROGERS: You really said "discussions," so there were more than one.

DR. KEEL: That is correct.

"Chairman Rogers: Am I hearing you say that you recommended against launch and you never changed your mind?

"Mr. McDonald: No, I did not say that. We did change our mind afterwards.

"Chairman Rogers: What brought you to that decision?

"Mr. McDonald: Well, the data that was reviewed. NASA concluded the temperature data we had was inconclusive, and indeed a lot of the data was inconclusive, because the next worst blow-by we had ever seen in a primary seal, in a case to case seal joint, was about the highest temperature we had launched at,

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and that was true."

And I'm skipping ahead now in the testimony:

"Chairman Rogers: Did you change your mind?

"Mr. McDonald: Yes. The assessment of the data was that the data was not totally conclusive that the temperature could affect everything relative to the seal, but there was data that indicated that there were things going in the wrong direction, and this was far from our experience base, the conclusion being that Thiokol was directed to reassess all the data because the recommendation was not considered acceptable at the time, at the 53 degrees.

"NASA asked us for a reassessment and some more data to show the temperature in itself could cause this to be a more serious concern than we had said it would be. At that time, Thiokol in Utah said that they would like to go off-line and caucus for about five minutes and reassess what data they had there or any other additional data.

"That caucus lasted for I think a half hour before they were ready to go back on, and when they came back on they said they had reassessed all the data. They had come to the conclusion that the temperature influence, based on the data they had available to them, was inconclusive, and therefore they recommended a

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launch.

"Chairman Rogers: When you say 'inconclusive,' what does that mean?

"Mr. McDonald: Well, the fact is—

"Chairman Rogers: You told them the day before not to do it, and then now you've got some more data and you say it is inconclusive and you changed your mind?

"Mr. McDonald: I was not back at Wasatch when that discussion was being held. I was at Kennedy, and I do not know what other data they were looking at, other than the charts that I had in front of me and others that they had in front of them at both KSC and Marshall. I do not know.

"I do know they came back on and said they had reassessed it and concluded that it was okay to launch, and at that point in time Thiokol was requested to put that in writing.

"Chairman Rogers: Well, I think in view of the very serious nature of this and the fact that it will be scrutinized for years, that we should have precisely what the data was before we present it."

CHAIRMAN ROGERS: Well, this meeting today is in the nature of an investigation and not really a hearing, so it is not necessary to swear anybody in.

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When we have our regular formal meetings, we swear all our witnesses, but this really is for us to assess the facts and to see how they will be presented later on.

So, although it is going to be recorded, it is not going to be sworn testimony. That doesn't mean we don't want the truth, but it means that this is not formal, this is informal and in the nature of an investigation, and we want to have you feel free to have a discussion about the facts.

And what I have in mind particularly, and I think the rest of the Commission does, is to be sure that we know precisely what the facts were, who was involved, and as much as you can remember what was said, and if there were any documents relating to those discussions we would like to take a look at the documents.

And if there was any impounding of documents, critical documents, we would like to know that, when it happened, who has them if there are documents of that kind—all of the facts that relate to this very critical period in the launch.

MR. REINARTZ: Mr. Chairman, I'm Stan Reinartz from the Marshall Space Flight Center. I'm the project manager for our propulsion elements.

What we would propose, subject to your

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concurrence, is that Mr. Mulloy, our SRB project manager from Marshall, give you the background that led up to the discussion that we had with Morton Thiokol, list the participants, including myself, in that telecon, and run through what we had, a very brief summary, and then we would provide Morton Thiokol the opportunity to then discuss on their side of the situation how they viewed it, if that is acceptable.

CHAIRMAN ROGERS: Sure.

MR. REINARTZ: All right, Sir, Larry Mulloy.

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STATEMENT OF LARRY MULLOY

MR. MULLOY: Mr. Chairman and members of the Commission:

Good morning.

(Viewgraph.) [Ref. 2/14-1 1 of 4]

The decision to scrub the 51-L mission on January the 27th was made at approximately 1:00 p.m. by my recollection. Now, I had put approximate symbols in front of these times but I don't have the precise times.

But of course during the launch process, the project managers at Marshall and Dr. Lucas are on a communication net with personnel back at our Huntsville operations support center and also personnel that we have in the backup firing room here at Kennedy. We communicate with those people—they are technical specialists—relative to any problems or during the launch count any anomalies that come up, any potential launch commit criteria violations that come up during the course of the count.

And then subsequent to the launch or subsequent to a launch scrub, we stay on the net and communicate with those people relative to our ability to turn around within 24 or 48 hours on any concerns that might be related to that.

So what happened on the 27th, after the

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decision to scrub, we polled our support personnel who were on the net, and that includes USBI, Booster Production Company, and Morton Thiokol personnel, regarding any constraints to a 24 hour recycle, in other words of going at 9:38 on the 28th of January.

This was about 30 minutes after the launch scrub. Some input did come back, and what they had looked at was all elements of the system. Our request then went into the system, which Morton Thiokol will discuss as to how they reacted to that request, any constraints for a 24-hour turn-around.

The input that I got back about 30 minutes after launch scrub on the net was that the concerns that had been worked were related to the recovery battery temperatures on the solid rocket boosters. We have a launch commit criteria red lined for those, and the fuel service module, hydrozene modules in the aft skirt that work the thrust vector control system on the solid rocket booster.

At that time, at about 1:30, there was no concern or no input back relative to any concerns for the solid rocket motor, considering the predicted temperatures.

The next event that I am aware of is about 5:15 that afternoon. In response to that request to

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work, Al had—well, I will let him cover what he did, but he called our MSSE resident office, Cecil Houston, to inform him that Thiokol engineering did have some concerns regarding the function of the O-rings at the predicted temperatures.

CHAIRMAN ROGERS: Could I interrupt. Was there a discussion about the weather prior to the scrub?

MR. MULLOY: No, sir.

CHAIRMAN ROGERS: In other words, the first discussions you remember about the weather occurred at 5:15?

MR. MULLOY: Yes, sir. It was in the assessment of, do we have any problems with going at 9:38 on the 28th of January. And then it was pointed out that the temperatures were predicted to get below freezing, and there were a lot of aspects of that consideration for the work. Of course, there was water on the pad and potential bursting of pipes, ice on the external tank, et cetera.

MR. ALDRICH: Excuse me, Larry. To be consistent with the discussion we had yesterday, between the 1:00 p.m. meeting and the 5:15 p.m. meeting there was a mission management team that met, too, which Jess Moore and myself conducted in the launch control center, where we met with all of the projects and discussed whether we should pick up for launch the next day.

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And at that meeting, we elected to pick up, and we were most concerned about the temps on the facility. But we asked the facility and the other elements to review the temperatures and call if we developed any complications downstream which would make that decision.

CHAIRMAN ROGERS: So at 2:00 o'clock—

MR. ALDRICH: So at 2:00 o'clock we called the level two and the level one of the projects, which fits between these two times.

CHAIRMAN ROGERS: Excuse me. I think I've changed my mind. If you don't mind, I think all the people from Thiokol that are going to talk today, let's swear them all in at one time, because if we don't and we have sworn the other witnesses it may in retrospect look a little odd.

So if you will all stand up and be sworn.

(The following witnesses were sworn: Jerry Mason, Cal Wiggins, Joe Kilminster, Robert Lund, Don Ketner, Roger Boisjoly, Arnie Thompson, Al McDonald, and Boyd Brinton.)

MR. MULLOY: Sir, I will show on the chart the participants who are here.

CHAIRMAN ROGERS: When you are asked to comment, if you would identify yourself so the reporter will be able to record this.

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MR. MULLOY: And following up on Arnie's comment—

CHAIRMAN ROGERS: Now, as much as possible, when you say we called the facility, if you remember who you called or who somebody called, let's use names, so if we trace what happened we have people instead of buildings.

MR. MULLOY: Yes, sir, will do.

Following up on Arnie's comment, what I just stated relative to our concerns, I stated in that mission management team meeting that we had worked those two concerns. And I cannot recall any discussion in that mission management team meeting relative to SRM temperatures.

MR. ALDRICH: I do not recall such discussion.

MR. MULLOY: Now, Al called the resident office. He will testify to that. Cecil Houston about ten minutes later then was able to get in contact with Jud Lovingood, who is here today—he's the Deputy Manager of the Shuttle Projects Office, of Mr. Reinartz—and relaying this concern that Al had pointed out to establish a telecon with Reinartz, myself, George Hardy at the Marshall Space Flight Center, and engineering and program people who were at the Thiokol Wasatch division

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in Utah.

That telecon did occur. I did not participate in that telecon. They were unable to get in touch with me at that time, but that telecon occurred between Lovingood, Reinartz, and Thiokol personnel, and other personnel at Marshall Space Flight Center. And the result of that was to listen to the concerns as expressed here just in oral transmission. They had no data at that time.

The result of that was to set up a telecon where we could get all of the data transmitted to all of the parties and have more personnel participating in that.

CHAIRMAN ROGERS: And we will be able to be told by somebody what the comments were at that time?

MR. MULLOY: Yes, sir. At the 8:15?

CHAIRMAN ROGERS: Yes.

MR. MULLOY: Yes, sir, and I'm going into that.

DR. WHEELON: Is it your practice to record these telecons on magnetic tape?

MR. MULLOY: No, sir.

DR. WHEELON: There is no record of that—

MR. MULLOY: To my knowledge, this telecon was not recorded.

DR. WHEELON: Are others, and this not? Or is it your practice not to do so?

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MR. MULLOY: It is our practice not to do so.?

DR. COVERT: Do you write up notes?

MR. MULLOY: Yes, sir.

DR. COVERT: Does everybody write up notes? Or if you don't know—

MR. MULLOY: I don't know, sir, that everybody writes up notes. I am aware that I wrote up notes in this case. [Ref. 2/14-1 2 of 4]

DR. WHEELON: May we have copies of those notes?

MR. MULLOY: Yes, sir.

DR. COVERT: We might want to collect these different notes and see if they all attended the same meeting.

MR. MULLOY: I don't think, when you see the number of participants—there were some 30 participants—I cannot say that everyone had notes.

DR. COVERT: I understand. Press on, Larry.

CHAIRMAN ROGERS: Well, we should, if there are notes, we should have them, because there may be a difference of opinion on what was said, and it would be helpful to us to have them all. And if there are some that exist and we don't have them, that would be a mistake. We should get them all.

MR. MULLOY: Yes, sir. We will collect those.

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That telecon was a little late starting. It was intended to be set up at 8:15. The 30 minute delay was due to the transmission of the engineering material from Wasatch in Utah to the Kennedy Space Center and to the Marshall Space Flight Center. That did arrive, though, in Huntsville and here at Marshall, and the telecon was begun at 8:45.

And Thiokol will then present to you today the data that they presented to us in that telecon. I will not do that. The bottom line of that, though, initially was that Thiokol engineering, Bob Lund, who is the Vice President and Director of Engineering, who is here today, recommended that 51-L not be launched if the O-ring temperatures predicted at launch time would be lower than any previous launch, and that was 53 degrees. Yes, sir.

MR. WALKER: May I ask a question. I wish you would distinguish between the predicted bulk temperatures and the O-ring temperatures. In fact, as I understand it, you really don't have any official O-ring temperature prediction in your models, and it seems that the assumption has been that the O-ring temperature is the same as the bulk temperature, which we know is not the case.

MR. MULLOY: You will see, sir, in the Thiokol

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presentation today that that is not the case. This was a specific calculation of what the O-ring temperature was on the day of the January 1985 launch. It is not the bulk temperature of the propellant, nor is it the ambient temperature of the air.

It was Thiokol's calculation of what the lowest temperature an O-ring had seen in previous flights, and the engineering recommendation was that we should not move outside of that experience base.

I asked Joe Kilminster, who is the program manager for the booster program at Thiokol, what his recommendation was, because he is the gentleman that I get my recommendations from in the program office. He stated that, based on that engineering recommendation, that he could not recommend launch.

At that point I restated, as I have testified to, the rationale that was essentially documented in the 1982 critical items list, that stated that the rationale had been that we were flying with a simplex joint seal. And you will see in the Thiokol presentation that the context of their presentation is that the primary ring, with the reduced temperatures and reduced resiliency, may not function as a primary seal and we would be relying on secondary.

And without getting into their rationale and

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getting ahead, the point, the bottom line, is that we were continuing—the assessment was, my assessment at that time was, that we would have an effective simplex seal, based upon the engineering data that Thiokol had presented, and that none of those engineering data seemed to change that basic rationale.

Stan Reinartz then asked George Hardy, the Deputy Director of Science and Engineering at Marshall, what his opinion was. George stated that he agreed that the engineering data did not seem to change this basic rationale, but also stated on the telecon that he certainly would not recommend launching if Thiokol did not.

At that time Joe Kilminster requested a five minute off-net caucus, and that caucus lasted approximately 30 minutes.

DR. COVERT: Larry, I'm confused, and could I go back about four meatballs here. What is a simplex joint seal?

MR. MULLOY: No redundancy.

DR. COVERT: That's a single O-ring?

MR. MULLOY: Yes, sir.

DR. COVERT: Is this common notation, or is it named after Charlie Simplex or something?

MR. MULLOY: Perhaps it is an unfortunate

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phrase. I guess I consider "simplex" singular and "duplex" dual, and I was using it in that context.

MR. WALKER: So you're talking about the secondary seal.

MR. MULLOY: Yes.

MR. WALKER: In other words, if the first one didn't work the second one would?

MR. MULLOY: Yes, that is correct. And the engineering rationale you will see for accepting the situation was counting on the secondary seal, and that will be developed. Yes, sir.

MR. SUTTER: Is this in line with—I thought the philosophy was at least dual redundancy on there being critical.

MR. MULLOY: Yes, sir, that——

MR. SUTTER: Where is the rationale that says a single ring is okay and anything single is okay?

MR. MULLOY: The design goal is certainly to have redundancy in critical systems, and in late 1982 it was recognized that this joint design did not provide that redundancy and the criticality on the critical items list, the criticality for this joint was changed from criticality 1-R, which is redundant, to a criticality 1.

And there are numerous systems on the shuttle system that are criticality one. They are not redundant. They are indeed criticality one.

This was changed into that criticality, recognized, and the rationale for the acceptance of that

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condition based on analysis and comparison to a similar joint design which flies with a single O-ring, namely the Titan, which at that time had had some 70 successful launches.

So since December of 1982, we have been operating the system with the recognition that under worst case conditions, worst case tolerance buildups, and certain other conditions, that you could indeed not have redundancy in the joint. And what I am stating here is that my con-

clusion, based upon engineering data presented, was that that situation here was not any different.

MR. HOTZ: But doesn't the Titan use a different joint?

MR. MULLOY: It is a similar joint, sir. It's a tang and clevis with a single O-ring that they don't leak check. The primary––

MR. HOTZ: But isn't it reversed, that the tang and the clevis go the opposite way?

MR. MULLOY: Yes, sir, I believe it is, yes, sir. It is the tang is down—or, excuse me, the tang is up in flight direction and the clevis is down.

MR. HOTZ: So you're not basing it on exactly the same type of a joint.

MR. MULLOY: No, sir; similar.

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MR. WALKER: There's also a quasi-seal of the insulation, and that may be a very critical difference.

MR. MULLOY: There is an interference fit in the insulation, which is not related to the physical joint design. But it is related to the insulation layup.

CHAIRMAN ROGERS: Larry, in criticality one problems in the past, do you remember any other examples where there was such a late decision on a criticality one and whether there were disagreements among the participants?

MR. MULLOY: No, sir, I do not. We are researching those records now for the total shuttle system, and I'm certainly doing that for the SRM. I am not aware since December of 1982. I took over the project in November of 1982.

I do not remember any changes from a criticality 1-R to a criticality 1 in the solid rocket booster. What we're looking at now is what are all of the criticality ones and getting a listing of those for the solid rocket booster. The total system is doing that, I believe, are you not, Arnie?

MR. ALDRICH: Yes, sir, Mr. Chairman. We are preparing a summary for you on the critical items and the nature of some of them and the total set, and that

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will be available and we can discuss it later today if you wish.

MR. SUTTER: Well, I think that that discussion shouldn't be discussed today, because I think it is a discussion that should take all day some time. This is not exactly the subject we're discussing now, but I would like to some time have a discussion on the design criteria, and is redundancy an objective or a requirement and how do you go from 1 to 1-R, how do you decide these things, and how do you say that at one place you've got redundancy and then you don't? I think that is a whole day's discussion.

CHAIRMAN ROGERS: I agree. Dr. Keel, would you make a note, and we will schedule one as soon as we can. I think you're absolutely right.

DR. RIDE: Larry, in your discussion relative to the CIL or your decision relative to the CIL, does that mean that you were assuming what the effects of temperature were on a secondary seal?

MR. MULLOY: No, I don't believe that was considered at that time. I believe the total rationale for retention on the CIL from 1-R to 1 is attached there, and all of the considerations are there, and I do not believe temperature is included in that consideration.

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DR. RIDE: I guess I meant, when you were making this decision at the meeting were you assuming that the effects of the cold weather––

DR. MULLOY: Yes.

DR. RIDE: that you were assessing were going to affect the secondary seal and not the primary seal?

MR. MULLOY: Yes, and that will come out, I think, in Thiokol's——

MR. REINARTZ: Wait a minute. Excuse me. Repeat your question, Sally, if you would, please, for Larry.

DR. RIDE: Okay. I guess I was just looking at the CIL, and it says that—it looks to me like it says that the primary O-ring is I heard now a single point failure, because you can't count on the secondary O-ring. Is that a fair assessment of the CIL?

MR. MULLOY: That is correct.

DR. RIDE: And then I guess my question was, in your discussion the day before launch and the evaluation of the effects of the cold temperatures on the O-ring, if you were going to base your decision on the CIL it seems that you would have to assume that the cold temperature affected the secondary O-ring, but not the primary O-ring, since the primary O-ring is the criticality one.

MR. MULLOY: Let me restate. I did not base

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my decision on the CIL. The CIL states that we have a simplex—the rationale for a simplex seal. We do not have a redundant seal. My assessment at the time that I made this comment right here, that it did not seem to change that basic rationale, was related to the engineering data that Thiokol will show, that shows that even with the effects of the cold temperature that we expected to have a simplex seal under the worst case conditions, the basic rationale being simplex and not redundancy, a single functioning O-ring.

DR. RIDE: A simplex seal where the one O-ring was the primary O-ring?

MR. MULLOY: No. The rationale, you will see, says we were counting on the secondary O-ring to be the sealing O-ring under worst case conditions and the worst case analysis that is presented here.

DR. RIDE: But doesn't the CIL say you can't count on the secondary O-ring?

MR. MULLOY: Yes, and you have to see the engineering development for the rationale that states that if the cold effect on the primary O-ring—and I'm getting into Thiokol's engineering data, but basically, and I guess I should say what my understanding was, the effect of the cold on the O-ring is it reduces the diameter of the O-ring. It also reduces the resiliency of the

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O-ring.

The primary seal under the leak check pressure is seated in the wrong direction. Therefore, there is a time required before the primary O-ring can become effective. The consideration was that if the primary O-ring under the worst case conditions, which was not predicted, incidentally, as you see the engineering data—it was predicted that the primary O-ring would have sufficient compression and sufficient resiliency to extrude into the gap and serve as a functioning seal.

But we went a step further and said, suppose under worst case conditions the cold effect caused the primary O-ring to be totally ineffective. If the primary O-ring is ineffective, the secondary O-ring, which is in a position to seal, will be pressure actuated in the time before the joint rotates.

DR. FEYNMAN: Excuse me. I'm getting very confused, and I would prefer a more logical description and order. I think your original plan was to explain the order of things quickly and then we could go back and listen to all of the arguments in the order in which they were presented. This way things are being pulled out of order and it is hard to follow. Is that okay?

CHAIRMAN ROGERS: Yes, but if anybody else has questions they should ask them.

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DR. FEYNMAN: Yes, but we have the opportunity for some orderliness——

CHAIRMAN ROGERS: We haven't followed the procedure of not interrupting so far, so let's go ahead.

[Laughter.]

CHAIRMAN ROGERS: If anybody has a question he wants to ask, he should ask it.

MR. REINARTZ: Mr. Chairman, let me do one thing. When we considered the question of the joint and the simplex, that that was the conclusion of a worst case possibility. What we were looking at from the data presented was that there was an increased probability that you may not have a primary seal.

But we did not accept the fact that the basic starting condition was assume that you had no primary seal to start with. There was an increased probability of not sealing with the primary, and if that did occur then we had a simplex with a secondary seal, and that was the final point that Larry was making there.

So there was some increased probability, but it was not a foregone conclusion, that the primary seal would not operate, and I think that will come out today.

CHAIRMAN ROGERS: It's a little difficult to compare what you just said with what the document says, though.

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Let's go ahead.

MR. MULLOY: Yes, sir. This is the list of the total participants, the total number of participants in the telecon. At Morton Thiokol in Utah were these. We brought the principal technical disciplines. We did not bring all of these people here today, Mr. Chairman, and of course they are available.

But in Florida there was myself, Stan Reinartz, Cecil Houston, and Jack Buchanan. Did he get in? Okay, Jack Buchanan is here also. He's the manager of the launch support services here, and Al McDonald; at Marshall, the following individuals with the disciplines that they represent indicated. There are some 30 people then involved in that 8:45 telecon.

The next chart, please.

(Viewgraph.) [Ref. 2/14-1 3 of 4]

Moving on, then. That telecon lasted, then, after Thiokol requested their caucus and it took about 30 minutes, at that point Mr. Kilminster came back on the net on the telecon and read the rationale for recommending launch. And that was rapid-faxed to me here at Kennedy about 35 minutes later, and I will show you what that says.

I guess it is an assumption on my part, Joe, that you were reading from that. It sounded the same.

MR. KILMINSTER: I was reading from some other

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notes.

GENERAL KUTYNA: During that 30 minute off-the-net conference, were there telephone calls between the centers and Thiokol to discuss this thing?

MR. MULLOY: No, sir. We were off the net and we were not carrying on any discussion on this. Those of us who were here at Kennedy were discussing among ourselves and relooking at some of the engineering data, and I'm sure that perhaps George Hardy can testify to the fact of what they were doing back there. I don't know.

We did not carry on any continuing dialogue during that 30 minute period.

What Joe Kilminster then essentially stated at the end of that telecon and what he subsequently faxed down 35 minutes later, which I then had, was:

"Calculations show that the SRM-25 O-rings will be 25 degrees-20 degrees colder than SRM-15"—which was that one that was at 53 degrees.

MR. WALKER: 33 degrees, in other words, was the predicted temperature?

MR. MULLOY: I believe, yes, sir.

DR. COVERT: What's the difference between 25 and 15?

MR. MULLOY: That should be 25 degrees? That's an error? Well, this is the chart that I worked from.

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MR. KILMINSTER: That is the way it read, but it should have said 25.

DR. COVERT: What's the difference between 25 and 15? Is that just a serial, or sequence?

MR. MULLOY: Yes, these are the SRM numbers. It's ten SRM builds later. It happens to be the difference between January and January.

DR. COVERT: Okay, fine. Thank you. Press on.

DR. MULLOY: "Temperature data is not conclusive on predicting primary O-ring blow-by. The engineering assessment is that colder O-rings will have increased effective durometer. They will be harder. The harder O-rings will take longer to seat, that more gas may pass the primary O-ring before the primary seal seats as it translates from its upstream position to its downstream position, that the demonstrated sealing threshold, however, is three times greater in terms of erosion than we've experienced on SRM-15."

And I will let Thiokol engineering explain more about the analysis that goes into that.

"If the primary seal does not seat, the secondary seal will seat. Pressure will get to the secondary seal before the metal parts rotate. The O-ring pressure leak check places the secondary seal in outboard position, which minimizes sealing time, and MTI recommends 51-L launch proceed on 28 January 1986 and

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that SRM-25"—which are the motors that were on 51-L—"will not be significantly different from SRM-15."

And with that, sir, I will turn this over to Thiokol, who will discuss their deliberations during this period of time. Mr. Jerry Mason is the Vice President and General Manager of the Wasatch Division of Thiokol.

I might mention, Mr. Chairman, we also have from Thiokol here today Mr. Ed Garrison, who is the President of the Aerospace Group in Chicago, Mr. Mason's supervisor.

CHAIRMAN ROGERS: Mr. Mason, might I suggest in your discussion with us today that, please disclose anything that you know about that may turn up. If you have documents that we don't know about that would be embarrassing to you, tell us about them now.

We don't want to have to pry information out of you. You know what's there. Tell us the whole story, if you will.

JANUARY 27, 1986

TIME (EST) | EVENT

~1:00 P.M — OECISION TO SCRUB. POLL OF SRB LAUNCH SUPPORT PERSONNEL REGAROING CONSTRAINTS TO 24 HOUR RECYCLE.

- MTI PERSONNEL AT KSC ANO HUNTSVILLE OIO NOT IOENTIFY ANY CONCERNS.
- USBI-BPC IOENTIFIEO CONCERNS REGARDING RECOVERY BATTERIES ANO FUEL SERVICE MODULE REDLINES.

~2:00 P.M. — MISSION MANAGEMENT TEAM MEETING.

- REPORTEO NO SRB CONSTRAINTS TO 24 HOUR RECYCLE ANO STATEO THAT CONCERNS FOR LOW TEMPERATURE LCC VIOLATIONS HAD BEEN ASSESSEO WITH CONCLUSION THAT THERE WERE NO SRB CONSTRAINTS TO A 9:38 A.M. LAUNCH ON 1/28/86.

~5:15 P.M. — MTI (AL MCDONALD) CALLEO MSFC RESIOENT OFFICE AT KSC (CECIL HOUSTON) TO INFORM HIM MTI ENGINEERING HAD CONCERNS REGAROING O-RING TEMPERATURES.

[Ref. 2/14-1 1 of 4]

~5:25 P.M. — CECIL HOUSTON CALLEO JUO LOVINGOOO TO ESTABLISH A TELECON WITH REINARTZ, MULLOY, HAROY ANO MTI WASATCH OIVISION.

~5:45 P.M. — TELECON BETWEEN LOVINGOOO, REINARTZ, MTI ANO OTHERS RESULTEO IN SETTING UP TELECON FOR 8:15 P.M. EST TO INCLUDE AOOITIONAL TECHNICAL PERSONNEL AND OATA.

~7:00 P.M. — REINARTZ ANO MULLOY VISITEO BILL LUCAS ANO JIM KINGSBURY IN THEIR MOTEL ROOM TO INFORM THEM OF CONCERN AND PLANNEO TELECON.

~8:45 P.M. — CHARTS CONTAINING MTI ENGINEERING RECOMMENOATIONS RAPIOFAXED FROM WASATCH TO HUNTSVILLE ANO KSC. TELECON BETWEEN MTI WASATCH, MSFC HUNTSVILLE, AND MSFC/MTI PERSONNEL AT KSC STARTEO.

MTI ENGINEERING (BOB LUND) RECOMMENDEO 51-L NOT BE LAUNCHEO IF O-RING TEMPERATURES PREOICTEO AT LAUNCH TIME WOULO BE LOWER THAN ANY PREVIOUS LAUNCH (LESS THAN OR EQUAL TO 53 OEGREES F.). CONCERN WAS FOR POTENTIAL OF INCREASED PRIMARY O-RING BLOWBY BECAUSE OF REOUCEO RESILIENCY.

JOE KILMINSTER, VP, SPACE BOOSTER PROGRAMS AT MTI, STATEO THAT BASEO ON ENGINEERING RECOMMENOATION, HE COULO NOT RECOMMENO LAUNCH.

[Ref. 2/14-1 2 of 4]

MULLOY RESTATED RATIONALE FOR FLYING TO DATE IN LIGHT OF PREVIOUSLY OBSERVED PRIMARY O-RING EROSION AND BLOWBY (INCLUDING RECOGNITION IN 1983 CIL WAIVER THAT UNDER CERTAIN CONDITIONS ONLY ONE O-RING MAY BE EFFECTIVE) AND STATED THAT DATA PRESENTED BY MTI ENGINEERING DID NOT SEEM TO CHANGE THAT BASIC RATIONALE.

STAN REINARTZ ASKED GEORGE HARDY TO COMMENT ON MULLOY'S RATIONALE. GEORGE STATED THAT HE BASICALLY AGREED BUT WOULD NOT RECOMMEND FLYING IF MTI DID NOT.

JOE KILMINSTER REQUESTED FIVE MINUTES OFF-NET CAUCUS.

AL MCDONALD ASKED JOE KILMINSTER TO BE SURE TO CONSIDER IN THEIR CAUCUS THAT THE CONCERN CREATED BY THE COLD TEMPERATURE WAS PRIMARY O-RING BLOWBY. HE STATED THAT IF THIS OCCURRED, IT WOULD OCCUR AT IGNITION -- BEFORE THE JOINT GAP BEGAN TO OPEN -- AND THAT THE SECONDARY O-RING WOULD THEN BE PRESSURE ACTUATED AND WOULD SEAL.

CAUCUS LASTED APPROXIMATELY THIRTY MINUTES. DURING CAUCUS, KSC AND HUNTSVILLE WERE ON MUTE AND NO DISCUSSIONS OCCURRED BETWEEN THOSE TWO SITES.

~11:00 P.M. JOE KILMINSTER CAME BACK ON NET AND READ RATIONALE FOR RECOMMENDING LAUNCH.

~11:35 P.M. MTI RECOMMENDATION TO FLY 51-L RAPIDFAXED TO MULLOY AT KSC.

[Ref. 2/14-1 3 of 4]

JANUARY 28, 1986

~5:00 A.M. DISCUSSED MTI CONCERNS, TELECON RESULTS AND FINAL RESOLUTION WITH DR. LUCAS, MSFC DIRECTOR, AND JIM KINGSBURY, MSFC DIRECTOR OF ENGINEERING.

[Ref. 2/14-1 4 of 4]

SRM ASSESSMENT
COLD O-RINGS

Larry Mulloy

- BLOW-BY OF O-RINGS CANNOT BE CORRELATED TO TEMP. (SRM 22)
- SOOT BLOW-BY PRIMARY O-RINGS HAS OCCURRED ON SEVERAL OCCASSIONS
- SOMETIMES, PRIMARY EROSION OCCURS DUE TO CONCENTRATED HOT GAS PATH THRU PUTTY.
- MAX ALLOWABLE EROSION & STILL SEAT DEMONSTRATED BY TEST IS 0.125" (~~3000 PSI~~)
- NO SECONDARY O-RING EROSION OR BLOW-BY TO DATE. IN FIELD JOINTS
- COLDER TEMP MAY RESULT IN GREATER PRIMARY O-RING EROSION AND SOME HEAT AFFECTED SECONDARY BECAUSE OF INCREASED HARDNESS OF O-RING RESULTING IN SLOWER SEATING
- EARLY STATIC TEST (HYDROTEST) WITH 90 DUROMETER SHOWED SEATING (0.275" ϕ)
- SQUEEZE AT 20°F IS POSITIVE (>.020")
- SECONDARY SEAL IS IN POSITION TO SEAT (200 PSI / 50 PSI LEAK CHECK)
- PRIMARY MAY ~~[illegible]~~ NOT SEAT DUE TO REDUCED RESILIANCY — HOWEVER, DURING PERIOD OF FLOW PAST PRIMARY — SECONDARY WILL BE SEATED

CONCLUSION

RISK RECOGNIZED THRU-OUT PROGRAM IS APPLICABLE TO SRM (83 C/L)

PC 071544

MR. CHAIRMAN, MEMBERS OF THE COMMISSION, I HAVE PREVIOUSLY TESTIFIED TO THE STS-51L FLIGHT READINESS REVIEW PROCESS LEADING UP TO THE LAUNCH MINUS ONE DAY REVIEW AT KENNEDY SPACE CENTER ON JANUARY 26, 1986. I HAVE STATED HOW THE CONTINUING CONCERN FOR JOINT O-RING EROSION HAD BEEN TREATED IN THE FLIGHT READINESS REVIEW PROCESS. MY STATEMENT TODAY WILL BE LIMITED TO MY KNOWLEDGE OF THE SPECIFIC EVENTS OF JANUARY 27, 1986 LEADING TO THE DECISION TO LAUNCH STS-51L ON JANUARY 28, 1986.

AT APPROXIMATELY 1:00 P.M. ON JANUARY 27, THE DECISION WAS MADE TO SCRUB THE LAUNCH FOR THAT DAY. ALL PROJECT ELEMENTS WERE ASKED TO ASSESS THE ABILITY TO BE PREPARED TO LAUNCH AT 9:38 A.M. EASTERN STANDARD TIME ON JANUARY 28. MR. STAN REINARTZ, SHUTTLE PROJECTS MANAGER FOR MSFC, ASKED OVER THE LAUNCH COMMUNICATIONS NET FOR INPUT FROM THE MSFC PROJECT ELEMENTS MANNING THE BACKUP CONSOLES IN FIRING ROOM 2 AT KSC AND AT THE HUNTSVILLE OPERATIONS SUPPORT CENTER AT MSFC. PRIME CONTRACTOR SUPPORT PERSONNEL, INCLUDING MR. ALLEN MCDONALD, WHO WAS THE SENIOR THIOKOL REPRESENTIVE AT KSC TO SUPPORT THE LAUNCH AND MR. BOYD BRINTON, THE SENIOR THIOKOL REPRESENTATIVE AT MSFC, HAVE ACCESS TO THIS COMMUNICATION NET. (IT HAS BEEN REPORTED TO ME THAT MR. BRINTON WAS ASKED BY MR. LARRY WEAR, MY SRM ELEMENT MANAGER, IF THIOKOL HAD ANY CONCERNS FOR A LAUNCH ON JANUARY 28. MR. BRINTON CALLED THIOKOL ENGINEERING TO HAVE THEM ASSESS THAT QUESTION).

AT APPROXIMATELY 1:30 P.M., THE MSFC ELEMENTS REPORTED TO MR. REINARTZ THAT THERE WERE NO CONSTRAINTS. THE SRB SUPPORT PERSONNEL AT THE HUNTSVILLE OPERATIONS SUPPORT CENTER DID STATE THAT THEY WERE STILL LOOKING AT THE RECOVERY BATTERY AND FUEL SERVICE MODULE LAUNCH COMMIT CRITERIA TEMPERATURE REDLINES BUT SAID THEY DID NOT THINK AT THAT TIME THAT IT WOULD BE A PROBLEM. I TOLD THEM TO LET ME KNOW IF THAT ASSESSMENT CHANGED.

AT APPROXIMATELY 2:00 P.M. I REPORTED TO THE MISSION MANAGEMENT TEAM THAT THERE WERE NO SRB CONSTRAINTS TO A LAUNCH AT 9:38 A.M. ON JANUARY 28. I STATED THAT WE WERE CONTINUING TO ASSESS THE RECOVERY BATTERY AND FUEL SERVICE MODULE TEMPERATURES BUT THAT OUR ASSESSMENT AT THAT TIME WAS THAT THEY WOULD NOT BE A CONSTRAINT.

AT APPROXIMATELY 2:30 P.M. I DEPARTED KSC FOR THE MOTEL. [Ref. 2/14-2 2 of 7]

IT HAS BEEN REPORTED TO ME THAT AT APPROXIMATELY 5:15 P.M. MR. MCDONALD, AFTER BEING IN COMMUNICATION WITH ENGINEERS AT THIOKOL IN UTAH, ATTEMPTED TO CONTACT ME AT MY MOTEL ROOM AND, BEING UNABLE TO DO SO, CALLED MR. CECIL HOUSTON, THE MSFC RESIDENT MANAGER AT KSC. HE INFORMED MR. HOUSTON THAT THIOKOL ENGINEERS WERE CONCERNED ABOUT THE EFFECT OF THE COLD TEMPERATURES THAT WERE PREDICTED FOR THE NIGHT AND AT LAUNCH TIME. MR. HOUSTON THEN ATTEMPTED TO CONTACT ME AND MR. REINARTZ BUT WAS UNABLE TO DO SO. HE THEN CONTACTED DR. JUD LOVINGOOD, MR. REINARTZ'S DEPUTY AND INFORMED HIM OF WHAT MR. MCDONALD HAD TOLD HIM. HE SUGGESTED THAT A TELECONFERENCE BE SET UP TO DISCUSS THE CONCERNS. THAT TELECONFERENCE DID OCCUR AT APPROXIMATELY 5:45 P.M. IT DID NOT, HOWEVER, INCLUDE ME AND OTHER PARTIES NECESSARY TO DISPOSITION THE STATED CONCERNS NOR WAS IT POSSIBLE TO UNDERSTAND THE CONCERNS BECAUSE OF THE LACK OF WRITTEN DATA AND POOR COMMUNICATION WITH PERSONNEL WHO WERE AT THEIR HOMES. IT WAS THEREFORE DECIDED TO HAVE THIOKOL TRANSMIT THE DATA ON WHICH THEIR CONCERNS WERE BASED TO KSC AND MSFC AND ESTABLISH A TELECONFERENCE FOR 8:15 P.M. WHERE ALL NECESSARY PERSONNEL COULD BE ASSEMBLED IN TELECONFERENCE FACILITIES AND REVIEW THE DATA.

MR. REINARTZ NOTIFIED ME AT APPOROXIMATELY 7:00 P.M. OF THE THIOKOL CONCERNS AND OF THE TELECONFERENCE PLANNED FOR 8:15 P.M. MR. REINARTZ AND I THEN VISITED DR. BILL LUCAS, MSFC CENTER DIRECTOR, AND MR. JIM KINGSBURY, MSFC DIRECTOR OF SCIENCE AND ENGINEERING, IN DR. LUCAS' ROOM AND INFORMED THEM OF THE CONCERN AND THE PLANNED TELECONFERENCE. MR. REINARTZ AND I THEN PROCEEDED TO OUR RESIDENT OFFICE AT KSC WHERE WE WERE JOINED BY MR. HOUSTON, MR. MCDONALD, AND MR. JACK BUCHANAN, THIOKOL LAUNCH SUPPORT SERVICES MANAGER AT KSC.

THE CHARTS CONTAINING THE THIOKOL ENGINEERING DATA AND THE CONCLUSION THAT THIOKOL ENGINEERING WAS DRAWING FROM THOSE DATA ARRIVED AT KSC AND MSFC AT APPROXIMATELY 8:45 P.M. AND THE CONFERENCE BEGAN. A LIST OF PARTICIPANTS AT EACH SITE HAVE BEEN PREVIOUSLY SUBMITTED AND IS INCLUDED IN THE PACKAGE PROVIDED TO YOU TODAY.

THE DATA PRESENTED ADDRESSED THE LOW AMBIENT TEMPERATURES PREDICTED FOR THE NIGHT AND AT LAUNCH TIME, THE RESULTING TEMPERATURE OF THE SRM FIELD JOINTS, AND THE EFFECT OF THOSE LOW TEMPERATURES ON THE O-RINGS IN THE JOINTS. THE THIOKOL ENGINEERS STATED THAT THEY BELIEVED THE EFFECT OF THE TEMPERATURE ON THE O-RINGS WOULD BE TO SLOW THE TIME FOR THE PRIMARY O-RING TO SEAL, RESULTING IN GREATER HOT GAS PAST THE PRIMARY SEAL AND POSSIBLE EROSION OF THE SECONDARY SEAL. [Ref. 2/14-2 3 of 7]

THE DATA SHOWED THAT THE PREVIOUS COLDEST LAUNCH, STS 51-C, HAD, AT LEAST QUALITATIVELY, THE WORST BLOWBY OF ANY OBSERVED. CONSIDERABLE DISCUSSION BETWEEN MSFC AND THIOKOL ON THE SIGNIFICANCE OF THE DATA ENSUED. THE MAJOR FOCUS OF THE DISCUSSION WAS THE EFFECT THE LOW TEMPERATURES COULD HAVE ON THE DURATION OF BLOWBY OF THE PRIMARY SEAL. ALL PARTICIPANTS WERE IN GENERAL AGREEMENT THAT EFFECT WOULD BE TO INCREASE THE DURATION OF BLOWBY. THE QUESTION WAS, WOULD THE SECONDARY O-RING, WHICH WAS IN A POSITION TO SEAL, BE ENERGIZED BY THE BLOWBY PRESSURE AND FORM A SEAL BEFORE SIGNIFICANT JOINT ROTATION OCCURRED AND REDUCED RESILIENCY BECOME A MORE SIGNIFICANT FACTOR. AT THE CONCLUSION OF THE DISCUSSION, MR. BOB LUND, VICE PRESIDENT OF THIOKOL ENGINEERING, SUMMARIZED THE RECOMMENDATION THAT HAD BEEN PREPARED BEFORE THE DISCUSSION BEGAN AND CONCLUDED THAT THE SHUTTLE SRM NOT BE LAUNCHED WHEN THE JOINT TEMPERATURE WAS LESS THAN 53° F. THE LOGIC FOR THIS RECOMMENDATION WAS THAT THE TEMPERATURE OF THE JOINT ON STS 51-C WAS 53° F. AND FUNCTIONED SATISFACTORILY.

I ASKED MR. KILMINSTER, VICE PRESIDENT, SPACE BOOSTER PROJECTS, FOR HIS RECOMMENDATION FOR 51-L. HE STATED THAT BASED ON THE ENGINEERING RECOMMENDATION HE COULD NOT RECOMMEND LAUNCH. I THEN STATED THAT MY ASSESSMENT OF THE DATA PRESENTED, WHEN CONSIDERED IN CONTEXT WITH ALL ENGINEERING DATA PREVIOUSLY DEVELOPED FROM TEST AND PREVIOUS OBSERVATIONS OF PRIMARY O-RING BLOWBY AND EROSION ON FLIGHT MOTORS, DID NOT SEEM TO SUPPORT A CHANGE TO PREVIOUSLY DEVELOPED ENGINEERING RATIONALE FOR FLIGHT READINESS. MY ASSESSMENT WAS:

[Ref. 2/14-2 4 of 7]

COLD O-RING ASSESSMENT

- O BLOW-BY OF O-RINGS CANNOT BE CORRELATED TO TEMPERATURE STS 61-A HAD BLOWBY AT 75°F.
- O SOOT BLOW-BY PRIMARY O-RINGS HAS OCCURRED ON MORE THAN ONE OCCASSION, INDEPENDENT OF TEMPERATURE.
- O PRIMARY EROSION OCCURS DUE TO CONCENTRATED HOT GAS PATH THRU PUTTY
- O MAX ALLOWABLE EROSION AND STILL SEAT DEMONSTRATED BY TEST IS 0.125"
- O NO SECONDARY O-RING EROSION OR BLOW-BY TO DATE IN FIELD JOINTS
- O COLDER TEMP MAY RESULT IN GREATER PRIMARY O-RING EROSION AND SOME HEAT EFFECTED SECONDARY BECAUSE OF INCREASED HARDNESS OF O-RING RESULTING IN SLOW SEATING
- O EARLY STATIC TESTS (HYDROTESTS) WITH 90 DUROMETER SHOWED SEATING (0.275" O-RING DIAMETER)
- O SQUEEZE AT 20°F IS POSITIVE (>0.020")
- O SECONDARY SEAL IS IN POSITION TO SEAT (200 PSI/50 PSI LEAK CHECK)
- O PRIMARY MAY NOT SEAT DUE TO REDUCED RESILIENCY - HOWEVER, DURING PERIOD OF FLOW PAST PRIMARY - SECONDARY WILL BE SEATED AND SEAL BEFORE SIGNIFICANT JOINT ROTATION OCCURS.

<u>CONCLUSION</u>

RISK RECOGNIZED AT ALL LEVELS OF NASA MANAGEMENT IS APPLICABLE TO STS 51-L

[Ref. 2/14-2 5 of 7]

MR. REINARTZ THEN ASKED FOR MR. HARDY TO COMMENT ON MY ASSESSMENT. MR. HARDY STATED THAT HE BASICALLY AGREED WITH MY SUMMARY BUT THAT HE WOULD MOST CERTAINLY NOT RECOMMEND LAUNCH AGAINST THIOKOL'S RECOMMENDATION. MR. KILMINSTER THEN REQUESTED AN OFF-NET CAUCUS OF THE THIOKOL PERSONNEL IN UTAH. MR. MCDONALD THEN MADE THE FIRST COMMENT THAT I CAN RECALL DURING THE ENTIRE TELECONFERENCE. HE STATED TO JOE KILMINSTER THAT DURING THEIR CAUCUS THAT THEY SHOULD CONSIDER THE COMMENT MADE BY MR. HARDY DURING THE COURSE OF THE DISCUSSIONS THAT THE CONCERNS EXPRESSED WERE FOR PRIMARY O-RING BLOWBY AND THAT THE SECONDARY O-RING WAS IN A POSITION TO SEAL DURING THE TIME OF BLOWBY AND WOULD DO SO BEFORE SIGNIFICANT JOINT ROTATION HAD OCCURRED. MR. MCDONALD STATED THAT HE THOUGHT THAT WAS AN IMPORTANT CONSIDERATION. ALL SITES THEN PUT THE CONFERENCE ON MUTE AND NO FURTHER DISCUSSION OCCURRED BETWEEN THE SITES UNTIL APPROXIMATELY THIRTY MINUTES LATER WHEN THE THIOKOL CAUCUS IN UTAH WAS COMPLETED.

AT THE COMPLETION OF THE CAUCUS, MR. KILMINSTER STATED THAT THEY HAD ASSESSED ALL THE DATA AND CONSIDERED THE DISCUSSIONS THEREON AND PRESENTED THE THIOKOL RATIONALE FOR RECOMMENDING LAUNCH OF STS 51-L. MR. REINARTZ ASKED IF THERE WERE ANY FURTHER COMMENTS AND TO MY RECOLLECTION THERE WERE NONE. I THEN ASKED THAT MR. KILMINSTER SEND ME A COPY OF HIS FLIGHT READINESS RATIONALE AND RECOMMENDATION. THE CONFERENCE WAS THEN TERMINATED AT APPROXIMATELY 11:15 P.M.

AFTER THE TELECONFERENCE WAS COMPLETED, MR.MCDONALD INFORMED MR. REINARTZ AND ME THAT IF THE MTI ENGINEERING CONCERN FOR THE EFFECT OF COLD WAS NOT SUFFICIENT CAUSE TO RECOMMEND NOT LAUNCHING, THERE WERE TWO OTHER CONSIDERATIONS - LAUNCH PAD ICE AND RECOVERY AREA WEATHER. I STATED THAT LAUNCH PAD ICE HAD BEEN CONSIDERED BY THE MISSION MANAGEMENT TEAM BEFORE DECIDING TO PROCEED AND THAT A PERIODIC MONITORING OF THAT CONDITION WAS PLANNED. I FURTHER STATED THAT I HAD BEEN MADE AWARE OF THE RECOVERY AREA WEATHER AND PLANNED TO PLACE A CALL TO MR. ALDRICH AND ADVISE HIM WEATHER IN THE RECOVERY AREA EXCEEDED LAUNCH COMMIT CRITERIA. AT APPROXIMATELY 11:30 P.M. MR. HOUSTON ESTABLISHED A TELECONFERENCE WITH MR. ALDRICH AND MR. SESTILE OF KSC. I INFORMED MR. ALDRICH THAT THE WEATHER IN THE RECOVERY AREA WOULD PRECLUDE IMMEDIATE RECOVERY OF THE SRBS AND THAT THERE WAS A POSSIBILITY OF LOSS OF MAIN PARACHUTES, FRUTUMS AND DROGUE PARACHUTES. MR. ALDRICH DECIDED TO PROCEED WITH LAUNCH.

MR. REINARTZ AND I THEN RETURNED TO OUR MOTEL.

[Ref. 2-14-2 6 of 7]

AT APPROXIMATELY 5:00 A.M. ON JANUARY 28, I INFORMED DR. LUCAS AND MR. KINGSBURY OF THE DATA THAT LED TO THE THIOKOL ENGINEERING CONCERNS AND THE FINAL RESOLUTION THEREOF. IT DID NOT OCCUR TO ME TO INFORM ANYONE ELSE THEN NOR DO I CONSIDER THAT IT WAS REQUIRED TO DO SO TODAY.

IN CONCLUSION, MY DECISION TO PROCEED WITH THE LAUNCH AS RECOMMENDED BY THE THIOKOL OFFICIAL RESPONSIBLE FOR MAKING SUCH RECOMMENDATIONS WAS BASED SOLELY ON THE ENGINEERING DATA PRESENTED BY THIOKOL ENGINEERING AND THE MSFC ENGINEERING EVALUATION OF THOSE DATA. I AM CERTAIN THERE WERE NO OTHER FACTORS SUCH AS SCHEDULE PRESSURES INVOLVED IN MY DECISION.

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TESTIMONY OF JERRY MASON, SENIOR VICE PRESIDENT, WASATCH OPERATIONS, MORTON THIOKOL

MR. MASON: We will do that. First off, what we had in mind was that I would give you the kind of overview that Larry has, except to give you more specifics of what occurred at the Wasatch Division in Utah. And then after that, our Vice President of Engineering will go through the specific technical charts that were reviewed that evening, and then Mr. Kilminster is prepared to provide the additional rationale that led us to the final conclusions.

But one thing I would like to explain. Larry's introduction was a little bit in error. I am actually the senior vice president of the Wasatch operations, and I need to explain that. We have three divisions there, the space division, strategic, and the tactical division. And of course, the space division is dedicated to the shuttle program, and we have a general manager for each division.

Mr. Wiggins, who is with us today, is the general manager of the space division, and reporting to him is Mr. Kilminster, who is the program manager over manufacturing and quality operations. And then Mr. McDonald reports to Mr. Kilminster as the program

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manager on space and the solid rocket motor itself.

On the other side, also reporting to me, is the vice president of engineering, Mr. Lund. So I hope that clarifies the relationship a little bit.

The chronology is essentially as Larry had identified there.

MR. SUTTER: Can I ask one question now? Between Kilminster and Lund, who makes the "technical" or engineering decisions?

MR. MASON: Lund makes the technical recommendation to Mr. Kilminster.

MR. SUTTER: He can act on it as he sees fit?

MR. MASON: The final decision comes from the program manager to the general manager, and if necessary to me. The engineering, since it is a separate function, does provide a bit of an oversight on the program manager's functions. In other words, he doesn't report to him on a hard line; he reports to him in an advisory role, if you will.

The sequence was essentially as Larry had identified it. We were notified both from Marshall and Kennedy that the temperatures were going to be quite low the next morning for the launch if it were to occur at 9:38.

As a result of that, our engineering people

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looked at all of the aspects of the motor, particularly at the propellant mean bulk temperature, which is normally something we are concerned about, and that was not of any concern in this case, and identified that the only area of concern was the seal.

And they wanted to get together more data that existed in various places to take a further look at the seal, which they did. They started gathering that information, and the preliminary telecon occurred and arranged for the subsequent formal telecon.

In the formal telecon then, we reviewed the technical data, which Mr. Lund will cover with you, and the sequence was that we were gathering this data and attempting to reach our firm recommendation at the same time. So we were in a position that we got the charts together and

were trying to make the final chart on our recommendations, and at that point had not had any, I would say, complete discussions.

There were differing opinions within the group, and the only opinion that survived everyone was to launch at 53 degree O-ring temperature or higher.

CHAIRMAN ROGERS: What was the nature of the discussions? I mean, you say there were a lot of different views. What were they, some of them?

MR. MASON: Well, they centered around the

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effect of the cold on the ability of the O-rings to respond, and it was primarily the primary O-ring because, as has been mentioned, when you pressurize, when you run the leak test, you put the primary O-ring on the wrong side of the roof, whereas the secondary goes onto the side that you want it on.

So the question was, will the cold change the response time enough to keep that primary from sealing? And we did not have hard data that would give us that answer.

DR. RIDE: What was the lowest temperature that your data went down to?

MR. MASON: We had—the only meaningful, I guess the only conclusive data was the flight data, which was the 53 degrees. In other words, we had information on the hardness of the O-ring and various other things like that, which Bob will cover with you in the chart. But we did not have anything that said specifically how long does it take for the O-ring to move across.

Now, the reason, of course, that is the primary issue is that it needs to seal before you get a high enough pressure in the case to open that gap, or the so-called joint rotation. So it needed to seal in the 160 milliseconds.

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GENERAL KUTYNA: If I may, sir, this has been a problem since about 1980 off and on, and it was looked at as a criticality one failure mode for a few years and it has been continuing. It seems strange that you had to gather the data on the environments of this thing at this very moment; that if I had a problem like this in an airplane, I would have been working that environment data from the year 1980 or 1981, and not 1985 or 1986.

Why was the environment not considered and why hadn't you gone through an exhaustive review of the environmental effects on the failures prior to this time?

MR. MASON: Well, some of the data that we were gathering is data that was currently being created at that time as a result of the effort we were making to improve the reliability. For example, there is a blow-by test in there. That one had been run with argon and we were going to run it with freon, and we were getting the latest, very latest information we had, because we were giving consideration to that.

DR. RIDE: Let me ask Don's question in maybe a slightly different way. You had at some point to sign up for the SRB's working at 31 degrees ambient temperature, because that is the launch commit criteria on the entire system.

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What data did you use to certify the boosters to work at 31 degrees before you allowed that to be taken into account?

MR. MASON: Well, let's see. I think the way it is set up is, we signed up to work—to have it work with the propellant mean bulk temperature from the 40 to 90 degrees. I believe that is the performance requirement.

MR. WALKER: Why was there no separate requirement on the O-rings? That is the issue.

DR. RIDE: I mean, surely that requirement was imposed on you by NASA. NASA must have said: Your solid rocket booster has to work at 31 degrees; do the analysis of all the parts to determine that it does.

MR. MASON: Well, I have to say what my current understanding of the requirements is, which is that it has to perform, the propellant, at mean bulk temperature from 40 to 99, and that says it has to withstand temperatures of 31 to 99. And the attention has all been focused on getting the performance when the propellant mean bulk temperature was 40 to 99.

DR. WHEELON: A question. It is normal to establish specifications on a unit and then to complete a qualification program for a unit, prior to flight. Did you have a

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temperature specification on the O-ring or the joint assembly, either one? And can you describe what your qualification program of testing to verify that that specification was being met?

And if you personally don't know the answer to that question, I would like that question then to revolve to whoever in your organization picks it up. But I think we need a clean, crisp, clear answer to the question.

MR. MASON: Okay. I think it would be better that I not—I think I know the answer, but I have not enough confidence to try it.

DR. WHEELON: Who are you going to lateral that football to?

CHAIRMAN ROGERS: Who can answer that?

MR. MASON: Joe, can you answer that?

MR. KILMINSTER: I believe, as Jerry mentioned, the major focus of emphasis was on the 40 to 90 degree requirement for the mean bulk temperature. When it comes to the seal, we have a procurement spec for that material, a Mil R specification, that calls out that material being capable over the temperature range of minus 30 to 500 Fahrenheit. And it was on that basis that we qualified the use of that material as far as the seal is concerned.

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GENERAL KUTYNA: Capable of what?

MR. WALKER: Isn't that specification for a captured O-ring inside of a groove with a flat mating surface, not this kind of configuration?

MR. KILMINSTER: I don't believe the specification specifies what type of construction or what kind of design. It is a material capability.

MR. WALKER: Could we have that information? That information must be in Parker's specifications.

MR. KILMINSTER: It's in the Mil R specification.

DR. WHEELON: What did you do to assure your material was meeting that specification? What was the qualification program?

MR. KILMINSTER: Early in the program, it was determined that we would not have a program, a development program, and a test program that would qualify over that full temperature range.

DR. WHEELON: What range were you going to qualify over?

MR. KILMINSTER: Again, the emphasis was based on the solid propellant bulk temperature, and that qualification was done by analysis. We did not conduct a test.

DR. WHEELON: Let's set aside the question of

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bulk temperature on the propellant. The question is what specification did you have on the joint and the O-ring, and how did you test to verify that in fact you were meeting that specification, or did you not have a specification and not test?

MR. KILMINSTER: The specification we have is a Mil R specification.

DR. WHEELON: Which is a generic spec to cover a whole range of military equipment, right?

MR. KILMINSTER: In this case, it is this specific material.

DR. WHEELON: Okay. So it is minus 30 to plus 500, is that correct?

MR. KILMINSTER: That's correct.

DR. WHEELON: And how did you shade that requirement and how did you test to make sure that you had met that shaded requirement?

MR. KILMINSTER: We did not test specifically to identify that requirement or test against that requirement.

DR. WHEELON: Don't you find that a little surprising?

DR. KILMINSTER: There are many areas, as I mentioned, based upon the original intention that we would not conduct full-scale firings, full-scale tests, using

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a full range of temperatures.

DR. WHEELON: Did you use any subset of that full range of temperatures in your tests?

MR. KILMINSTER: Yes, we did.

DR. WHEELON: What range was that?

MR. KILMINSTER: That will be discussed when Bob discusses his charts. I believe we had a static firing as low as 47 Fahrenheit.

DR. WHEELON: Ambient?

MR. KILMINSTER: No, that was the predicted O-ring temperature, using ambient calculations lower than that.

CHAIRMAN ROGERS: Could I make a suggestion about procedure? Let's not worry about—if there is a question that is asked and somebody can answer it, have them answer it. I mean, you can still continue your presentation that you have organized, but you are all here now and it's reasonably informal.

So when Bud asks a question like that and somebody can answer it, just have them stand up and answer it.

DR. WHEELON: So you think went down to 47 degrees, in terms of a spec for ground testing of the seal?

MR. KILMINSTER: Yes.

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DR. WHEELON: But no lower than that?

MR. MASON: That was the seal temperature.

DR. WHEELON: I understand, not the ambient. But you qualified the seal at 47 degrees Fahrenheit?

MR. KILMINSTER: We verified it in a static test at 47 degrees.

DR. WHEELON: A static ground test?

MR. KILMINSTER: Yes, sir.

DR. RIDE: And you did no tests on the joint below 47 degrees?

MR. KILMINSTER: That is correct.

GENERAL KUTYNA: Bob Crippen, I thought somebody said yesterday, maybe it was you, that the shuttle was cleared to fly with a shuttle temperature of 31 degrees or thereabouts.

VICE CHAIRMAN ARMSTRONG: Arnie said that.

GENERAL KUTYNA: Now, how do you correlate the fact that the shuttle is cleared to fly at 31 degrees and yet you have only tested down to 47 degrees, and by analysis only to 40 degrees? How do you explain that?

MR. KILMINSTER: The only explanation I have is that we felt that we had a margin because of the material being capable down to minus 30 as identified in the specifications.

DR. WHEELON: Capable of what?

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MR. KILMINSTER: Capable of functioning.

MR. WALKER: I really think we need to understand exactly what that specification implies in terms of the use of O-rings in various kinds of configurations. We would really like to have that information as soon as possible on exactly what that specification implies.

DR. WHEELON: Or better yet, can you give us the specification from which you were working?

MR. BOISJOLY: The specification in question is Mil R 83248A.

DR. WHEELON: A further question. Did you do any further testing of the O-rings or the O-ring material on your own, independent of the motor?

MR. KILMINSTER: Not that I can recall.

DR. WHEELON: So you were just working to the specification of the material as provided in the Mil Spec?

MR. KILMINSTER: I'm talking in the original qualification program. We subsequently have done testing.

DR. WHEELON: How subsequently? The last couple of days?

MR. KILMINSTER: No. Over the last probably year, year and a half.

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DR. WHEELON: And what did those tests over the last year tell you?

MR. KILMINSTER: There is one test that is included in Bob's presentation that goes down to 30 degrees and identifies no blow-by at that point in a sub-scale joint configuration.

DR. WHEELON: Is this a scale test?

MR. KILMINSTER: It is a scale model.

MR. MASON: It's a full scale O-ring and full scale joint. It's just short, a small diameter.

DR. WHEELON: How many such tests did you run?

MR. KILMINSTER: I can't recall.

DR. WHEELON: Can you get us that data? I don't mean to tax your memory.

MR. KILMINSTER: Roger, do you recall the number of tests that were run at the cold temperatures?

MR. BOISJOLY: No, I don't.

MR. THOMPSON: Arnie Thompson here. There were two tests run at 30 degrees and two tests run at room temperature with the blow-by device, using argon as a tracer gas. The results were that we could see no blow-by with the apparatus.

We recognized that we needed to have a better sensitivity, so we went to freon 14, which shows better

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on the mass spectrometer. And those tests are proceeding and we have some data from that.

DR. WHEELON: In view of that apparently satisfactory validation, why were you concerned on the day before launch? It seems to me these tests should have set you at ease, and yet you were uneasy. Why?

MR. BOISJOLY: It should be emphasized that that test simulates the gap on a subscale basis, but does not simulate the dynamics of the joint and O-ring. It is not a joint. It is a gap, a physical fixed gap. And it was intended to measure if blow-by could occur past an O-ring at a very low temperature—not the temperature, but at a very low pressure, at the beginning of the ignition transient.

That test was specifically set up after the SRM-15 blow-by, which occurred a year ago last January, because at that presentation we made a point of saying that there is a small, miniscule portion of air that will go past any seal, any O-ring seal, in its attempt to do its initial sealing at low pressures. And then rapidly, as the pressure increases, the seal is functioning better and better at higher pressures, and that was the purpose of that test. That is the climate in which that test was developed.

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GENERAL KUTYNA: But yesterday we were shown that that joint does in fact compress. The gap moves, opens, closes.

MR. BOISJOLY: That's correct.

GENERAL KUTYNA: at the light of the SRB's, and possibly even before that, when the SSME's are lit and you have the twang effect.

And what you're saying is you did not take that into consideration during this test?

MR. BOISJOLY: On that particular test, that is correct.

GENERAL KUTYNA: It's a totally static joint with no compression?

MR. BOISJOLY: Yes.

DR. COVERT: I would like to ask a procedural question, please. Is that spec you gave us and that number, is that the one that was in force at the time the decision was made, or is that the one that is in force now? Or what's the deal?

MR. BOISJOLY: That is the mil spec that controls the flight-on? O-ring material.

DR. COVERT: What is the date of the spec?

MR. BOISJOLY: I don't think I have that information.

DR. COVERT: I think that would be helpful to know.

DR. WHEELON: Did these tests figure in your

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decision to proceed, or were they not a part of your thinking when you re-made the decision to proceed?

MR. BOISJOLY: To proceed with flying?

DR. WHEELON: With flying?

And I'm trying to say, there apparently was a caucus out in Utah at which you were asked to consider your decision not to go and in effect you wound up going. Were these test results a part of that reconsideration?

MR. BOISJOLY: Yes, sir.

DR. WHEELON: What role did they play?

MR. MASON: Well, sir, if I could, maybe I could pull that, the next steps together, and explain what things were involved in the reconsideration. We got to the point where we had faxed these copies of these charts, and at that time our best conclusion was to stay within our experience base.

We had not been able to determine whether it was rational to extrapolate, and so at that point we recommended the 53 degree or higher temperature, because that was our experience base.

DR. KEEL: Mr. Chairman, could we have it clarified for the record exactly what "that point" means in the time sequence?

MR. MASON: That point was at the time when we faxed the charts to start the formal group telecon.

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DR. KEEL: And in addition, can we have those charts? Do you have them with you today?

MR. MASON: Yes, sir. Mr. Lund is going to review those charts for you.

CHAIRMAN ROGERS: When you say "formal telecon" and "informal," how do you distinguish?

MR. MASON: Well, it really was whether you have the whole group on or there is an individual telecon. Earlier there had been a telecon that Larry mentioned with Mr. Lovingood, in which there was just talk between Marshall and Morton Thiokol. And then there had been the call from Kennedy to us saying it was going to be cold, and so I call that the informal telecon. It was just communicating generally.

And then it culminated in what I call the formal telecon, at which we had everyone on the net—Marshall, Kennedy, and ourself.

MR. LOVINGOOD: To clarify that, Kennedy was on, Jerry, during the 4:45 telecon, but we didn't have all the people in that 4:45 meeting that we had in the later meeting.

CHAIRMAN ROGERS: Let's discard the idea of "formal" and just call it telephone conversation, and who was talking and what was said.

MR. MASON: I think now I probably ought to

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address it as the final telecon in which we had everyone that Larry had listed to on the line. And it was interrupted by our caucus, and some people would call that two meetings or two telecons in one, but in my mind it was a continuing one, just interrupted by the caucus.

Now, we reviewed the charts in there and drew our conclusion that we ought to fly at 53 degrees or warmer. And at that point we received a number of comments on the net. The ones that I looked upon as being of consequence were that we were reminded that there really wasn't good correlation or valid correlation between temperature and blow-by, and that was pointed out because we had two cases of blow-by and one had been at 75 degrees and one had been at 50 degrees, and we had a lot of cases at varying temperatures where there had been no blow-by, and so we had very limited basis for saying that blow-by correlated with temperature. [Ref. 2/14-3]

MR. WALKER: Now you're talking about seal temperatures?

MR. MASON: We're talking about seal temperatures, right.

DR. RIDE: Where was the lowest temperature that you had no blow-by?

MR. MASON: Well, in the static test motors it was 47, I believe was the lowest, but

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it's on Bob's charts. We have cases where we had blow-by listed and we have—what we did was pick the coldest static test and the flight test. And of course, the static test didn't have any blow-by.

The static tests are slightly different and Bob will explain those. We tended to not put a lot of weight on those.

GENERAL KUTYNA: But the worst blow-by you had was at the lowest temperature, which was 51 C. did that not give you a strong correlation?

MR. MASON: That was the exact discussion that we had, was whether the fact that that one was somewhat worse than the one at 75 degrees, was that a correlation with temperature or was the fact that they blew by at both the 75 and 50 indicating that it was relatively independent of temperature.

DR. COVERT: Did you have any data on the stacking or the clearances on the 75 degree one as compared to the other?

MR. MASON: Yes, we did.

DR. COVERT: And were those clearances different significantly?

MR. MASON: They were relatively nominal, and we have that on the chart also.

MR. WAITE: Is last year's experience what led

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to this increased level of testing? In other words, the decision to go back and test?

MR. MASON: Well, you might say cranked up the gain in August when we had summarized everything and concluded that we needed to get a more aggressive effort on improving things.

MR. WAITE: So it was your experience on this one occasion that led to this need for more testing?

MR. MASON: I would say it was collective experience, sitting down and looking at it totally.

In any event, with the one comment about the lack of hard evidence that there was really a correlation, although instinctively we felt that the cold would make it somewhat worse. The other point that was made was that the concern about the primary having to move and it being colder, it might move slower. We said, don't overlook the fact that the secondary is in position and therefore it doesn't have to move, so the time element is not as of great a consequence on the secondary as it is on the primary.

So with those two comments and others, but those were the ones that we considered of consequence, we decided that we ought to have a caucus and assess whether it would be reasonable to extrapolate below our experience.

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DR. COVERT: How much more rigid is the grease at 30 degrees than it is at 50 degrees?

MR. MASON: I can't give you a number. The grease is somewhat stiffer, but it is just a film on the outside of the O-ring.

DR. COVERT: At least in principle it is.

MR. MASON: Yes. And we made that observation, that both the grease and the O-ring would be stiffer. Now, we had durometer readings on the O-rings and we had the general knowledge that the grease gets somewhat stiffer.

DR. FEYNMAN: Could I interrupt, because there's some physical thing that I don't understand quite clearly, and that is why the need to move the O-ring makes it so much less likely to seal than if it is in the right place.

MR. MASON: Because if it doesn't move fast enough and the joint opens up before it is seated, then it won't seat.

DR. FEYNMAN: Why?

MR. MASON: Because it doesn't have enough compression of the O-ring to hold the gas that forces down into the crack.

MR. WALKER: In other words, it has to be deformed initially before the gap opens up?

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MR. MASON: It has to initially so that when the gas hits it it will flow down into the crack and if you lose too much of that compression then it will flow by instead of pushing it in.

So that was our exact concern, that that very small distance that it had to move, if it didn't move that within the 160 milliseconds before the gap opened.

DR. FEYNMAN: That theory of how it works would account for the very much larger fraction of nozzle joints which failed compared to field joints, in which the nozzle joints have to move about seven times as far as a field joint, and that is consistent with your view.

MR. MASON: Exactly.

So we then had our caucus, in which we revisited all of the things we had talked about before. And we recognized two primary things that are covered in Mr. Kilminster's chart, and that was that the worst experience we had had in erosion was 38 thousandths, and we know from tests that the O-ring would seal with over 120 thousandths of erosion.

So our first thought was that if we had more erosion on the primary because it took longer, it would still seal, even if it were eroded three times as much. So we said we still had a reasonable expectation that

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the primary would seal, but we didn't have absolute data that said how long it would take to move.

So we then said, what happens if it doesn't? And we took the second point, which was that the secondary was in position and did not have to move. So we felt that the primary probably would seal, but if it didn't the secondary would because it was already in position.

DR. FEYNMAN: During this discussion, nobody noticed the possibility, I presume, that even though the secondary is in position, if the thing opens up and the resiliency of the material is zero because it is too cold, it won't close, it won't fill into the seal? Was that considered or not?

MR. MASON: That was considered, and the key issue became that what had to happen is either the primary or the secondary had to seal in the 160 milliseconds, before resiliency came into play, because in both cases if it seated before it opened up then it would extrude down into the joint.

But as you pointed out, if it started to open, then the resiliency question came into play.

GENERAL KUTYNA: But that's 160 milliseconds after lightoff of the solids. How about the resiliency effects in the seven seconds that the SSME's are

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compressing and bending this particular joint? Is there not an opening and closing of the gap during that time, that could have compressed the seals?

MR. MASON: Well, I can't answer that with certainty. I believe that those effects are relatively nominal on that joint, because the stiffener rings take out the bulk of that load on that joint.

DR. RIDE: Has that analysis been done?

MR. MASON: They have been looking at that effect again since the incident, and I can't tell you what the outcome is right now.

DR. RIDE: Where is that done? Is that done at Thiokol?

MR. MASON: Both. We're doing it both at Thiokol and at Marshall. We cross-check each other.

But I know there were some rough numbers done that said that that is quite stiff and it is not likely to be, but it needed to be done in great detail.

DR. RIDE: But it hadn't been done before 51-L?

MR. MASON: Yes, the analysis, the load analysis in the joint had been done. I can't tell you whether we had looked at whether it had any effect on the O-ring. I can't answer that question.

DR. FEYNMAN: I've got the timing problem

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mixed up with the 160 and 600 milliseconds and so on. Let me understand. Supposing the secondary seal is in place at some time and the primary seal has failed or something, and so the secondary seal is supposed to be pushed in by compression.

Now, it opens? Or is that not the way it works?

MR. MASON: Now it opens.

DR. FEYNMAN: How does it stay against the metal?

MR. MASON: Because the pressure is high enough. You see, it's now got 800, 900 psi, so now the pressure overrides the lack of resiliency.

DR. COVERT: Have you calculated how far this thing opens during this twanging motion because of the—it is a two-part system and it is a pin-connected thing, with a clamp there and the rest of it acts like it's free, in effect.

MR. MASON: We've calculated how fast it opens and how much due to pressurization, and I believe that the data will show that that is the primary driver, that that overrides any impact from the loads.

DR. COVERT: From the bending.

MR. MASON: But that I can't say has been verified yet.

DR. COVERT: But I agree with you that the pressurization deformation may be large compared to the

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bending deformation. But if we are at a marginal situation, it seems to me you can't throw away the small part, because that might be the thing that tips it across the edge.

Is that a reasonable way to look at it?

MR. MASON: I think we have to look at every small potential contributor, yes.

MR. HARDY: Mr. Chairman, I might make one comment to Sally's question earlier about calculating the effect of the transverse loads on the joint. We did in the early part of the program run static structural tests, and I talked about the dynamics of the joint, but we did run static structural tests where the test article with a flight representation of the joint under pressure, under a number of pressure cycles I think it was said yesterday. I don't know how many, but a large number of pressure cycles, where we did put in the transverse load and the maximum design transverse loads on the case, both at the aft joint and at the forward joint while we were pressure cycling.

GENERAL KUTYNA: But did you do it prior to pressure cycling? Because that's when it happens.

MR. HARDY: We did it in the precise sequence of the pressurization: the twang load, the pressurization, and then the flight dynamics.

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GENERAL KUTYNA: And how much opening or closing of that gap did you get?

MR. HARDY: Well, we measured the opening and closing. We measured it for the total effect, that is both the pressure effect and——

GENERAL KUTYNA: But now the pressure happens afterwards. How much did you get?

MR. HARDY: I can't recall that. I don't remember that. I don't remember the precise effect of the pre-ignition load on the joint.

GENERAL KUTYNA: But would that be a factor if we have in fact a seal that has lost its resiliency, that you would be concerned about how much that gap opened and closed and squished that seal prior to the pressurization?

MR. HARDY: Depending upon the effect and how much that load might open the gap, it could be a factor. And we are after that answer right now.

MR. SUTTER: On these tests, was it just on one set of O-rings, or did you change your gaps and the dimensions of the O-rings? Did you run a variation?

MR. HARDY: I can't remember how many sets of O-rings were involved. I do remember that repeated pressure cycles were done on the same O-ring, but I can't remember exactly how many sets of O-rings were

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involved.

CHAIRMAN ROGERS: You took part in all of these conversations yourself?

MR. MASON: I took part in the final conversation from the beginning, the whole telecon, from when we faxed the charts down until we drew the final conclusion. I was there.

CHAIRMAN ROGERS: Mr. McDonald testified, at least I thought he did, that the recommendation was not to launch, Thiokol's recommendation was not to launch. Who—at one point. Who was involved in that discussion leading up to the decision not to launch?

MR. MASON: All of us that were in the meeting, we were gathered and we were reviewing the data and attempting to reach our recommendation simultaneously.

CHAIRMAN ROGERS: Now, could you give us, without being too precise, the nature of the arguments against launch? Tell us what was being said. There must have been a consensus against launch if that is what you conveyed to NASA.

MR. MASON: Well, at that point we were trying to meet the deadline of getting the data together and getting the recommendation in, and so the engineering people were generating that and put together the

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charts.

CHAIRMAN ROGERS: What were they saying, though?

MR. MASON: They were saying, we're going to be outside of our data base to go colder than 53 degrees, and we're concerned about whether the O-ring will move fast enough to seat and seal before the joint opens up. And that was the thrust of the issue, is not knowing exactly how long it would take for the O-ring to move into position.

DR. COVERT: Jerry, if the O-ring has been eroded a little bit or ablated or charred or whatever the damn thing does, it is now oval rather than round, and the action—is this going to tend to rotate vertically? Is that the stable motion, or does it tend to, say, slide in, skinny in the vertical direction as opposed to rotating like this?

MR. MASON: I don't know. Perhaps. There is probably some theory there, and Roger is probably the best one to answer that. I'm not sure we know exactly.

MR. BOISJOLY: That was one of my major concerns, and I addressed that as a timing function to seal. And I believed and I still believe and I believed that night that there isn't anybody on the face of this earth that can tell you exactly the mechanism that happened in that joint.

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And even before the fact, you don't understand if it's going to rotate and walk up and delay or either slide because of its stiffness and delay. But the timing function that I spoke of that night had to do with the fact that I was afraid that that timing function could throw us in from an ignition transient at the start to somewhere after that start time, and that is what my major concern was about.

DR. COVERT: I think Roger and I are thinking along the same line. If it rolls in, then it is not a problem because you have the full exposure. But if it slides in and it is not skinny, it may in fact continue to blow by.

MR. BOISJOLY: But my concern even went beyond that point, because as it is performing this function at the beginning of the transient cycle, it still is being attacked by hot gas.

DR. COVERT: Precisely.

MR. BOISJOLY: And it is eroding at the same time it is trying to seal, and it is a race between, will it erode more than the time allowed to have it seal.

And that was my major concern, because SRM-15 showed erosion and hot gas blow-by at a low temperature, and that was the major issue on the table at that time.

DR. COVERT: Thank you very much.

CHAIRMAN ROGERS: Did you change your mind?

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MR. BOISJOLY: No, sir, never.

MR. SUTTER: Can I ask a question, Mr. Chairman?

In these conversations, this discussion about this simplex seal right there, was that discussed with you?

MR. MASON: Well, on the telecon Mr. Mulloy did go through his rationale and talked about the simplex seal.

MR. SUTTER: Did Thiokol agree with the fact that you could work with the simplex seal? Did that influence your decision? What if there was no statement that the criteria said the simplex seal is okay? Would you have still made the decision you did?

MR. MASON: I'm not sure I understand that question.

MR. SUTTER: Well, I guess my question is, were you designing the same criteria that NASA was designing, and who established the criteria, and what were your ground rules for design?

MR. MASON: Well, let's see. If we get back to that fundamental design criteria, I really can't answer that. I'm not sure whether Joe or Bob can.

I know what we were endeavoring to do was to be confident that it was safe to fly, and we weren't

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going back to that kind of a fundamental issue.

MR. WAITE: I have the same question in regards to the initial conversation about the single seal versus the two seals. You didn't really address the simplex seal until the latest conversation.

MR. MASON: Our thought has always been that it is essential to have a seal in that first 100 and—well, 160, 180, 200 milliseconds, before the joint opens up. If we get a seal at that point, then it doesn't really make any difference whether it's the primary or secondary. Once it is sealed, then it is reliable.

And so from a practical standpoint, we have been addressing getting the high confidence of getting that seal at ignition, and in that sense——

MR. SUTTER: But you're saying that when that seal—when one seals, then there's no assumption that any other failure might wipe out that seal?

MR. MASON: Well, everything we have known and everything demonstrated that once you had that seal, that there wasn't another cause that we could identify that would cause it to fail, because once it's sealed there is no more gas flow and then it is simply a benign environment.

DR. COVERT: But if it seals on the secondary flow before the joint enlarges or rotates, whatever you

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call it, and now it is a benign environment because there is no gas flow, and if the primary seal is not yet seated and now you get joint rotation and you feel the pressure, is what you said before, the pressure is sufficient to deform the secondary seal so that the primary will never be called into play again, is that right?

MR. MASON: That is correct. And there is—on that point, there is a unanimous position. There isn't any difference in that, that once it is pressurized it will handle the joint rotation. The issue is whether it gets pressurized rapidly enough.

VICE CHAIRMAN ARMSTRONG: I am unclear as to why you considered 53 degrees was the limit of your experience base, rather than the 47 that you had had in the static test.

MR. MASON: Okay, I will explain that. In the static test, we static test it in a horizontal position, and in the process of mating the segments the putty is exposed differently. It flows differently than it does in the vertical mode.

You don't have to touch it in the vertical mode, but in the horizontal mode we have to go inside the motor and tamp the putty to get it in the right position. So our feeling was that that probably made—

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well, it made a putty job that could be classed as better than the flight condition, and so it may have masked something.

And so we were hesitant to use it absolutely as evidence that it was okay.

MR. WALKER: Why didn't you ask for inspection of the putty seals in the flight, or didn't you think that was important, the vertical mating?

MR. MASON: Well, we had finally reached the conclusion that you can have—you can trap air in the mating operation. You can cause an air void when you leak test, when it blows back into the putty, and you can't be sure. You can go look and if it hasn't erupted through the putty you may have a path that is there but it hasn't exposed itself.

So even if you inspect it, you have the possibility of that path. So we felt that inspection wouldn't eliminate that possibility, and so we had to have a design approach that would tolerate that. That was our thought.

MR. WALKER: Can you respond to the question as to why the shuttle SRB design is different than the Titan design in terms of these seals? Were both of these designs done by your company?

MR. MASON: No, no. United Technologies.

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MR. WALKER: That is probably the answer.

MR. MASON: And I think maybe there are some difference in burning surface requirements. I'm not sure. But anyway, I know the Titan was considered at the time the joint design was done, but I think there were reasons.

But the point I would like to get to, however, is that we had the caucus and had these discussions, and it was clear that we were not going to reach a unanimous position there, and so we were then faced with the issue: Shall we stay with 53 degrees or is it reasonable to extrapolate?

And we had the reasons that I've already identified that made us feel that it was reasonable to extrapolate. So our final conclusion was reached by me having a review with the vice president of engineering, the program manager Lund, Kilminster, and the general manager Wiggins, and my own opinion.

We collectively agreed that it was reasonable to extrapolate, with the rationale that was listed on Kilminster's chart that said that we had a substantial margin, that we could erode more in the primary by a factor of three than what we had seen before; and that even if that didn't happen, that didn't seal, we had the secondary in a position where it did not have to move in

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order to seat, and therefore it would seal.

And that was the basis for our conclusion. And we have brought with us today the people that had objections. In fact, Roger, as you just noted, is one who says he didn't change his mind. But it was one of those where it becomes the responsibility of management to make we think a rational decision, and that is what we did.

MR. WALKER: How many people in the meeting were considered seal experts? That is, their primary interest or their knowledge would qualify them as particularly knowledgeable on seals?

MR. MASON: Well, to varying degrees I guess there's a half a dozen people there. Bob, you would be best to respond to that.

MR. LUND: It is a matter of degree. It's a judgment factor. Certainly Roger Boisjoly is a seal expert, and Arnie Thompson.

MR. WALKER: Did you seal experts agree, or is it kind of a confusing issue?

MR. LUND: There was much data, and I will present that data to you in a little bit, that is both ways. Some say it's okay, some say it's not. So each engineer that was there reached his own conclusion from the data that was presented, and so there is a diversity

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of opinion.

MR. MASON: Mr. Chairman, I did make one point while you were gone that was consistent with your earlier request, and that is that when we came down here we did review—and first, I made the point that we did not have a unanimous opinion, and because of the fact—we had a unanimous opinion among the key people reporting to me, but because of that we did identify and talk to the people who had concerns, and we brought the people that had concerns, so that if you wanted to you could talk to them, or if they wanted to they would have an opportunity to speak.

CHAIRMAN ROGERS: Very good. Thank you.

MR. MASON: Now, that is as far as I was intending to proceed. I thought Mr. Lund could go through the charts that we looked at that night.

CHAIRMAN ROGERS: Any other questions?

MR. WAITE: I have one more. I still haven't got the sequence of events in terms of this single versus dual seal concept. Did the dual seal discussion come up after you had been contacted or after your caucus, or before the caucus?

MR. MASON: Well, I think that it became a thought, a primary thought, after we had said that we ought to fly to 53 and we had that ensuing

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conversation. And it was pointed out at that point that the secondary seal was in position. And we had been focused so much on the primary seal that we said: Wait a minute, let's take a look at that, because we really hadn't——

MR. WAITE: Who pointed it out?

MR. MASON: Mr. McDonald. He said, don't overlook that. His comments were that the cold is in the direction of badness, but that—and that is the concern with the primary. The secondary, however, is in a better position. Don't overlook that.

And we said, well——

DR. COVERT: Could I ask you one more.

MR. MASON: Well, I guess we ought to have Al say just exactly what he said.

MR. McDONALD: I commented that lower temperature is in the direction of badness for both O-rings because it slows down the timing function, but that the effect was much worse for the primary O-ring compared to the secondary O-ring because the leak check port puts the primary O-ring on the wrong side of the groove, while the secondary O-ring is in the right direction, and that this condition should be evaluated in making the final decision for recommending the lowest acceptable launch temperature.

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Based on the data presented in chart 2-1, I considered this very important because, depending upon how much delay one has with getting a good, reliable primary seal affects the capability for the secondary O-ring to seal.

MR. CRIPPEN: Excuse me. And we made that in light of even though we had a CIL saying the secondary might not be there during separation?

MR. McDONALD: Well, the charts that were presented that night, one of the charts, and I had presented that chart earlier in August in a meeting at NASA headquarters, and it was presented that night and we looked at that and it showed the——

DR. KEEL: Do we have that chart, so all the Commissioners can see it?

MR. McDONALD: Yes, in Mr. Lund's presentation.

DR. KEEL: Can you pull it up now if you're going to talk about it, though? Can you put chart 3 up and let Mr. McDonald make his point.

(Viewgraph.) [Ref. 2/14-3]

MR. MASON: Do you want to do it right now?

DR. KEEL: Yes, it is up there.

MR. McDONALD: That was one of the charts, and what I was looking at was that we could conclude that this lower temperature doesn't affect the timing functions,

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which I said I really feel it does—so that we really don't change the time for the primary seal to really seal, what is being eroded, up to the 170 milliseconds. That is, we haven't changed this launch from any other.

However, if the colder temperature takes longer to not only move the seal, but also to extrude it in the gap because it's harder, then that might throw us into those longer times where, as you can see, it reduces the probability of the reliable secondary seal.

That was my concern, and if you go through it far enough it says that we had already stated that we have a high probability of no secondary seal because of the rotation problem, and I said that is a very important consideration.

MR. MASON: Let me address then the CIL question, because the basis for——

DR. COVERT: What's a "CIL"?

MR. MASON: Critical Items List.

The reason that that was changed from the 1-R to 1 was this very rationale here, which said that after early on, after the 170 or 330 milliseconds, you didn't have a redundant seal, and so it was changed because it was not redundant all the time.

But it didn't really remove the redundancy at ignition.

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CHAIRMAN ROGERS: Could I put the case and oversimplify it just a bit, because this is a good opportunity to be argumentative, if you will, on why you changed your mind. First, it was criticality item one, so that meant that if it was a failure, the mission was a catastrophe, and there had been discussion about that, a lot of it.

Then you made a decision, you and Thiokol made a decision, to recommend against launch for this very reason, that you were concerned. And then Mr. McDonald, in describing what happened, said that Thiokol had notified NASA that they should in effect not launch or conditions were not appropriate for launch, however you want to say it.

And then there was a phone call and he said, the conclusion being that Thiokol was directed to reassess all the data because the recommendation was not considered acceptable, in that—he was a little unclear about what he said, but anyway the recommendation was not considered acceptable.

And then you were supposed to have a five minute phone call caucus, which lasted 30 minutes, and you changed your mind.

Now that, you know, it is hard to explain it to an average outsider. You would think that that was

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one of the most critical things you can imagine in this program, and it had been so considered by NASA for a long time, and you obviously knew it.

What caused you to change your mind? Try to explain it. The impression is that you were directed to do it, that there was so much pressure to get this launch off that you were directed to do it, and you did it.

Now, if that is not the case, try to explain it in language that the public will understand: Why you changed your mind and how you did it so quickly?

MR. MASON: Well, I think I would start by saying that when we picked the 53 degree temperature it was clearly the most conservative approach, and it was done because we had only gotten to a position in time to get to the telecon discussion. We had not established it as an unequivocal position, but rather one that was conservative, that stayed within our experience base.

That was what we were thinking at that time.

When we had the telecon, then the other factors that I mentioned came out, and I would not characterize that NASA said, directed us to reassess. What NASA said was: Here is our feeling

and our rationale, and we wonder whether you feel strongly that that 53 degrees is as low as it is rational to fly.

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And I think we all recognized that it certainly was good at 50 degrees and 45, and so the question wasn't one of do you go below 53 degrees.

It was a question of how much below 53 can you go.

DR. RIDE: What data did you have to make that extrapolation? Did you have any data at all?

MR. MASON: Well, we had the effect of the temperature on the durometer of the O-ring. We had the one blow-by test that showed that it didn't blow by at 30 degrees. And other things that don't come to my mind that are in Mr. Lund's charts.

We had the higher margin of erosion. I mean, we had the ability to tolerate considerably more erosion than we had ever had and still seal.

But the reason for the debate was the fact that we didn't have hard evidence of how quickly that O-ring would move. And it became a matter of judgment rather than a matter of data, and that is the reason we couldn't reach a unanimous opinion.

CHAIRMAN ROGERS: What was the split? How many people were against it, even at the last moment?

MR. MASON: We didn't poll everyone.

CHAIRMAN ROGERS: Well, roughly.

MR. MASON: Well, I really couldn't guess. There

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were so many people—people were shuffling their positions within their own minds during the conversation.

CHAIRMAN ROGERS: But I mean, was there a substantial number or just one or two?

MR. MASON: There were I would say probably half a dozen, five or six in engineering, that at that point would have said it is not as conservative to go to that temperature and we don't know. The issue was we don't know for sure that it will work.

DR. RIDE: Roughly how many in engineering said they felt it was okay?

MR. MASON: Bob, do you want to guess?

MR. LUND: Just from an informal poll, I would guess there were five or six, the same way.

CHAIRMAN ROGERS: So it's about evenly divided among the engineers?

MR. LUND: That's a very estimated number.

MR. MASON: Remember, of course, it's a lot easier to say just stay conservative.

MR. CRIPPEN: Mr. Chairman, if I may make an observation. Since the earliest days of the manned space flight program that I've been associated with and Mr. Armstrong has been associated with, our basic philosophy is: Prove to me we're ready to fly. And somehow it seems in this particular instance we have

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switched around to: Prove to me we are not able to fly.

I think that was a serious mistake on NASA's part, if that was the case.

DR. WHEELON: May I make an observation while this chart is up, Mr. Chairman. You notice that it says from 330 milliseconds to 600 milliseconds there is a high probability of no secondary seal capability. I just point out that at 445 milliseconds was when the black smoke was starting to come out. I presume they are correlated. That's no big deal, but just before the chart gets away from us.

CHAIRMAN ROGERS: Just one more question. In the final telecon, did you or anybody from Thiokol let NASA know that the engineers were reasonably evenly split on whether to launch or not launch?

MR. MASON: No, we did not. We did—it was on the telecon when it was asked individually of the three gentlemen down there, Mr. Lund, Wiggins, and Kilminster, whether they agreed to fly. But there was no discussion about the overall vote or poll.

Now, it was clear, I believe, that it was not unanimous because of the lengthy discussion and the caucus and so forth. But we did not say in detail

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that.

CHAIRMAN ROGERS: In other words, I'm not speaking about a poll now, an exact count. But I wondered whether you told NASA, we've got a pretty good split here among our engineers and you should know that, and we've decided to recommend it, but we have got some engineers that are pretty well split.

I mean, did you say something like that?

MR. MASON: No, sir, I did not. But during the telecon I think most of the people know most of the people by voice, and the pros and cons as discussed by the telecon I think would have conveyed the general statement.

CHAIRMAN ROGERS: So those engineers that opposed it were on the telecon, too?

MR. MASON: Oh, yes.

CHAIRMAN ROGERS: And they expressed their opinion at that time?

MR. MASON: Yes.

DR. WHEELON: To NASA or to you?

MR. MASON: To the group on the telecon.

DR. WHEELON: So NASA heard their reservations on the telecon?

MR. MASON: Yes, sir, I believe so.

MR. SUTTER: Can I ask a question. Why wasn't Houston in on that phone call? Aren't they responsible for the design of the machine?

DR. COVERT: Marshall is.

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MR. REINARTZ: Mr. Chairman, I might answer that directly. The item that was being considered was an SRB item, a level three item, and that was treated as a level three item, as are many items by all of the elements, either orbiter, SRB, or ET. And we considered that, and depending upon the outcome of that telecon, an unfavorable recommendation by Thiokol to not launch or by the level three group, we would have then gone to JSC and said: Here is the situation as we see it and why we would not recommend launch at this time. In the conditions.

We treated it as a level three matter. It did not violate any of our—it did not require any waivers for our hardware and did not violate any launch commit criteria that we were made aware of by Thiokol. And on that basis, we did not bring in the level two organization into that discussion.

MR. SUTTER: Do you mean the decision to not have a redundant seal situation was not a waiver?

MR. REINARTZ: No, sir, it was not a decision to not have a redundant seal, as we indicated that the worst case condition that you could get into would be a single seal, which is the same situation as is now in the critical items list, that you could have a single

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seal, and that would be a worst case situation that you would get into.

We indicated during the discussion there may be some increased probability of erosion on the primary, but it was not concluded that you would have as a foregone conclusion——

MR. SUTTER: You don't understand my question. At one point in time, somebody laid down the design criteria that this would be a redundant seal. Somewhere along the line it got changed, and all I would like to know is who made that decision, and then would he go along with this decision.

MR. ALDRICH: Could I speak for Houston and for the level two organization, which is responsible for the integration of this total shuttle system? The critical items list is prepared and managed by the level two across all the projects, and they contribute to it, either with compliance through the two and three tolerance requirements that were described, I think yesterday or earlier last week.

And that process is formal, and the document that Sally has here in front of her is a document which in fact existed as 1-R early in the program for this case. It was changed in 1983, and it was reviewed and signed off at all levels of NASA, not only the level

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three project, but also in Houston by the level two project management and in Washington by the level one management, as the final concurrence in that.

So the situation of agreeing to accept this joint as a single seal during the timing period described here when only one of the seals came into play was reviewed and accepted for the program at all levels formally and understood.

This meeting that we're discussing here, however, where that was discussed as it pertains to the cold or the performance of the seal on the actual flight day of 51-L, is a meeting that was held only at level three, as Stan just discussed. And not only the synopsis of the points considered in that meeting, but the fact that the meeting was held, was not known to myself or to the level one organization, Jesse Moore, above it until subsequent to the event of the launch day.

MR. CRIPPEN: Sir, if I might point out, I guess maybe it's a matter of interpretation of the CIL. I don't think the CIL was written to say it's okay to fly with one seal. I thought the CIL was written to say that it is okay to accept that after the thing separates the secondary may not be sealed.

DR. KEEL: Mr. Chairman, I think that is just

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the point I was going to make. If you look, there is some ambiguity here or the danger of ambiguity. The criticality items list says specifically that the primary seal, the primary seal, not a single seal, is considered criticality one, which means that therefore the primary seal presumably has to be viewed as working and cannot fail.

DR. FEYNMAN: Could I ask a question? Could you tell me, sir, the names of your four best seal experts, in order of their ability?

MR. MASON: I would ask Mr. Lund to respond to that. Bob, did you hear the question?

MR. LUND: Yes. Roger Boisjoly I think is number one, with Arnie Thompson—I'm not sure who the best is. It's one and two.

DR. FEYNMAN: Approximately—what was that?

MR. LUND: Roger Boisjoly and Arnie Thompson, and Jack Kapp, K-a-p-p.

DR. FEYNMAN: And some other guy. And if you don't have any further, that'll be all right.

MR. LUND: Jerry Burns.

DR. FEYNMAN: Now, I would like to ask one further question. What is the opinion of Mr. Boisjoly about the seals, about the decision that was made? Were you in agreement with the result of this caucus that

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said that it was okay to fly?

MR. BOISJOLY: No, I was not.

DR. FEYNMAN: Now, Mr. Thompson, were you in agreement, and so forth?

MR. THOMPSON: I was not in agreement.

DR. FEYNMAN: Mr. Kapp?

MR. LUND: He is not here.

DR. WHEELON: Does anyone know what his position was?

MR. LUND: Yes. I talked to him and he said—after the meeting, because there had been so much discussion—I asked him, I said, how did you really feel, Jack, and he said: I would have made that decision, given the information we had that evening.

CHAIRMAN ROGERS: Would or would not?

MR. LUND: Would have made the same decision.

DR. FEYNMAN: So it may be that he would be in agreement?

MR. LUND: Yes, he was in agreement. That is as close as I can put it.

DR. FEYNMAN: And the fourth man's name?

MR. LUND: Jerry Burns. I don't know.

DR. FEYNMAN: So of the four, we have one don't know, one maybe yes or very likely yes, and two who were first mentioned without doubt as being the seal

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experts, they both said no. That is the information I wanted, that's all. Thank you.

MR. ACHESON: Could I ask a question. This Mil Spec A3248A that was referred to earlier is described as meeting the requirement that this material be capable, I think was the word, of between minus 30 and 500 degrees Fahrenheit. Capable of what wasn't clear to me.

Capable relating to the function as seals, or capable only in the sense that the raw material would not disintegrate or break up or something?

MR. KILMINSTER: I can't recall the specific working in the back of that specification.

MR. ACHESON: What I'm trying to find out is how much reliance was placed by MTI when it acquiesced to fly in this stack and that they were confident of the material in that respect.

MR. LUND: We didn't discuss that specification at all. We depended upon our data. I don't think we discussed the specification at all.

MR. WALKER: Is that a materials specification or an operations specification?

MR. LUND: It's a material specification for the Viton.

MR. WALKER: So it doesn't speak to the use,

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configuration, or operation?

MR. BOISJOLY: Just in the form of an O-ring, but it does not speak to the form of how that O-ring——

MR. WALKER: That wouldn't seem to be an appropriate specification for this particular question, would it? Wouldn't you need a functional specification to say that under a certain configuration the O-ring would operate? Wouldn't that be what you would want, rather than just a specification on the material?

DR. COVERT: Let the record show that Mr. Lund nodded yes.

MR. LUND: Yes.

CHAIRMAN ROGERS: Mr. Aldrich, going back to your statement, neither you nor Mr. Moore knew about this telecon or the discussions that took place on that?

MR. ALDRICH: Yes, sir. I can comment that it is my understanding that Mr. Moore did not know of it, because I did not know of it, and I have only found out about this meeting since the incident.

CHAIRMAN ROGERS: And were there others like yourself, that we would have expected would have known about it that didn't know about it? Do you understand my question?

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MR. ALDRICH: The two important people in terms of responsibility in the chain of command are Jess and myself. And I should comment that the following day we met early before the launch in the firing room, myself and Mr. Reinartz from Marshall and Mr. Moore and Mr. Lucas—Dr. Lucas from Marshall, all of us in the same area, physically adjacent as we are in this meeting.

And we discussed concerns and progress in the countdown continually during that time, and we also called for flight readiness—that is a specific goal of each project for myself and Jess—at minus 20 minutes in the countdown and at minus 9 minutes in the countdown.

And through that morning, because of the launch delay there was probably a five or six hour period when we were gathered there, with a break which I took to review the ice situation outside of the area. During that period of time, we did not discuss the concern with respect to the SRB and the temperatures, or particularly with respect to this joint or the seals.

CHAIRMAN ROGERS: And you did not know that there was a split among the engineers in Thiokol about whether the launch should occur?

MR. ALDRICH: I did not know that there was a

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concern about the solid rocket booster joints, nor that there had been discussions relating to the possibility of concern.

CHAIRMAN ROGERS: And you think that is true about Mr. Moore, too?

MR. ALDRICH: Yes, sir, I would be certain of that.

DR. RIDE: Do you think that is appropriate?

MR. ALDRICH: Let's see. I would be more pleased if we would have had a full discussion. As you saw earlier, we had a series of meetings and a series of requests from each element of our program to review their concerns and put them forward.

CHAIRMAN ROGERS: How is the final decision made? Is it made between you and Mr. Moore, or is there a committee?

MR. ALDRICH: We have a process of a lot of formal documentation, and you have had some extensive questions about wanting to understand it, and it is a complex system. But the formal process of documentation specifies the criteria to conduct a launch, such as the 31 degrees for the system launch failure is one criteria.

And the management is not in an in-line role of allowing the launch to proceed. We have a launch

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team in Houston with a flight director. We have a launch team here at Kennedy with an operations director, and they conduct the entire launch from prior to the countdown through the 43 hour countdown period and through the launch, based upon the rules, regulations, and data with respect to those rules that their people are looking at.

Management monitors that process and gives concurrence at the points we have talked about: the flight readiness review, maybe a week to two weeks before, the L minus one day, one day before, and at these commitment periods I talked about.

But that is a concurrence that we know of no reason not to proceed, and if we were not there to manage it the structure of the documentation in the team is such that they could go through all of the orderly engineering checks and procedures and process to allow the launch to occur.

MR. SUTTER: But aren't they given instructions that, yes, they are supposed to clear the vehicle for launch, but within the approved documentation?

MR. ALDRICH: Yes, sir.

MR. SUTTER: Well, wasn't this outside of the approved documentation?

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MR. ALDRICH: No, sir. There is no requirement in any level in our formal documentation that violates any of the criteria that had been reviewed over the years and documented and was in place.

VICE CHAIRMAN ARMSTRONG: This is one I was going to ask earlier, once we got past Thiokol. But we're in this discussion and I would like to bring it up now. It seems that if everything is falling within the mission rules, the launch rules, 31 degrees and so on, then for concerns that people have—and let's talk specifically, as an example, about that temperature, the bulk mean temperature or the seal temperature or other temperatures—your process depends on people at one or another of these commit meetings bringing their concerns to you, even though you are within the launch range rules, bringing their concerns in.

And then you make a decision at that point that, even though we are in the launch rules, because there are certain concerns, this type of thing, you depend upon these contacts, personal contacts, is that right, coming in to tell you about things that would prevent you from going?

MR. ALDRICH: Yes. In fact, at our 2:00 o'clock meeting, the afternoon meeting—that didn't appear on today's list, but did appear on yesterday's—

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it was a level that had Jess Moore and myself also and our people there. We concluded that meeting, where there had been a discussion of concern about whether the facility would make it successfully through all of the parameters because of temperature, that if any concerns for temperature did occur, would we please be called and we would discuss them.

And it was highlighted with respect to the facility, but it was a general request to the team that was there, that represented each of these.

DR. WHEELON: Arnie, had you known of this divergence among the technical people at Morton Thiokol, would you have proceeded to launch?

MR. ALDRICH: I would have proceeded to review it in detail at the next higher level, which is my level, and I can't say what decision we would have made. But we would have pursued the same things that are being pursued here today and were pursued in the discussion that was held by the people on that night.

CHAIRMAN ROGERS: I think Mr. Armstrong's question goes to the heart of the matter. Let's talk a little more about it. I mean, if you just have a checklist and you say everybody is checked off, and then you have no assurance that the human factors are going to come into play, so that if there is real concern on

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the part of people that you're not going to notify, it seems to me that that is——

VICE CHAIRMAN ARMSTRONG: Let me just characterize the question is a little different way. Your rule says that you should be above 31 degrees, and beyond that there is no detailed specification of past history. So one could assume that, within your mission rules at least, it could have sat there for two months at 31 degrees and everything could be at 31 degrees, seals and the mean bulk temperature.

MR. ALDRICH: No, there is a rule on the bulk temperature and that would have been violated.

VICE CHAIRMAN ARMSTRONG: I guess I am questioning whether you believed that the kind of approach, this approach of deviations being required to be reported to you and discussed at commit meetings, is satisfactory?

MR. ALDRICH: Well, I'm certain we will discuss downstream today and as we go forward the formality of our process, the types of meetings we had, and what might be more appropriate or more adequate now.

I believed up until this point that the process we were using was thorough and adequate. In fact, could I characterize the meeting? I might have

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talked too long, but if I could characterize the meeting.

CHAIRMAN ROGERS: No, why don't you talk more.

MR. ALDRICH: This meeting came out of a day where we held, because the hatch handle had a problem and that delayed us, and during that delay this weather front progressed into the Florida area. The winds became too high to support a return to launch, and so we scrubbed.

There were no other problems known with the launch system or the flight system at that time. We had this meeting to decide if it was reasonable to proceed the next day, and the primary point of discussion was whether, if the winds had been high, to scrub on the 27th.

It was projected to be cold on the 28th, and so the focus of the meeting—the way we do that is we have a weatherman come in from the Kennedy weather station, that supports us in great detail, and he made a full presentation of the weather forecast, which we had at the prior meetings also, because that was the point of involvement.

All parties there in this meeting heard the weather forecast and heard the report that the night

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before the temperatures had dipped to 22 to 24 degrees and the predicted range was 24 to 27, but perhaps as low as 22 to 24, that for the preceding, but it was going to be above freezing by launch because that was going to be at 11:00 or 11:30.

So that was a well-known fact of the condition we expected. Now, one year previously we had the STS-51-C launch, in January of the year before, and we came up on a very cold night with a similar set of discussions at that time, and we had management meetings.

And this is all, by the way, my recollection, and I'm not reading this from a formal review, but this is my recollection.

We had a meeting that day. The forecast was it would be very cold that night, and should we tank the external tank and should we proceed with the launch? And we said we don't know, let's meet again later in the evening and see how we are proceeding with the filling of the tank.

We met late in the evening, and the concern we expressed, that the cold might be difficult for us, was in fact true, because the facility was already experiencing ice and problems with their equipment. And at that time we elected not to go because it was clear

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the facility needed a better procedure for protecting it from cold weather that was not in place.

However, also at that time a year ago, we had no discussion about other elements of the launch and flight system being concerned or proceeding with that cold and proceeding on the launch day.

In other words, it was a similar situation. However, we did not proceed because of specific problems with the ground equipment. And I think in some of our minds, in my mind—and by the way, I was not in this job at that time. I was in a job where I was at the meeting, where I was involved directly with the orbiter at that time.

But I got a good impression out of that meeting that there was, beyond the facility, not a concern with launching at cold temperature in the range of 30 degrees, and my impression was we were proceeding to launch.

CHAIRMAN ROGERS: You're speaking about a year ago?

MR. ALDRICH: A year ago.

CHAIRMAN ROGERS: But did you go through them on that launch? Wasn't there a blow-by that launch in the O-rings a year ago?

MR. ALDRICH: There was an item that was worked

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later.

CHAIRMAN ROGERS: Now, why wouldn't that call everybody's attention that weather might have been related to that blow-by, and why a year later, when you had even colder conditions, wouldn't anybody in the loop have said, my God, even the Thiokol people are split on whether this will work or not.

MR. ALDRICH: Well, that had been reviewed in a series of meetings over the course of that year, and in fact there was more blow-by and more erosion. And I would have to yield to these people that worked this problem better than myself, that the correlation of that to cold was never made totally clear in the discussions that I am aware of or the documentation that I know about.

CHAIRMAN ROGERS: In other words, no one related the fact that that happened to be the coldest launch and that launch was the only one that had the blow-by?

MR. ALDRICH: I couldn't say no one related that. That was not raised as an issue at the level in the program that I'm speaking from.

CHAIRMAN ROGERS: Just going back for a moment to the procedure, just speaking personally now, I'm really surprised that you and Jess Moore were not made

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—that you weren't given the knowledge that there was such a considerable question and even a split among the engineers at Thiokol.

Didn't you know that at one point that evening they had recommended against launch and changed their mind?

MR. ALDRICH: I did not know that they had this series of telephone calls.

CHAIRMAN ROGERS: But did you know they had recommended against launch the night before?

MR. ALDRICH: No, sir.

MR. ACHESON: Who is the senior NASA official who knew of the split of opinion at MTI?

MR. REINARTZ: I was at Marshall. The discussion that we had, and as Jerry Mason indicated, that the evidence of the informal poll that was mentioned here today, was not provided as

a matter of course on the telecon that we had that evening. There was the initial discussion, as Jerry had indicated, Mr. Mason had indicated, that the initial recommendation and question as Mr. Mulloy went through a set of rationale and asked for comment. I then also asked Mr. Hardy, who was our senior technical representative that was on the line in Huntsville, to comment or add the technical position from Huntsville. And he stated that he had in essence

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agreed with the rationale of Mr. Mulloy, but however he did not want MTI to be recommending or to indicate that they would not—excuse me, let me start again.

MR. ACHESON: I was asking for a name, the name first, and then the description.

MR. REINARTZ: All right. I was the individual. The Marshall projects, including the SRB, report to me, the Marshall projects, shuttle projects manager, and in that sense I was the senior program official that was on the line on the telecon.

And we had the discussion. We were not made aware, as was indicated—your other question was the poll. We were not made aware of that situation. We knew that the initial discussion had been the 53 degrees. We were certainly aware of that.

And after we went through and Mr. Hardy had said—and maybe it would be best to let him directly say, that he made a comment asking Thiokol for their further views, because he was surprised at their recommendation on the basis of the data they had with that for the 53 degrees.

And he asked them to—he said: Does that confirm that? He did not want to put them in the position of saying, hey, we are trying to force anybody to launch. He wanted the MTI opinion and their

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technical opinion of that. He asked them if they had any additional data supporting their opinion.

It was at that time that Thiokol then went off the line to have their caucus.

And in regard, I might address the other one, Mr. Chairman, that, as I believe that Arnie said and as far as was made available to me at that time, that we were within the ground rules and had no violations of ground rules regarding any of the written requirements.

And as Mr. Armstrong said, in discussing any concerns that would prevent you from going, that after we had our discussion and then we came to our final conclusion, we did not have a concern that would prevent us from going at that time.

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DR. RIDE: Why did you not at least bring up the meeting the concern, the original concern, and the discussions, and Level 2?

MR. REINARTZ: I guess, Sally, at that point it was because there was not any violation of the activities. There are a number of activities at all of the projects, both at Marshall and JSC. There are Level 3 considerations or items involving their hardware, and they are discussed, and the concerns are discussed and then resolved. And if they do not have an impact or waiver of the requirements, it is not a foregone thing that each one of those is then brought to Level 2, if they are successfully resolved within the framework of the criteria.

DR. RIDE: Did you at Marshall have any data or any models for simulations that you considered reliable to indicate that you could launch at 38 degrees, or 31 degrees?

MR. REINARTZ: George, I would have to ask you, on that question.

MR. HARDY: Let me just go back a little bit and cover the consideration of the discussions and my own position regarding the meeting.

CHAIRMAN ROGERS: Hold on just a minute, before you tell us, because I think we have an idea what

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the general argument is and the position is, what you knew about Thiokol and the engineers and their attitude, I mean, specifically now.

These things are tough, and we're not trying to make it tougher for you, but we might as well know. Were there two or three engineers in Thiokol who expressed a view that the launch shouldn't take place, and did you know that?

MR. HARDY: I did not know, specific, by name, at Thiokol, who was making firm recommendations as to launch or not launch.

CHAIRMAN ROGERS: But did you know there was a division?

MR. HARDY: I did not know the degree of division. I was well aware of the concerns that Mr. Boisjoly had and that he did express during the conversation, and the points that he brought out about the concern of the performance of the seal under cold temperatures.

I would say that, of all the people involved in the teleconference, my ability to detect levels of concern during the course of that discussion would be that he appeared to be the individual that expressed the greatest concern.

CHAIRMAN ROGERS: And you didn't convey that

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to Mr. Aldrich and Mr. Moore?

MR. HARDY: No, sir. That would not be in my reporting channel to do that.

VICE CHAIRMAN ARMSTRONG: Mr. Chairman, I think I kind of understand all the details of this, but the gap that appears in my mind is the fact that we seem to have a situation where we are intended to have an operational vehicle that is going to fly on winter mornings for twenty years, and it should be able to handle conditions that are down around freezing, certainly if it is going to be operational in that category; and we have the mission rules, a launch rule that says it ought to be able to launch at 31 degrees or above; and yet we have seals that have constraints that are substantially away from what I would think would be a normal operating environment.

They have had to work very carefully to make sure that, in fact, this critical system was going to operate well within what I would think would be an operating envelope.

If I don't understand that right, I would like to have somebody tell me why that doesn't characterize the system.

DR. WALKER: In fact, there weren't any specific constraints on the O-ring temperature, specific

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to the O-ring.

DR. RIDE: Well, that is one of the things I don't understand, because I guess I agree with Crip, that the NASA philosophy is it is supposed to be proved to me that it works under these conditions, and, as far as I can see, there is no data and no tests that indicate, that give you any confidence that the joint would be expected to operate at 31 degrees. I mean, maybe it does, but there is no data that proves that.

MR. SUTTER: Could I ask one more question, a quick one, I hope?

In this case, there was an input—don't fly, and then it was changed to do fly, and over all of the other launches. Has this happened before many times? And was it settled at Level 3, Level

2, or Level 1? Can you go back in history? Is this a normal occurrence or is this a very unusual occurrence?

MR. ALDRICH: I would try to answer that. It is hard to recreate history of a fifteen year program, and all of the critical things we have worked. But I would personally think that the process that these gentlemen talked about, polling all of the engineers and hearing all of the opinions and their positions, and finding there is disagreement, is probably something that has occurred often.

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CHAIRMAN ROGERS: I think you ought to consider, though, on criticality one—I mean, I could see others, but Joe's question seems to be, in my mind, too directed toward criticality one. Everybody knows if there is a failure, all is lost. So it seems to me everybody would be alerted, and you had the experience the year before, and that was a cold day.

It's hard for me to understand why everybody didn't talk to everybody about it and say my God, there are some concerns, are we sure that we are going to be protecting, that the astronauts are going to be protected?

They just assumed, everyone assumed, the temperature of the O-rings would be the same as the ambient temperature, or would be at the time of launch?

MR. MASON: No.

We ran a calculation based upon the temperature forecast and the heat transfer to get the O-ring temperature. And, again, that is on one of Mr. Lund's charts. We didn't make an assumption. We ran a calculation what the temperatures would be.

GENERAL KUTYNA: And you had IR readings, is that not true, of some sort?

MR. ALDRICH: The IR readings that were discussed yesterday were taken at various points on the

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external of the vehicle, and they were taken on the lower portion of the SRB. It would take a calculation to extrapolate that also, to get the O-ring temperature.

GENERAL KUTYNA: But what did that calculation reveal as far as the temperature?

It was down in the twenties, below?

MR. ALDRICH: I wish Horace Lamberth were here to give you exactly what he said yesterday. But the gist on the discussions of the readings that were taken with the IR was that there was a requirement to take them on the external tank, and that is taken with the understanding that we want to understand ice on the external tank for its impact to the launch system when we lift off. The readings on the SRB were taken by the team because they chose to, and there is neither a requirement to take them nor a specification for what range would be acceptable or not acceptable.

They were not reported to any point in the chain because there is not such a requirement.

DR. WALKER: That raises a question as to why there was not a requirement to get a temperature on the SRB near the position of the rings if Morton Thiokol thought that was a concern.

I mean, for example, why didn't the Morton Thiokol people ask for a temperature measurement there?

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DR. KEEL: Mr. Chairman, if it will clarify the record according to my notes, Mr. Lamberth said yesterday that the IR readings indicated that the left solid rocket motor had temperatures

as low as 25 degrees, and the right-hand as low as 10 degrees, and with some lower temperature in the neck of the nozzle.

CHAIRMAN ROGERS: I suggest we take a recess for ten minutes.

DR. WHEELON: I can't let that go by. You said there was a requirement to take higher measurements. He said there weren't. What is the story?

DR. KEEL: No, I just said that there were measurements taken, and that is where he indicated the readings were.

MR. ALDRICH: He is reporting on the measurements that were taken as a matter of course, on the day before the launch. It was not a requirement, specifying SRB constraints. There was no requirement to measure that.

DR. WHEELON: But, in fact, measurements were made and they were 25 degrees.

MR. ALDRICH: Measurements were made. They are under investigation and test, because the measurements on the right SRB were lower than expected or can be explained, and that was part of the discussion

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yesterday. They were 24 approximately on the left SRB, and they were in the seven to nine range on the right SRB. We think there is a magnitude of error between five to as much as, I guess, perhaps twelve degrees being worked by the team now.

CHAIRMAN ROGERS: Okay.

If we may, I would like to have a recess now.

[A brief recess was taken.]

CHAIRMAN ROGERS: The Commission will come to order.

We have just had a private meeting and decided to eliminate a couple of the items on the agenda, so that we can complete our work today. Mr. Keel will point that out.

We want to go ahead with this discussion, and when you finish your presentation, then we would like to have——

MR. MASON: I think I am finished, unless there are more questions.

Mr. Lund was next up.

CHAIRMAN ROGERS: And then we would like to have the two engineers that raised some questions about it.

This gentleman—what's your name?

MR. BOISJOLY: Boisjoly, Roger Boisjoly.

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MR. THOMPSON: Thompson, Arnold Thompson.

CHAIRMAN ROGERS: One other thing that I would like to comment on, and I hope everybody is listening, is this.

I would like everybody to consider that the Commission has requested from each of you all of your private notes—not only your official notes and your official files, but any private notes you have in your own handwriting or any other notes you have, particularly as they relate to these conversations we have been talking about this morning.

MR. MASON: Would it be appropriate if we gather all of those notes—some of those would be in Utah and some in Huntsville—if we gathered those and delivered them on, like, Monday?

CHAIRMAN ROGERS: Yes.

I would like to have you work it out with Mr. Keel, the Executive Director. I mean, there may be some that we would want to get directly, I don't know. What we want to be sure about is, because of the nature of these documents, to be sure that we've got them first-hand, and then

anybody who wants to give us documents should feel free to give them to the Commission directly, if they would like to.

So I will leave that up to Dr. Keel about how

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those will be delivered.

The principal point is we want to be sure that they are protected and to save them, so that no claim can be made later on that they were destroyed, because of their importance.

Okay, Mr. Lund.

TEMPERATURE CONCERN ON SRM JOINTS

27 JAN 1986

[Ref. 2/14-3 1 of 13]

HISTORY OF O-RING DAMAGE ON SRM FIELD JOINTS

			Cross Sectional View			Top View		
		SRM No.	Erosion Depth (in.)	Perimeter Affected (deg)	Nominal Dia. (in.)	Length Of Max Erosion (in.)	Total Heat Affected Length (in.)	Clocking Location (deg)
Oct 30, 1985	61A LH Center Field**	22A	None	None	0.280	None	None	36°–66°
	61A LH ~~CENTER~~ AFT FIELD**	22A	NONE	NONE	0.280	NONE	NONE	338°–18°
	51C LH Forward Field**	15A	0.010	154.0	0.280	4.25	5.25	163
	51C RH Center Field (prim)***	15B	0.038	130.0	0.280	12.50	58.75	354
	51C RH Center Field (sec)***	15B	None	45.0	0.280	None	29.50	354
	41D RH Forward Field	13B	0.028	110.0	0.280	3.00	None	275
	41C LH Aft Field*	11A	None	None	0.280	None	None	--
	41B LH Forward Field	10A	0.040	217.0	0.280	3.00	14.50	351
	STS-2 RH Aft Field	2B	0.053	116.0	0.280	--	--	90

*Hot gas path detected in putty. Indication of heat on O-ring, but no damage.
**Soot behind primary O-ring.
***Soot behind primary O-ring, heat affected secondary O-ring.

Clocking location of leak check port - 0 deg.

OTHER SRM-15 FIELD JOINTS HAD NO BLOWHOLES IN PUTTY AND NO SOOT NEAR OR BEYOND THE PRIMARY O-RING.

SRM-22 FORWARD FIELD JOINT HAD PUTTY PATH TO PRIMARY O-RING, BUT NO O-RING EROSION AND NO SOOT BLOWBY. OTHER SRM-22 FIELD JOINTS HAD NO BLOWHOLES IN PUTTY.

[Ref. 2-14-3 2 of 13]

PRIMARY CONCERNS -

- FIELD JOINT - HIGHEST CONCERN
  - EROSION PENETRATION OF PRIMARY SEAL REQUIRES RELIABLE SECONDARY SEAL FOR PRESSURE INTEGRITY
    - IGNITION TRANSIENT - (0-600 MS)
      - (0-170 MS) HIGH PROBABILITY OF RELIABLE SECONDARY SEAL
      - (170-330 MS) REDUCED PROBABILITY OF RELIABLE SECONDARY SEAL
      - (330-600 MS) HIGH PROBABILITY OF NO SECONDARY SEAL CAPABILITY
  - STEADY STATE - (600 MS - 2 MINUTES)
    - IF EROSION PENETRATES PRIMARY O-RING SEAL - HIGH PROBABILITY OF NO SECONDARY SEAL CAPABILITY
      - BENCH TESTING SHOWED O-RING NOT CAPABLE OF MAINTAINING CONTACT WITH METAL PARTS GAP OPENING RATE TO MEOP
      - BENCH TESTING SHOWED CAPABILITY TO MAINTAIN O-RING CONTACT DURING INITIAL PHASE (0-170 MS) OF TRANSIENT

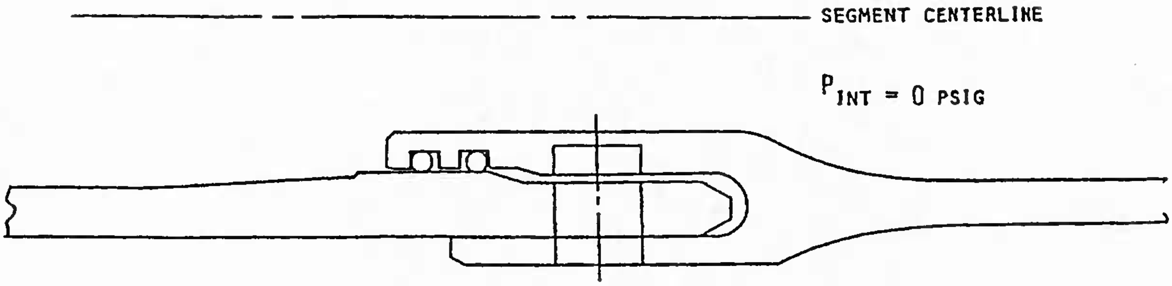
[Ref. 2/14-3 3 of 13]

ELD JOINT PRIMARY CONCERNS SRM 25

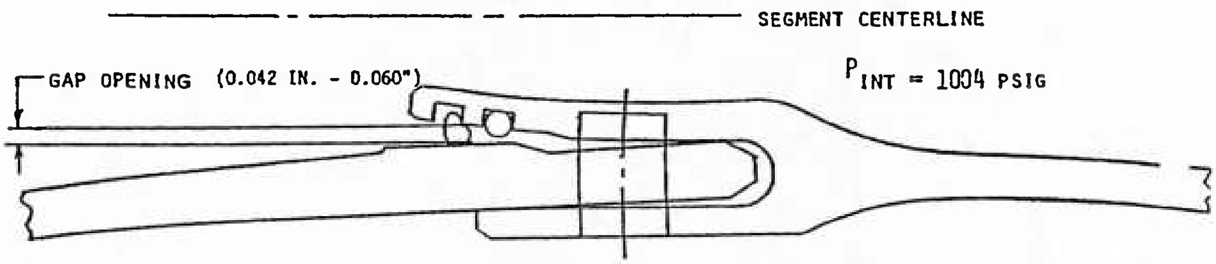
- A TEMPERATURE LOWER THAN CURRENT DATA BASE RESULTS IN CHANGING PRIMARY O-RING SEALING TIMING FUNCTION
- SRM 15A — 80° ARC BLACK GREASE BETWEEN O-RINGS
  SRM 15B — 110° ARC BLACK GREASE BETWEEN O-RINGS
- LOWER O-RING SQUEEZE DUE TO LOWER TEMP
- HIGHER O-RING SHORE HARDNESS
- THICKER GREASE VISCOSITY
- HIGHER O-RING PRESSURE ACTUATION TIME
- IF ACTUATION TIME INCREASES, THRESHOLD OF SECONDARY SEAL PRESSURIZATION CAPABILITY IS APPROACHED
- IF THRESHOLD IS REACHED THEN SECONDARY SEAL MAY NOT BE CAPABLE OF BEING PRESSURIZED

[Ref. 2/14-3 4 of 13]

PRIMARY CONCERNS - CONT



UNPRESSURIZED JOINT - NO ROTATION



PRESSURIZED JOINT - ROTATION EFFECT (EXAGGERATED)

[Ref. 2/14-3 5 of 13]

BLOW BY HISTORY

SRM-15 WORST BLOW-BY

- 2 CASE JOINTS (80°), (110°) ARC
- MUCH WORSE VISUALLY THAN SRM-22

SRM 22 BLOW-BY

- 2 CASE JOINTS (30-40°)

SRM-13A, 15, 16A, 18, 23A 24A

- NOZZLE BLOW-BY

[Ref. 2/14-3 6 of 13]

O-RING (VITON) SHORE HARDNESS VERSUS TEMPERATURE

°F	SHORE HARDNESS
70°	77
60°	81
50°	84
40°	88
30°	92
20°	94
10°	96

[Ref. 2/14-3 7 of 13]

SECONDARY O-RING RESILIENCY

DECOMPRESSION RATE
2"/MIN (FLIGHT ≈ 3.2"/MIN)

TEMP (°F)	TIME TO RECOVER (SEC)
50	600
75	2.4
100	*

* DID NOT SEPARATE

RIGHT DUROMETER (?)

[Ref. 2/14-3 8 of 13]

BLOW-BY TESTS (PRELIMINARY)

ARGON	TEMP. (°F)	RESULTS (IN^3/IN SEAL)
	75	NO LEAKAGE
	30	NO LEAKAGE

F-14		
	75	NO RESULTS YET
	30	NO RESULTS YET

Pres	
0	0
400	170 ms
	330 ms

1000 psi in 0.6 sec
3 to 4 tests at each condition
.020 compression

[Ref. 2/14-3 9 of 13]

FIELD JOINT O-RING SQUEEZE (PRIMARY SEAL)

MOTOR	FWD	CTR	AFT
SRM 15 A	16.1 (.045)*	15.8 (.044)	14.7 (.041)
SRM 15 B	11.1 (.031)	14.0 (.039)**	16.1 (.045)
SRM 25 A	10.16 (.028)	13.22 (.037)	13.39 (.037)
SRM 25 B	13.91 (.039)	13.05 (.037)	14.25 (.040)

* 0.010" EROSION
** 0.038" EROSION

[Ref. 2/14-3 10 of 13]

CHART - 4

HISTORY OF O-RING TEMPERATURES

(DEGREES - F)

MOTOR	MBT	AMB	O-RING	WIND
DM-4	68	36	47	10 MPH
DM-2	76	45	52	10 MPH
QM-3	72.5	40	48	10 MPH
QM-4	76	48	51	10 MPH
SRM-15	52	64	53	10 MPH
SRM-22	77	78	75	10 MPH
SRM-25	55	26	29 27	10 MPH 25 MPH

1-D THERMAL ANALYSIS

[Ref. 2/14-3 11 of 13]

CONCLUSIONS:

TEMPERATURE OF O-RING IS NOT ONLY PARAMETER CONTROLLING BLOW-BY

SRM 15 WITH BLOW-BY HAD AN O-RING TEMP AT 53°F
SRM 22 WITH BLOW-BY HAD AN O-RING TEMP AT 75°F
FOUR DEVELOPMENT MOTORS WITH NO BLOW-BY WERE TESTED AT O-RING TEMP OF 47° TO 52°F

DEVELOPMENT MOTORS HAD PUTTY PACKING WHICH RESULTED IN BETTER PERFORMANCE

AT ABOUT 50°F BLOW-BY COULD BE EXPERIENCED IN CASE JOINTS

TEMP FOR SRM 25 ON 1-28-86 LAUNCH WILL BE 29°F 9 AM
38°F 2 PM

HAVE NO DATA THAT WOULD INDICATE SRM 25 IS DIFFERENT THAN SRM 15 OTHER THAN TEMP

[Ref. 2/14-3 12 of 13]

RECOMMENDATIONS :

- O-RING TEMP MUST BE $\geq$ 53 °F AT LAUNCH

  DEVELOPMENT MOTORS AT 47° TO 52°F WITH
  PUTTY PACKING HAD NO BLOW-BY
  SRM 15 (THE BEST SIMULATION) WORKED AT 53 °F

- PROJECT AMBIENT CONDITIONS (TEMP & WIND)
  TO DETERMINE LAUNCH TIME

[Ref. 2/14-3 13 of 13]

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TESTIMONY OF ROBERT LUND, MORTON THIOKOL, INC.

(Viewgraph.) [Ref. 2-14-4]

MR. LUND: My intent to the Commission is to review those charts that we reviewed in this Monday night telecon and to make it very clear I was not the presenter on any of the charts, except the conclusions charts. There were other engineering people at the Wasatch Division making the presentation on the individual charts that they were preparing in real time.

Listed there were our understanding of those people who were involved. I think that Mr. Malloy's presentation is a more complete list. This would be incomplete as far as those who were involved.

(Viewgraph.) [Ref. 2/14-5]

MR. LUND: The thing that we were most concerned about from an engineering standpoint was reflected in the history of O-ring damage on the SRM field joints.

As noted on this chart, the top five notations here are the joints and the locations in the joints, in the motors, of Motor 61-A and 51-C: the erosion depth that occurred in those motors, the perimeter around the circumference of the O-ring that was effected by that erosion, and, of course, the nominal diameter of the

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O-ring. And then, if you took the O-ring and looked at at top view, looking down, this would be the length of that erosion. And then we looked for what we called heat affected zone, where you see some discoloration of the O-ring.

What brought the concern——

DR. COVERT: Mr. Lund, on the chart I have, there is another column that is not on the viewgraph.

MR. LUND: Let me tell you what that——

DR. COVERT: I'm just curious, what plus-Z and minus-Z are with respect to 36 degrees or something like that. I need to tie the two together with what I know.

GENERAL KUTYNA: Three sixty is the bottom on the SRB.

DR. COVERT: Three sixty is the bottom, so that's minus Z. Okay. Thank you, Don.

MR. LUND: Now, of chief concern were these five at the top. Those were the ones that experienced blow-by. The way we can tell what blow-by is, is we look for discoloration in the area aft of the primary O-ring. In the two instances, the one that was the most severe was the 51-C. There was black material between the two O-rings. And in the 61, there was some black material, but it was not anywhere near the amount that we experienced on 51-C. Also listed there for our

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consideration was the fact that on other motors we had had O-ring erosion, but no blow-by.

Now, what was not listed on the chart that night, but what was discussed in the meeting, was the fact that this motor was about, well, it was at 75 degrees, for a calculated O-ring temperature at the time of the launch.

This motor was at 53 degrees at the time of the launch.

DR. FEYNMAN: That says none. What was the matter with 61?

MR. LUND: It had blow-by. There was black products past the primary O-ring.

I think that's the message of that chart.

MR. CRIPPEN: Excuse me, sir.

Can we look at that chart and see the three cases of—what have you got there? Nine—and still say that there was no temperature correlation?

MR. LUND: The temperatures on the other motors, on 41, was 64 degrees. I'm sorry. I don't have, these are the overnight low temperatures. That's the data I have. These are the overnight low temperatures on the night before launch—was 64 on 41-D; 51 on 41-C; 56 on 41-B; 60 on STS-2.

GENERAL KUTYNA: And 53 on 51-C.

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MR. LUND: Let me review that again.

What I have given you is the overnight low temperatures. I don't have at my disposal the calculated——

GENERAL KUTYNA: What was it on 51-C, the overnight low?

MR. LUND: 51-C was 34.

DR. COVERT: And how about 61-A?

MR. LUND: Eighty-three.

DR. WHEELON: So the one where you had no erosion was the highest temperature by quite a bit?

Sixty-one-A had a relatively high temperature and overnight ambient temperature, and yet it had no erosion?

MR. LUND: But it had blow-by.

DR. WHEELON: The others were all in the forties and fifties?

MR. LUND: The overnight lows were in the fifties and sixties.

MR. KILMINSTER: I would like to correct one statement that was made about the caulking location on the right hand. For the right hand SRB, that is correct. Zero is in the minus-Z position. For the left-hand, it is in the plus-Z position.

MR. CRIPPEN: Unfortunately, some people use Z

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in different directions. Could you be a little bit clearer?

MR. KILMINSTER: Minus Z is away from the orbiter. Plus Z is toward the orbiter.

(Viewgraph.) [Ref. 2/14-6]

MR. LUND: What I would like to do is, is there anyone in here who is not familiar with that chart? We have had enough of that chart.

CHAIRMAN ROGERS: Yes, we have been over it several times.

(Viewgraph.) [Ref. 2/14-7]

MR. LUND: This chart has been previously reviewed. As was noted, the field joint was our highest concern. What we were concerned about is the erosion penetration of the primary seal, and if it penetrates, we want a very reliable secondary seal.

We had talked about the fact that that is a function of how quickly the pressure comes up in the motor. The radial expansion and the axial expansion of the steel deforms the joint. That is shown over here.

So we are concerned about the timing of how those seals seat and the rate of rise of pressure within the motor.

Now in steady state of course, if we have erosion penetrating this primary seal, there is a

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possibility that this seal would not be there, and we could have blow-by and gas escape.

MR. SUTTER: I'm confused with that statement.

I thought the decision was if you had the primary blow, the secondary would hold? That it was a simplex seal?

MR. LUND: Only if this was in position early in the ignition process.

MR. SUTTER: I thought NASA's position said that a simplex seal is okay, which means that if the primary is gone, the secondary is holding. That is what I thought NASA was leaning on.

VICE CHAIRMAN ARMSTRONG: I think, to clarify that position a little bit, when we reviewed this at the Tuesday meeting, I guess it was, they pointed out that the reason it went from criticality 1-R to criticality 1 was that during testing—not firing, but testing—of the flexure, here (indicating), they found that they get this particular situation, and the secondary seal might not seat, in which case they only had one seal, the primary seal, and that is what caused the change.

It wasn't a firing test that did this, as I understand it.

MR. LUND: That is correct.

VICE CHAIRMAN ARMSTRONG: But it was static

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testing that made them to go this criticality 1 as opposed to criticality 1-R, that is, this particular characteristic right here.

MR. SUTTER: But, during these conversations, with the concern over temperature that surfaced, I thought that the chart said it is still okay because you've got the simplex seal, which is a secondary seal. Am I confused?

VICE CHAIRMAN ARMSTRONG: No. That's true.

MR. LUND: We'll talk a little bit more about that in just a second. All I'm saying is that in a normal motor operation, you could experience this sort of thing. It could happen. That is a worst case.

DR. COVERT: Sir, have you ever taken this joint apart and examined the pin for wear or abrasion or scarfing of any kind, that would indicate that there is a bending of the nature that has been shown in that sketch?

MR. LUND: Maybe Mr. Boisjoly can talk about that. He's worked hard on that.

MR. BOISJOLY: Yes. I've taken many of them apart at the Cape, when they return from flight, and there is no indication in the pins at all that they've even been rubbed or even been used, for that matter. The only time the pins get pushed out of the recycle flow is when they have slight pitting, due to corrosion

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or something on that nature. We would take them out of the flow.

But, as far as wear marks, the only wear marks that have ever been seen on any pins are those units that were taken purposely as engineering test units to burst, and then you started to see the shear effect and the rotation effects on the pins.

DR. COVERT: I'm thinking about fretting.

MR. BOISJOLY: There is no indication on any pins I'd ever seen.

DR. COVERT: Are the pins that loose that it's possible to get a deflection without fretting?

MR. BOISJOLY: No, I don't think so.

MR. LUND: I agree with that.

(Viewgraph.) [Ref. 2/14-8]

MR. LUND: This next chart then summarized our concerns in the list.

First of all, the temperature lower than the current data base results in changing primary O-ring sealing-timing function. We were concerned that, the O-ring was going to move at a different rate.

We had some indication of that with 15-A and 15-B, with the black in the area between the O-rings.

We also know with the temperature that the O-ring does contract, and there is a 2 or 3 mil

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contraction of the rubber itself at the lower temperature.

We know that the O-ring was harder at lower temperature.

We indicated that, of course, the grease is there, and it has a higher viscosity at the lower temperature.

Our feeling was, then, from all of that, that we would have a higher O-ring pressure actuation time.

Our concern was if the actuation time increased, the threshold of secondary seal pressurization capability is approached; and if the threshold is reached, then the secondary seal may not be capable of being pressurized.

Are there any questions on that chart?

MR. WAITE: So, you're saying you may not have any seals?

MR. LUND: Several things had to happen to cause that to occur.

MR. WAITE: Okay.

I want to get back to Mr. Sutter's point, that the primary seal concept was based on the fact that the secondary seal had been seated by the initial pressure check. You don't show that.

MR. LUND: In the seating process, if you have

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the instant blow-by, because you would have a pressure at the secondary O-ring at time zero.

MR. WAITE: No. We're talking about pre-launch, pre-operation procedures which seat the secondary O-ring.

MR. LUND: Oh, the leak check.

MR. BOISJOLY: That shows in the top chart, the position of the secondary O-ring.

MR. LUND: The secondary is here, as a result of the leak check.

MR. WAITE: But in terms of the discussion we've been going through this morning, if you're talking about a primary seal, it is the secondary seal that is the primary seal.

MR. LUND: There is a scenario that would draw that conclusion. That is correct.

MR. WAITE: I'm having trouble with your scenario with that second seal, upset like that.

MR. LUND: If the primary O-ring seals, then this could result.

MR. WAITE: Even though it had been seated before?

MR. LUND: You see, the resiliency and the cold temperature and the rotation would tend to make you do that.

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Now, the pressure force, even if this came through, the pressure force may be sufficient to push it in. But, in a worst case scenario, it could be up there if the primary seals.

MR. WAITE: Do you agree with that?

MR. SUTTER: I'm slightly confused.

MR. WAITE: Are you saying this scenario calls for a change in the secondary seal from its previous condition?

MR. LUND: This scenario is the one that has drawn all the attention for several years, that the gas comes down, actuates the primary seal, and it seals. As the rotation occurs, there is a possibility that the secondary seal would lose squeeze and be capable of having blow-by, if it ever burned through the primary.

It is not an easy concept.

CHAIRMAN ROGERS: Okay, Mr. Lund.

Proceed with your presentation.

(Viewgraph.) [Ref. 2/14-9]

MR. LUND: This was a summary chart prepared by the next presenter.

In essence, he's just reiterated the same thing we had seen, that the SRM 15 had the worst blow-by, must worse visually than SRM 22. Twenty-two had blow-by and we also experienced, of course—then

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also blow-by. And that was also discussed this morning.

(Viewgraph.) [Ref. 2-14-10]

MR. LUND: I would like to show you now the hardness data on the O-ring. This is the Shore A reading.

The hardness goes from 77 to 96, as you go from 70 down to 10 degrees.

DR. FEYNMAN: What is the hardness data—the amount of force you need to change it?

MR. LUND: Yes. You push on it and measure the amount of push.

DR. FEYNMAN: It has nothing to do with time?

MR. LUND: No. It's just hardness.

DR. WHEELON: Is that like a standard Brinell hardness test?

MR. LUND: It's like a Rockwell A on rubber.

(Viewgraph.) [Ref. 2-14-11]

MR. LUND: This is a result of some of the resiliency testing that we're running. During the initial pressurization rate of the solid rocket motor, pressure comes up relatively slowly for the first hundred milliseconds or so, and then goes more quickly, and then turns over and goes slowly again.

If we took a secant across the total time, we would have a rate of movement of the two metal parts that the O-ring is trying to seal of about 3.2 inches

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per minute.

Your time is much shorter, and so are the deflections, so that is the equivalent time.

And so we put into our tensile testing machine a two-inch-per-minute rate. We compressed the O-ring in a groove—and let me get those exact—and "squozed" the O-ring. Forty thousandths is very representative of what the squeeze is, as we squash the O-ring between the two metal parts.

CHAIRMAN ROGERS: When you say "we," was this an old test?

MR. LUND: Yes.

This has been done previously.

And then we moved the head of this tensile machine away from the O-ring at two inches per minute.

VICE CHAIRMAN ARMSTRONG: At what point is it moving two inches per minute?

MR. LUND: The O-ring is in a groove, and we squash it down 40/1,000ths, and then we moved the head of the tensile machine away from the O-ring at two inches per minute to try to simulate—

VICE CHAIRMAN ARMSTRONG: It doesn't lie within a groove when you're doing this? This is just a push against the machine?

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MR. LUND: Yes. The machine forms the other seating surface that would occur in a rocket motor.

What this said was that at 100 degrees, as that head came back to its original position, I'm sorry, not to its original position, but to within 5/1,000ths of its original position, still maintaining the squeeze, that at a high temperature, at 100 degrees, the O-ring came right back.

It followed the machine right up. At 75 degrees, it took several seconds to recover. And, as we went lower in temperature, it took much longer to recover.

DR. WHEELON: That's pretty nonlinear. How do you account for that?

MR. LUND: It is the modulus of the rubber. Those polymers do that.

DR. FEYNMAN: The rubber—ordinarily in materials, like steel or something when you squash it, you are compressing the molecules together and they simply expand back. When you stretch a piece of rubber, the reason that it responds is because of dynamic motion. It is trying to shake molecules and pull something; like, you take a long chain across a room, which has a lot of tennis balls bouncing in it. The chain will be "ponged" by the balls and pulled

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together. If the balls are slowed up, and low temperature means slowed up, then there is much less ponging and much less pulling back together, and the same way responding.

I used the expansion. But you can do the same thing with compression. If you compress it out of shape, it goes back into shape because of thermal notion, really, not because of spring. And when the thermal temperatures change, it goes back very, very much lower.

It is very characteristic of materials of this kind, that have this enormous effect. Temperature has such enormous effect.

DR. COVERT: Does it follow the square root?

DR. FEYNMAN: No. It's E to the minus A over T. It is exponentially. So, at 32 degrees on the scale, you probably wouldn't be able to measure the time. It would be too late to wait for the hour, or whatever.

MR. LUND: Now, keep in mind that resiliency is not the only issue. But you have the pressure load coming in and the pressurization of the motor kicking that O-ring up and saying go with me.

So this is one factor.

(Viewgraph.) [Ref. 2/14-12]

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DR. FEYNMAN: But if there is a gap left, is there really any pressure remaining to push? Suppose the O-ring did return, and there's a little gulf, and the gas comes. Why wouldn't that push it?

DR. COVERT: Because there's a lower pressure at the minimum area point.

MR. LUND: There's a nozzling effect, and the pressure is going to go down. If the pressure is half that, it is out in the chamber it's feeding, and it would suck it. That is one of the reasons O-rings work.

(Viewgraph.) [Ref. 2/14-12]

MR. LUND: Here are the results of the blow-by test.

This blow-by test fixture is simply a little ten inch rig that has simulated in it the actual geometry of the seating surfaces, only instead of being 146 inches in diameter, it is 10 inches in diameter, the O-ring diameters, the grooves. The gap widths are all the same.

What we wanted to do in this test is, first, by the same technique that is used in the motor, put pressure between the seals, to push them into the position that they actually occur in the rocket motor. Then, through another channel in the test vehicle, we pressurize, like the motor does. We loaded the

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pressurization medium with argon, and ran a couple of tests at each one of these temperatures, and there was no indication of leakage that we could tell, with the argon. And we worried about the sensitivity of that.

We are in the process of using Freon 14, which is much easier to detect and categorize at much lower leakage rates.

CHAIRMAN ROGERS: I assume this is one you are doing now?

MR. LUND: It has been done now. We were right in the process of doing this at the time of the flight. And we had this done. We were in the process of doing this.

CHAIRMAN ROGERS: Was there any particular reason why you were doing it at the time of the flight?

MR. LUND: Oh, yes.

We have been working on this problem of this whole leakage O-ring scenario for some time.

MR. ACHESON: How many tests represent a reliable body of testing data?

MR. LUND: That is a tough question. I guess I couldn't quantify it.

I would certainly think that I would always want more than two.

CHAIRMAN ROGERS: When did this test start, or

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these tests start?

MR. LUND: Arnie, can you answer that?

MR. THOMPSON: We started about eight weeks ago, I guess, or ten weeks, to build the apparatus, and we're developing the system and the techniques to measure blow-by.

CHAIRMAN ROGERS: And in these telecons that you had prior to launch, did you discuss the fact that you were then conducting tests on this?

MR. THOMPSON: This data here was presented in the telecon.

MR. CRIPPEN: Excuse me, sir. Can you tell me whether this test stimulates the flexure of the joint?

MR. LUND: No, it does not.

What we were trying to do was measure the blow-by at the early part of motor operation. At this point in time, we had not concluded the Freon 14 tests. We have completed them now.

DR. COVERT: What does a quick look say?

MR. LUND: No leakage down to in the area of zero degrees.

When you get down below zero, there is some leakage. So that would tell you that the engineering thought process is certainly in the right direction.

(Viewgraph.) [Ref. 2/14-13]

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MR. LUND: This data was discussed very briefly in the telecon. All of this data was taken at 75 degrees.

What the intent here was is to indicate that when O-rings are squashed, "squozed" together, for long periods of time, that they will not return to their original form. They have a memory.

I might indicate that that scale is exactly reversed. This is really 70, 168, 500, and 1,000. But the answer is still the same, that there is some permanent set in all of these O-rings, as you store them for long periods of time.

That is another thing that was of concern to us, that there is permanent setting in the O-rings themselves.

DR. FEYNMAN: How big is the TO minus TS? Do you remember? The distance that you squoze it in the beginning? How much do you squeeze them when you do this?

MR. THOMPSON: Twenty-five percent.

DR. FEYNMAN: Thank you.

(Viewgraph.) [Ref. 2/14-13]

MR. LUND: We also carefully looked at the geometry on the "as built" dimensions, and compared the 15-A and B motors, where we had had erosion and

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blow-by with the geometry of the motors that were to be flown. Two conclusions out of here.

You can see the squeeze percent across 15. They range from about 11 to 16. You can also see that the one that had the highest squeeze had erosion, and the one that had the lowest squeeze did not have any erosion, which tells me that squeeze is not the only parameter affecting erosion.

We looked carefully at the squeezes here. They are in the same ballpark population, with the exception of the forward here (indicating) on the left hand, up front.

(Viewgraph.) [Ref. 2/14-14]

MR. LUND: These are the results of the thermal calculations that we performed. We went through our sequence of motors and picked those motors that we thought would give us the lowest operating temperature at the time of motor operation.

We then went back and looked at the environment that the motors were exposed to and picked out those motors that would give us the lowest O-ring temperature.

This is the list. The first four of these are static test motors that were static fired at the Wasatch Division.

You can see 15, 22, and 25.

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This is the mean bulk temperature of the propellant.

This is the ambient temperature at the time of launch, or suggested launch.

Here are the results of the thermal calculations, indicating the temperatures that we would predict the O-ring to be at at the time of the launch.

MR. ACHESON: Why is there such an erratic relationship between the ambient and the O-ring temperature?

MR. LUND: It turns out that the fillum grain is a very nice oven or hand warmer for the case. The grain, of course, has a lot of rubber and has aluminum powder in it. It has a good heat capacity. But it is insulated. All those little aluminum particles are separated by polymers. And so, it is a good heat capacitor.

But it contains a lot of heat.

Well, it transmits it slowly, too.

So, what you have to look at is how hot the grain is, what the temperature is, and you will always find that this is between them. They are being chilled and heated from two different sources, because we didn't have totally definitive velocities.

MR. ACHESON: Is there putty in between? I guess there is—at the time of this measurement?

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MR. LUND: There is putty in the gap, upstream of the O-rings.

MR. ACHESON: So, wouldn't that prevent the temperature of the bulk from warming?

MR. LUND: No, just conduction. The grain, the propellant is attached to a rubber insulator that is attached to the case. So it is just a conduction effect.

We also looked to see what the effect of the convective part of the heat transfer was. We didn't have definitive winds for each of those launches, and so we just took a look and said well, what is the effect of varying that wind? And you can see it makes some difference, but not a tremendous differences in the temperature at the O-ring.

GENERAL KUTYNA: A quick point.

The night before, when we drained the external tank, we did not drain all the hydrogen out of it, which sometimes happens. You had hydrogen in there, ice cold. Would that have made that a little bit cooler because you had that hydrogen?

MR. LUND: Yes, sir.

VICE CHAIRMAN ARMSTRONG: One question.

Clearly, you had a concern about temperature, and so on. Did you ever consider or take thought of controlling the temperature at the seals, or to changing

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of the material of the seals to something that had different characteristics?

MR. LUND: Those thoughts have gone through our minds. There has been no positive action along those lines.

(Viewgraph.) [Ref. 2/14-15]

MR. LUND: So our engineering opinion, looking at that data, was, first of all, of course, that temperature of the O-ring is not the only parameter controlling blow-by. That was quite apparent to us. But we did know that SRM-15 with blow-by had an O-ring temperature that was low. We thought that was a contributing factor.

We had the four development motors from the previous chart that had no blow-by, that were tested lower than the SRM-15, in the area of 47 to 52 degrees. But, as was mentioned before this morning, because of the assembly problems, we had gone in and packed that putty, so we felt that it was a more representative case of the flight motors at that point in time. And we were concerned that we had done the right thing at that point in time, in looking back.

DR. WALKER: Let me understand that.

The motors had putty packing which resulted in better performance, better performance than the flight

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motors?

MR. LUND: The flight motors.

DR. WALKER: But I thought this morning, or earlier, rather, Mr. Mason indicated that he didn't think that packing the putty in the flight motors would really improve the performance because you couldn't necessarily see. There is evidently a difference of opinion.

MR. LUND: There is one of those that, you know, every engineer would have an opinion on that.

MR. MASON: Excuse me. I think the point was that if you packed the putty and you know there is a hole there, you would have filled it. But it doesn't assure you that you don't create one.

One of the primary ways of creating one is in the leak test. So, in the static test vehicle, you have the opportunity to run the leak test and make sure that if you have created any leaks, that they are plugged. Whereas, in the flight vehicle, you don't have that opportunity.

So, what we figured was we had a higher probability that we had plugged any leaks in the static test motor because, when we put it together and we knew we had a chance of creating the voids, and we knew that we had potentially created them with the leak test, so

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we went in and plugged those. And so, we just figured that there was a lower probability of a blow-through in the static test motor than there would be in a flight motor.

So we didn't want to deceive ourselves by assuming that the static test motor was exactly the same as a flight motor.

DR. WALKER: I guess the question I had was if you thought packing the putty was a good idea, why didn't you suggest packing the putty after the leak test for the flight motors? Is it just too difficult, because the flight motors were not accessible and you couldn't get inside easily?

MR. MASON: Fundamentally, that is correct.

You would have to take the igniter out and go down into the motor.

DR. WALKER: It just wasn't a practical thing to do?

MR. MASON: That is correct.

MR. LUND: We concluded that at about 50 degrees Fahrenheit, that blow-by could be experienced in those case joints.

We looked at the temperature and we were seeing numbers at that point in time of those kinds of temperatures. We concluded that we really don't have

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data that would indicate SRM-25 is different than SRM-15.

MR. SUTTER: Just to show you how confused I am reading what that says and going back to the point that the secondary seal may not be working, I would conclude that you shouldn't fly.

MR. LUND: Let's go to the next chart.

MR. SUTTER: Am I confused?

MR. LUND: No, let's go to the next chart.

MR. SUTTER: Am I confused?

(Viewgraph.) [Ref. 2/14-16]

MR. LUND: So, our recommendation was that we should maintain the O-ring temperature at greater than or equal to 53 degrees Fahrenheit at launch. We didn't feel that we had the data, as Mr. Mason pointed out, to really go down below, based upon the development motor because of the putty packing problem, and our engineering recommendation was to project the ambient temperatures, or conditions, the wind and the temperature, to determine the launch time.

DR. COVERT: Mr. Lund, what is the action time on the igniter, as it gets colder?

MR. BRINTON: About 450 to 500 milliseconds.
DR. COVERT: Then it is lower, as it gets colder?
MR. LUND: No. It is longer.
DR. COVERT: That is what I mean—it spreads out.
MR. LUND: It is slower, to slightly longer

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pressure.
DR. COVERT: And the primary grain ignites with greater difficulty, I suppose?
MR. LUND: That is a fair statement.
DR. WALKER: So, could we conclude then that, in addition to the two engineers who seemed to be the best experts on the O-rings, that you also were recommending against launch?
MR. LUND: Yes, sir, at that point in time.
DR. FEYNMAN: At this point in time is the caucus, because that's what we're looking at?
MR. LUND: That is what we're looking at, the charts that were presented in the caucus.
MR. SUTTER: Didn't the chart before this say SRM-15 had blow-by?
MR. LUND: Yes.
MR. SUTTER: What does it mean that it worked?
MR. LUND: There was no violation of the primary seal, other than the instantaneous blow-by and a little bit of gas. The seal functioned.
MR. SUTTER: I thought, though, that if you're concerned that the secondary seal may have a chance at not working, you sure wouldn't want to tolerate any blow-by in the primary seal. Then how can you say it worked to meet your criteria?

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MR. LUND: I don't have a good answer to that.
MR. WAITE: You see, because when you say that secondary seal potentially can be upset by deflections, which disturbs the initial pressure check position, then there is no seal, if you have blow-by on that parameter?
MR. LUND: If you have blow-by. I think Roger said it best—every seal blows by. There is probably always some blow-by in the seating process.
VICE CHAIRMAN ARMSTRONG: That is at the low pressure?
MR. LUND: As the pressure is just rising.
VICE CHAIRMAN ARMSTRONG: If it's rising, you get a little blow-by before it seats?
MR. LUND: Yes, so "blow-by" is a qualitative thing.
MR. WAITE: The blow-by is excessive?
MR. LUND: If the blow-by is excessive, then you would have erosion and you would blow-by the O-ring.
DR. COVERT: Ron, I think we'd all better remember that we're talking about margins. There's nothing absolute about this. The real question is, as Roger put it, who's going to win the race. Is the seal going to seat before it erodes away, or is it not? And the question is how big is the margin between them?
MR. LUND: That's right.

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MR. WAITE: I think I understand the mechanism. I'm just trying to understand what they're saying.
DR. WALKER: Could I ask a question.

Do you know if there is erosion and blow-by in the Titan seals, General Kutyna, and can we get that history?

GENERAL KUTYNA: I do not know. I've asked for that history.

CHAIRMAN ROGERS: Okay.

If there are no further questions, thank you very much.

Now, Mr. Boisjoly we realize you don't have a prepared statement to give and we will have other opportunities to talk to you, I'm sure. We just want to get a preliminary statement. You've already said on the record that you did not favor the launch. Could you tell us a little bit more about your involvement and the discussions in detail?

TEMPERATURE CONCERN ON SRM JOINTS

27 JAN 1986

[Ref. 2-14-4]

HISTORY OF O-RING DAMAGE ON SRM FIELD JOINTS

Oct 30, 1985

AFT

	SRM No.	Cross Sectional View: Erosion Depth (in.)	Cross Sectional View: Perimeter Affected (deg)	Cross Sectional View: Nominal Dia. (in.)	Top View: Length Of Max Erosion (in.)	Top View: Total Heat Affected Length (in.)	Clocking Location (deg)
61A LH Center Field**	22A	None	None	0.280	None	None	36°--66°
61A LH ~~CENTER~~ FIELD**	22A	NONE	NONE	0.280	NONE	NONE	338°-18°
51C LH Forward Field**	15A	0.010	154.0	0.280	4.25	5.25	163
51C RH Center Field (prim)***	15B	0.038	130.0	0.280	12.50	58.75	354
51C RH Center Field (sec)***	15B	None	45.0	0.280	None	29.50	354
41D RH Forward Field	13B	0.028	110.0	0.280	3.00	None	275
41C LH Aft Field*	11A	None	None	0.280	None	None	--
41B LH Forward Field	10A	0.040	217.0	0.280	3.00	14.50	351
STS-2 RH Aft Field	2B	0.053	116.0	0.280	--	--	90

*Hot gas path detected in putty. Indication of heat on O-ring, but no damage.
**Soot behind primary O-ring.
***Soot behind primary O-ring, heat affected secondary O-ring.

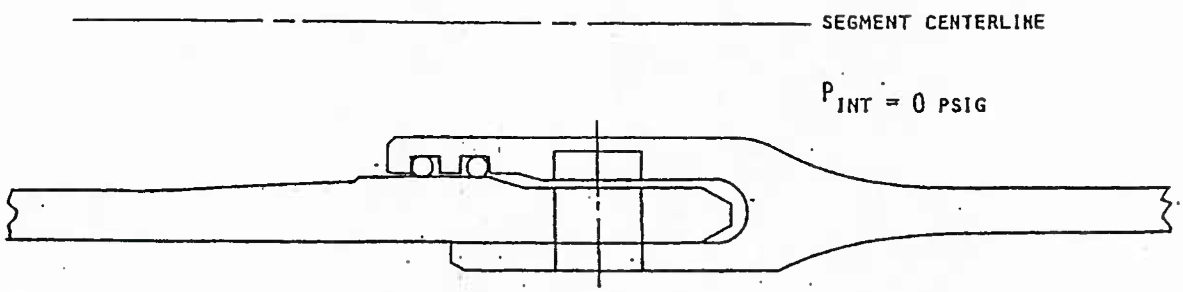
Clocking location of leak check port - 0 deg.

OTHER SRM-15 FIELD JOINTS HAD NO BLOWHOLES IN PUTTY AND NO SOOT NEAR OR BEYOND THE PRIMARY O-RING.

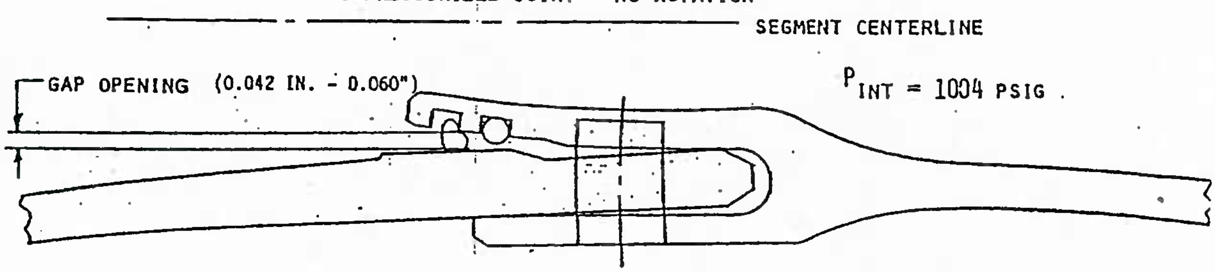
SRM-22 FORWARD FIELD JOINT HAD PUTTY PATH TO PRIMARY O-RING, BUT NO O-RING EROSION AND NO SOOT BLOWBY. OTHER SRM-22 FIELD JOINTS HAD NO BLOWHOLES IN PUTTY.

[Ref. 2/14-5]

PRIMARY CONCERNS - CONT



UNPRESSURIZED JOINT - NO ROTATION



PRESSURIZED JOINT - ROTATION EFFECT (EXAGGERATED)

[Ref. 2/14-6]

PRIMARY CONCERNS -

- FIELD JOINT - HIGHEST CONCERN
  - EROSION PENETRATION OF PRIMARY SEAL REQUIRES RELIABLE SECONDARY SEAL FOR PRESSURE INTEGRITY
    - IGNITION TRANSIENT - (0-600 MS)
      - (0-170 MS) HIGH PROBABILITY OF RELIABLE SECONDARY SEAL
      - (170-330 MS) REDUCED PROBABILITY OF RELIABLE SECONDARY SEAL
      - (330-600 MS) HIGH PROBABILITY OF NO SECONDARY SEAL CAPABILITY
  - STEADY STATE - (600 MS - 2 MINUTES)
    - IF EROSION PENETRATES PRIMARY O-RING SEAL - HIGH PROBABILITY OF NO SECONDARY SEAL CAPABILITY
      - BENCH TESTING SHOWED O-RING NOT CAPABLE OF MAINTAINING CONTACT WITH METAL PARTS GAP OPENING RATE TO MEOP
      - BENCH TESTING SHOWED CAPABILITY TO MAINTAIN O-RING CONTACT DURING INITIAL PHASE (0-170 MS) OF TRANSIENT

[Ref. 2/14-7]

IELD JOINT PRIMARY CONCERNS SRM 25

- o A TEMPERATURE LOWER THAN CURRENT DATA BASE RESULTS IN CHANGING PRIMARY O-RING SEALING TIMING FUNCTION
- o SRM 15A — 80° ARC BLACK GREASE BETWEEN O-RINGS
  SRM 15B — 110° ARC BLACK GREASE BETWEEN O-RINGS
- o LOWER O-RING SQUEEZE DUE TO LOWER TEMP
- o HIGHER O-RING SHORE HARDNESS
- o THICKER GREASE VISCOSITY
- o HIGHER O-RING PRESSURE ACTUATION TIME
- o IF ACTUATION TIME INCREASES, THRESHOLD OF SECONDARY SEAL PRESSURIZATION CAPABILITY IS APPROACHED
- o IF THRESHOLD IS REACHED THEN SECONDARY SEAL MAY NOT BE CAPABLE OF BEING PRESSURIZED

[Ref. 2/14-8]

BLOW-BY HISTORY

SRM-15 WORST BLOW-BY
- 2 CASE JOINTS (80°), (110°) ARC
- MUCH WORSE VISUALLY THAN SRM-22

SRM 22 BLOW-BY
- 2 CASE JOINTS (30-40°)

SRM-13A, 15, 16A, 18, 23A 24A
- NOZZLE BLOW-BY

[Ref. 2/14-9]

O-RING (VITON) SHORE HARDNESS VERSUS TEMPERATURE

°F	SHORE HARDNESS
70°	77
60°	81
50°	84
40°	88
30°	92
20°	94
10°	96

[Ref. 2-14-10]

SECONDARY O-RING RESILIENCY

DECOMPRESSION RATE
2"/MIN (FLIGHT ≈ 3.2"/MIN)

TEMP (°F)	TIME TO RECOVER (SEC)
50	600
75	2.4
100	*

* DID NOT SEPARATE

RIGHT DUROMETER (?)

[Ref. 2-14-11]

BLOW-BY TESTS (PRELIMINARY)

ARGON	TEMP. (°F)	RESULTS (IN^3/IN SEAL)
	75	NO LEAKAGE
	30	NO LEAKAGE
F-14		
	75	NO RESULTS YET
	30	NO RESULTS YET

Press	
0	0
400	170 ms
	330 ms

1000 psi in 0.6 sec
3 to 4 tests at each condition
.020 compression

[Ref. 2/14-12]

FIELD JOINT O-RING SQUEEZE (PRIMARY SEAL)

MOTOR	FWD	CTR	AFT
SRM 15 A	16.1 (.045)*	15.8 (.044)	14.7 (.041)
SRM 15 B	11.1 (.031)	14.0 (.039)**	16.1 (.045)
SRM 25 A	10.16 (.028)	13.22 (.037)	13.39 (.037)
SRM 25 B	13.91 (.039)	13.05 (.037)	14.25 (.040)

* 0.010" EROSION
** 0.038" EROSION

[Ref. 2/14-13]

CHART - 4

HISTORY OF O-RING TEMPERATURES
(DEGREES - F)

MOTOR	MBT	AMB	O-RING	WIND
DM-4	68	36	47	10 MPH
DM-2	76	45	52	10 MPH
QM-3	72.5	40	48	10 MPH
QM-4	76	48	51	10 MPH
SRM-15	52	64	53	10 MPH
SRM-22	77	78	75	10 MPH
SRM-25	55	26	29 27	10 MPH 25 MPH

1-D THERMAL ANALYSIS

[Ref. 2/14-14]

CONCLUSIONS:

- O TEMPERATURE OF O-RING IS NOT ONLY PARAMETER CONTROLLING BLOW-BY.

  SRM 15 WITH BLOW-BY HAD AN O-RING TEMP AT 53°F
  SRM 22 WITH BLOW-BY HAD AN O-RING TEMP AT 75°F
  FOUR DEVELOPMENT MOTORS WITH NO BLOW-BY WERE TESTED AT O-RING TEMP OF 47° TO 52°F

  DEVELOPMENT MOTORS HAD PUTTY PACKING WHICH RESULTED IN BETTER PERFORMANCE

- O AT ABOUT 50°F BLOW-BY COULD BE EXPERIENCED IN CASE JOINTS

- O TEMP FOR SRM 25 ON 1-28-86 LAUNCH WILL BE 29°F 9 AM
  38°F 2 PM

- O HAVE NO DATA THAT WOULD INDICATE SRM 25 IS DIFFERENT THAN SRM 15 OTHER THAN TEMP

[Ref. 2/14-15]

RECOMMENDATIONS :

- O-RING TEMP MUST BE $\geq$ 53 °F AT LAUNCH

  DEVELOPMENT MOTORS AT 47° TO 52°F WITH
  PUTTY PACKING HAD NO BLOW-BY
  SRM 15 (THE BEST SIMULATION) WORKED AT 53 °F

- PROJECT AMBIENT CONDITIONS (TEMP & WIND)
  TO DETERMINE LAUNCH TIME

[Ref. 2/14-16]

1198

TESTIMONY OF ROGER BOISJOLY, STRUCTURES SECTION, MORTON THIOKOL, INC.

MR. BOISJOLY: Yes.

I wrote some notes here. I have to confess that right now, my mind is basically like a sponge, with everything that has been going on after the incident and prior to the incident. So, I just had a sequence-down here, and I would just like to hit the highlights, if that's okay. [Ref. 2/14-17]

CHAIRMAN ROGERS: Yes, that's fine.

We have all been under a lot of pressure, too, so we understand that you may be tired.

But go ahead.

MR. BOISJOLY: Okay.

I first heard about the cold temperature at 1:00 p.m., the day before the launch, and we immediately, including myself——

CHAIRMAN ROGERS: Do you want to give your name?

MR. BOISJOLY: My name is Roger Boisjoly, and I'm in the Structures Section. I work directly for Jack Kapp, and I have been involved in these seals and the joints basically since I've come to work for Thiokol, which was some five and a half years ago.

1199

CHAIRMAN ROGERS: Thank you.

MR. BOISJOLY: Okay.

I first heard of the cold temperatures prior to launch at 1:00 o'clock on the day before launch, and from past experience, namely the SRM-15 launch, of which I was on the inspection team at the Cape, it just concerned me terribly.

And so we started in motion to question the feasibility of launching at such a low temperature, especially when it was going to be predicted to be colder than the SRM-15.

So we spent the rest of the day raising these questions.

DR. WALKER: And the O-ring was your concern?

MR. BOISJOLY: Yes.

I felt we were very successful up until early evening, because it culminated in the recommendation not to fly, and that was the initial conclusion. I was quite pleased with that.

I presented and prepared charts 2-1, 2-2, 2-3, 4-1, and 5-1, and basically those were the charts where I had that exaggerated view showing the O-ring in joint rotation.

There was the summary that put a probabilistic sequence on the timing of the seals, and then I prepared

1200

the chart of primary concerns.

I was basically concerned with how temperature, low temperature, affects the timing function and the ability of the seal to seal. Low temperature—and I stated this for over a year—is

away from the direction of goodness. I cannot quantify it, but I know that it is away from the direction of goodness.

I feel very strong, and I always have felt very strongly, that SRM-15 was telling us a message, and at the flight readiness review, we did not have any data to support anything but a generalized statement that said we feel that temperature was a contributor.

We did not make a major input in that. We did not make a major thing about it. But we said that we feel that temperature had something to do with that thing.

Subsequent to that, we designed a blow-by rig specifically to investigate the questions that were raised at the SRM-15 data review, subsequent to that data.

That blow-by test, as configured, was strictly configured to measure if we could put approximately a teaspoon or a tablespoon of cold gas past an O-ring at the low pressure regime of, say, 1 psi up to 50 psi, and if we

1201

could get that past, we would have explained the blow-by phenomenon, and then the timing function would have come out of that and that said it was a function of temperature because it is a delay. You are seeing hot gas indications pass, which was the visual indication between the seals in SRM-15. That was the primary crux of the whole situation.

GENERAL KUTYNA: Why did you not improve the dynamics?

MR. BOISJOLY: That was coming. But we had not got into that at that point.

On the net that night, after I presented those feelings very strong—I get very emotional about these things—and I was quite strong over the net about it, as George Hardy remembers.

Somebody brought up about SRM-22. I was not personally at the Cape, and disassembled, seeing the hardware in 22. But one of my colleagues was, a younger engineer. And I questioned him about this.

He told me that the gas blow-by that was observed on that was grey, splotchy-type blow-by, over a specific arc length, which I don't remember at the moment. I made that point that on SRM-15 we had over 100 degrees of arc, and the blow-by was absolutely jet black. It was totally intermixed in a homogeneous

1202

mixture in the grease. I attributed that to the pumping action of the joint as we were towing it back into the Cape. That is why it was totally homogeneous.

But we analyzed that chemically and found the products of combustion in it, we found the products of putty in it, we found the products of O-ring in it.

I made that point.

During the course of the evening, I also produced photos of the SRM-15, and my colleague produced photos of SRM-22. And you could visually see the difference in the amount of soot, as characterized past the O-ring seal.

I was asked then on the net to support my position with data, and I couldn't support my position with data. I had been trying to get data since October on O-ring resiliency, and I did not have it in my hand. We have had tremendous problems in trying to get a function generator and a machine to actually operate and characterize this particular pressurization function rate.

At that point, the telecon basically continued, and Mr. Lund presented his conclusions and recommendations.

So the formal part of the presentation was finished.

1203

Listeners on the other line seemed not very pleased with the recommendation. In fact, somebody asked Mr. Hardy what he thought about it, about our recommendation, and Mr. Hardy said he was appalled at MTI's decision. However, he would not go against our recommendation not to fly. If the contractor recommended not to fly, he would not go against that. He would recommend not to fly also.

There was a very short discussion that ensued, and we had, we asked for a five minute caucus. Our people asked for a five minute caucus to discuss the situation. Those opposed to launching continued to press their case with MTI management, and those opposed to the launch that pressed this case in the caucus were basically myself and Mr. Thompson. And we did everything we could to continue to try and press for not launching describing—I took the photographic position of the evidence and Mr. Thompson was trying to further elaborate on the sealing characteristics of the seals. When we realized that we basically had stopped in the discussion and we could go no further because we were getting nowhere, we backed off, both of us. We just sat back down.

GENERAL KUTYNA: What was the motivation

1204

driving those who were trying to overturn your opposition?

MR. BOISJOLY: They felt that we had not demonstrated, or I had not demonstrated, because I was the prime mover in SRM-15, because of my personal observations and involvement in the flight readiness reviews, they felt that I had not conclusively demonstrated that there was a tie-in between temperature and blow-by.

My main concern was if the timing function changed and that seal took longer to get there, then you might not have any seal left because it might be eroded before it seats. And then, if that timing function is such that it pushes you from the 170 millisecond region into the 330 second region, you might not have a secondary seal to pick up if the primary is gone. That was my major concern.

I can't quantify it. I just don't know how to quantify that. But I felt that the observations made were telling us that there was a message there telling us that temperature was a discriminator, and I couldn't get that point across.

I basically had no direct input into the final recommendation to launch and I was not polled.

I think Astronaut Crippin hit the tone of the

1205

meeting exactly right on the head when he said that, the opposite was true of the way the meetings were normally conducted. We normally have to absolutely prove beyond a shadow of a doubt that we have the ability to fly, and it seemed like we were trying to prove, have proved that we had data to prove that we couldn't fly at this time, instead of the reverse.

That was the tone of the meeting in my opinion.

CHAIRMAN ROGERS: Thank you very much—any questions?

DR. WALKER: Mr. Boisjoly, wait. I have one question.

We are still grappling with this little puff of smoke. Do you have any opinions about that?

MR. BOISJOLY: Yes, I have an opinion. But it is just an opinion, mind you.

If you get blow-by through both seals and you have erosion sufficient to do that, you may be seeing the products of the culmination of the hot gasses, the putty, the grease, the O-ring materials, blowing out the joint; but you still, because of the overwhelming amount of pressure being

forced on these seals, push the remaining part of an eroded seal into either/or the primary or the secondary.

Basically you can work this scenario both ways because if the primary seals, the secondary at some point in time, because it would have had it to be eroded

1206

too, because it would have been off of the seat, would have been off the seat, and then if you lost integrity later on in flight of the primary seal, you blow-by and fail.

If you use this scenario, that the primary seal was eroded so badly that it never sealed in the action, but there was enough of the secondary seal left and it did seat in the extrusion gap, and then subsequently later in flight, due to loads or whatever it got spit out, then you would have the same effect.

So you can do it either way, with either seal—and that you can't quantify, because you're talking about a hot gas happening with a cold gas test firing.

DR. WALKER: Do you think it is possible, by looking at the volume and duration of that smoke, to estimate just how much material was eroded?

MR. BOISJOLY: I understand that people at Marshall are trying to do just that thing.

DR. WALKER: Is there any result of that?

Could someone at Marshall comment on that?

MR. HARDY: I don't have the results. That activity is underway, was underway two days ago, when I came down here. That is being done, and we do think that is quite critical in understanding the scenario.

CHAIRMAN ROGERS: Thank you very much.

1207

DR. FEYNMAN: I just had one question, to try to understand a little more clearly what Mr. Lund showed us—maybe he should answer, I don't know—a series of graphs, of pictures that were supposedly occurring during the half hour caucus.

MR. BOISJOLY: No. That conclusion was the conclusion prior to the caucus.

DR. FEYNMAN: These were not the slides shown during the caucus?

MR. BOISJOLY: No, sir.

That whole presentation was a culmination before the caucus occurred.

MR. ACHESON: Is it practical to conduct a test with hot gas?

MR. BOISJOLY: Yes. In fact, as a result of the incident, we are going back, and one of the things we are trying to do is to take a full-scale joint and run a hot, 40 pound charged motor, and blow gas, hot gas, through it. We are also trying with cold gas to simulate the dynamics in that subscale test rate that would have the O-ring pressurized as the joint opening is added.

MR. ACHESON: Why wouldn't either or both of those variations in your test program have been instituted at the beginning of the test program?

1208

MR. BOISJOLY: I guess, in all honesty, the data that we ran in resiliency showed on that chart at room temperature showed that in a normal ambient environment, that we did not have a problem, and we had a very sufficient erosion margin.

Now, one thing that wasn't put up on that chart that we did discuss was there was one test, because I was concerned and I asked one more test be run in the time frame that those resiliency tests were run, back about eight or nine months ago, I asked that the O-ring be squeezed 40/1,000ths of an inch, and just back off 10/1,000ths of an inch. The rationale for doing that was to show what the effect of the secondary seal was at the ignition transient, beginning, namely, the

zero to 170 millisecond regime and spilling over into the next regime of 170 to 330. And that never lifted off.

That was the basis of my making that chart. I made that chart for the Washington presentation and I quantified that as best I could on the basis of that information, and I felt very comfortable with the fact that, if we were in that position of a launch that was above SRM-15, that we were okay for that region.

We had that as an experience base in our books, and that is why I was so adamantly opposed to go

1209

outside of it.

DR. WALKER: Did you present that argument at this half-hour meeting?

MR. BOISJOLY: Yes.

CHAIRMAN ROGERS: Thank you very much.

Mr. Thompson, Do you want to give your name and position?

WRITTEN 2/13/86

Summary Notes for January 27 and 28, 1986

1. The first I heard of a cold temperature et Kennedy Space Center prior to the launch wes at 1:00 p.m. on January 27, 1986. As time went on, I heard that it had been cold for several preceding days also.

2. I end others spent much of the afternoon trying to convince management not to fly in weather so cold. By now I heard that it wes predicted to get to 18° F overnight.

3. We were euccessful in convincing engineering management (Bob Lund) that we should not fly. This fact is evident from the last two charts in the presentation [FAX charts]. Bob Lund prepared these two charts during the prasantation preparation. The last two charts were conclusions and recommendations.

4. A telecon was set-up with Kennedy Space Centar and Marshall Space Flight Center in mid-evening (approximately 8:00 p.m.) to give our presentation.

5. The portions of the presentation were given by the person who was tasked with preparing that chart.

6. I gave my portion and explained that I was deeply concarned about launching due to tha low temperatura and its affect on tha O-rings. I wes asked why and I responded that the timing function of the seals to creata a seal into the extrusion gap would be affected but that I wee unable to quantify the threehold of ecceptability, except to say that it was away from goodness and our experience basa. I reminded everyone about the soot blow-by Solid Rocket Motor-15 [launched January 24, 1985 at 1:50 p.m. EST] in two field joints and how it was the coldest launch at that time and it was the first time that we had blow-by a primary seal in a field joint. A comment was then made the Solid Rocket Motor-22 [launched October 31, 1985 at 11:00 EST] elso had soot past the primary eeal and it was essentially e warm launch temperature. A conclusion was made that on this basis, temperature wes not a discriminator.

[6A moved from following page]

6A. I was asked if I had any data to support my claim. I said I have been trying to get lab deta since lest October but was not successful due to problems with the instron machine. To this answer Mr. Mason looked vary unpleased.

7. The preeentation continued, then Bob Lund gave his conclusions and recommendations. Tha listeners on the other lines were not pleased and began to question the rationale behind such a set of conclusions and recommendations. Bob Lund explained why we should etay within our data base as recommendad.

[ ] brackets denota comments added to author's notes for clarity.

Notes for January 27 and 28, 1986 (cont'd.)

8. George Herdy wae then asked by someone at either Kennedy Space Center or Marshall Space Flight Center what ha thought. He said he was eppalled at Morton Thiokol Inc.'e recommendation. He was then esked if he would fly and he said not if the contractor ie recommending not to fly.

9. A short discussion followed by various peopla. Morton Thiokol, Inc. management asked for e short five minute caucus offline and said they would get right back to them. The caucus lasted approximately 20-25 minutes.

10. The caucus started by Mr. Mason stating that a management dacision was necessary.

11. Those of us who opposed the launch were still trying to convince management that temperatura was indead a very important parameter.

12. Arnie Thompson went up and placed a pad of paper right in front of Mr. Mason on the table and attempted once again to explain the affect of a lower temperature. Arnie stopped when it became apparent that he wasn't getting through. I then epoke up again and this time made a big point about the photo evidence batween Solid Rocket Motor-15 [launched on January 24, 1985, at 1:50 p.m. EST with a 53° F O-ring temperature] and Solid Rocket Motor-22 [launched October 31, 1985 at 11:00 e.m. EST with 75° F O-ring temperatura]. I tried to axplain that Solid Rocket Motor-15 had a lot (approximataly 110 degrae arc) of black soot while Solid Rocket Motor-22 had a small (local greyish spots) amount of soot. I too stopped when it wes apparent to me that no one wanted to hear what I had to aey.

13. Sometime during tha caucus, Mr. Mason eeked if he was tha only one that was taking the position to fly. I really think that many paopla didn't hear him due to all tha discussion thet wee taking place.

[ ] breckets denote comments added to author'e notes for clarity.

Notae for January 27 and 28, 1986 (cont'd.)

14. Aftar Arnie and I had our lest eey, Mr. Mason turnad to Bob Lund and asked him to taka off his anginaering hat and put on his managamant hat.

15. Shortly after this we went beck on the telecon and meda the racommandation to leunch Solid Rockat Motor-25 without any tamperetura restriction.

16. The decision was bseicelly besad on the itams thet ware listed on a chart signed by Joe Kilminstar. This eigned sheat was raquastad by NASA.

17. I was not a perty to the contente on tha chart and in fact did not sea the chart until the naxt dey.

18. I left the room feeling badly defeetad but falt I did all I could short of baing firad. I knew that managament hed mede s tough decision but I didn't agrae with it et all.

19. Ona of my collaagues put it baet to sum it up. "This was a maeting where the determination wes to launch and it was up to us to prova beyond a shadow of doubt that it was not safe to do so." I might commant that this ie <u>not</u> the usuel tona of pra-flight meetinge. Wa ueuelly hava to <u>prove</u> beyond a shadow of doubt that it is okey to launch.

20. Rafar to notabook #2 pagas 9 and 10 for raal time antriae about faaling prior to and aftar launch.

21. My feelings ara well documantad by mamo and intarnal activity reports long befora this incident. I couldn't stand the thought of a disastar any longer so I wrota memo 2870:FY 86:073 soma time in July August of 1985. A saal task team was subeaquently formed for raal as a result of my memo -- refar to notabook #1 pege 1 for antry note.

22. I hope snd pray that I hava not risked my job and femily sacurity for baing honast in my conviction, to stend up for what is right and honorable.

Roger M. Boiejoly (signad by) — 2/13/86 4:50 p.m. EST

[ ] brackets danote commants edded to author's notas for clerity.

[Ref. 2/14-17 3 of 22]

SUMMARY NOTES FOR JAN 27 & 28 1986

1. THE FIRST I HEARD OF A COLD TEMPERATURE AT KSC PRIOR TO THE LAUNCH WAS AT 1:00 P.M. ON JAN 27, 1986. AS TIME WENT ON, I HEARD THAT IT HAD BEEN COLD FOR SEVERAL PRECEDING DAYS ALSO.
2. I AND OTHERS SPENT MUCH OF THE AFTERNOON TRYING TO CONVINCE MANAGEMENT NOT TO FLY IN WEATHER SO COLD. BY NOW I HEARD THAT IT WAS PREDICTED TO GET TO 18°F OVERNIGHT.
3. WE WERE SUCCESSFUL IN CONVINCING ENGINEERING MANAGEMENT (BOB LUND) THAT WE SHOULD NOT FLY. THIS FACT IS EVIDENT FROM THE LAST TWO CHARTS IN THE PRESENTATION. BOB LUND PREPARED THESE TWO CHARTS DURING THE PRESENTATION PREPARATION. THE LAST TWO CHARTS WERE CONCLUSIONS AND RECOMMENDATIONS
4. A TELECON WAS SET-UP WITH KSC AND MSFC IN MID EVENING (APPROX 8:00 P.M.) TO GIVE OUR PRESENTATION.
5. THE PORTIONS OF THE PRESENTATION WERE GIVEN BY THE PERSON WHO WAS TASKED WITH PREPARING THAT CHART.
6. I GAVE MY PORTION AND EXPLAINED THAT I WAS DEEPLY CONCERNED ABOUT LAUNCHING DUE TO THE LOW TEMPERATURE AND ITS AFFECT ON THE O-RINGS. I WAS ASKED WHY AND I RESPONDED THAT THE TIMING FUNCTION OF THE SEALS TO CREATE A SEAL INTO THE EXTRUSION GAP WOULD BE AFFECTED BUT THAT I WAS UNABLE TO QUANTIFY THE THRESHOLD OF ACCEPTABILITY, EXCEPT TO SAY THAT IT WAS AWAY FROM GOODNESS AND OUR EXPERIENCE BASE. I REMINDED EVERYONE ABOUT THE SOOT BLOW-BY SRM-15 IN TWO FIELD JOINTS AND HOW IT WAS THE COLDEST LAUNCH AT THAT TIME AND IT WAS THE FIRST TIME THAT WE HAD BLOW-BY A PRIMARY SEAL IN A FIELD JOINT. A COMMENT WAS THEN MADE THAT SRM-22 ALSO HAD SOOT PAST THE PRIMARY SEAL AND IT WAS ESSENTIALLY A WARM LAUNCH TEMPERATURE. A CONCLUSION WAS MADE THAT ON THIS BASIS, TEMPERATURE WAS NOT A DISCRIMINATOR

(GA. -EXT PG) 7. THE PRESENTATION CONTINUED, THEN BOB LUND GAVE HIS CONCLUSIONS AND RECOMMENDATIONS. THE LISTENERS ON THE OTHER LINES WERE NOT PLEASED AND BEGAN TO QUESTION

[Ref. 2/14-17 4 of 22]

THE RATIONALE BEHIND SUCH A SET OF CONCLUSIONS AND RECOMMENDATIONS. JOE KILMINSTER EXPLAINED WHY WE SHOULD STAY WITHIN OUR DATA BASE AS RECOMMENDED.

8. GEORGE HARDY WAS THEN ASKED BY SOMEONE AT EITHER KSC OR MSFC WHAT HE THOUGHT. HE SAID HE WAS APPALLED AT MTI'S RECOMMENDATION. HE WAS THEN ASKED IF HE WOULD FLY AND HE SAID NOT IF THE CONTRACTOR IS RECOMMENDING NOT TO FLY.

9. A SHORT DISCUSSION FOLLOWED BY VARIOUS PEOPLE. MTI MANAGEMENT ASKED FOR A SHORT 5 MINUTE CAUCUS OFFLINE AND SAID THEY WOULD GET RIGHT BACK TO THEM. THE CAUCUS LASTED APPROX 20-25 MINUTES

10. THE CAUCUS STARTED BY MR MASON STATING THAT A MANAGEMENT DECISION WAS NECESSARY.

11. THOSE OF US WHO OPPOSED THE LAUNCH WERE STILL TRYING TO CONVINCE MANAGEMENT THAT TEMPERATURE WAS INDEED A VERY IMPORTANT PARAMETER.

4. I WAS ASKED IF I HAD ANY DATA TO SUPPORT MY CLAIM. I SAID I HAVE BEEN TRYING TO GET LAB DATA SINCE LAST OCT BUT WAS NOT SUCCESSFUL DUE TO PROBLEMS WITH THE INSTRON MACHINE. TO THIS ANSWER MR MASON LOOKED VERY UNPLEASED.

2. ARNIE THOMPSON WENT UP AND PLACED A PAD OF PAPER RIGHT IN FRONT OF MR MASON ON THE TABLE AND ATTEMPTED ONE AGAIN TO EXPLAIN THE AFFECT OF A LOWER TEMPERATURE. ARNIE STOPPED WHEN IT BECAME APPARENT THAT HE WASN'T GETTING THROUGH. I THEN SPOKE UP AGAIN AND THIS TIME MADE A BIG POINT ABOUT THE PHOTO EVIDENCE BETWEEN SRM-15 AND SRM-22. I TRIED TO EXPLAIN THAT SRM-15 HAD A LOT (≈110° ARC) OF BLACK SOOT WHILE SRM-22 HAD A SMALL (LOCAL GREYISH SPOTS) AMOUNT OF SOOT. I TOO STOPPED WHEN IT WAS APPARENT TO ME THAT NO ONE WANTED TO HEAR WHAT I HAD TO SAY.

SOMETIME DURING THE CAUCUS, MR MASON ASKED IF HE WAS THE ONLY ONE THAT WAS TAKING THE POSITION TO FLY. I REALLY THINK THAT MANY PEOPLE DIDN'T HEAR HIM DUE TO ALL THE DISCUSSION THAT WAS TAKING PLACE.

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14. AFTER ARN. AND I HAD OUR LAST AT, MR MASON TURNED TO BOB LUND AND ASKED HIM TO TAKE OFF HIS ENGINEERING HAT AND PUT ON HIS MANAGEMENT HAT.

15. SHORTLY AFTER THIS WE WENT BACK IN THE TELECON AND MADE THE RECOMMENDATION TO LAUNCH SRM-25 WITHOUT ANY TEMPERATURE RESTRICTION.

16. THE DECISION WAS BASICALLY BASED ON THE ITEMS THAT WERE LISTED ON A CHART SIGNED BY JOE KILMINSTER. THIS SIGNED SHEET WAS REQUESTED BY NASA

17. I WAS NOT A PARTY TO THE CONTENTS ON THE CHART AND IN FACT DID NOT SEE THE CHART UNTIL THE NEXT DAY

18. I LEFT THE ROOM FEELING BADLY DEFEATED BUT FELT I DID ALL I COULD SHORT OF BEING FIRED. I KNEW THAT MANAGEMENT HAD MADE A TOUGH DECISION BUT I DIDN'T AGREE WITH IT AT ALL.

19. ONE OF MY COLLEGUES PUT IT BEST TO SUM IT UP. "THIS WAS A MEETING WHERE THE DETERMINATION WAS TO LAUNCH AND IT WAS UP TO US TO PROVE BEYOND A SHADOW OF DOUBT THAT IT WAS NOT SAFE TO DO SO." I MIGHT COMMENT THAT THIS IS NOT THE USUAL TONE OF PRE-FLIGHT MEETINGS. WE USUALLY HAVE TO PROVE BEYOND A SHADOW OF DOUBT THAT IT IS OKAY TO LAUNCH.

20. REFER TO NOTEBOOK #2 PAGES 9 AND 10 FOR REAL TIME ENTRIES ABOUT FEELING PRIOR TO AND AFTER LAUNCH.

21. MY FEELINGS ARE WELL DOCUMENTED BY MEMO AND INTERNAL ACTIVITY REPORTS LONG BEFORE THIS INCIDENT. I COULDN'T STAND THE THOUGHT OF A [redacted] DISASTER ANY LONGER SO I WROTE MEMO 2870:FY86:073 SOMETIME IN ~~JULY~~ AUG OF 1985. A SEAL TASK TEAM WAS SUBSEQUENTLY FORMED FOR REAL AS A RESULT OF MY MEMO — REFER TO NOTEBOOK #1 PAGE 1 FOR ENTRY NOTE.

22. I HOPE AND PRAY THAT I HAVE NOT RISKED MY JOB AND FAMILY SECURITY FOR BEING HONEST IN MY CONVICTION, TO STAND UP FOR WHAT IS RIGHT AND HONORABLE.

Roger M Boisjoly 2/13/86
4:50 PM
EST

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[Ref. 2/14-17 6 of 22]

Typed Version of Handwritten Notes of R. Boisjoly, Seal Task Force, Structures, Morton Thiokol, Inc.

January 27, 1986, 8:00 a.m. Meeting

- 65-6A vented putty with the double O-ring is planned to be fired today. This is a 402B motor with test section.
- Gave status on fluorocarbon seals.
- Looked out the window after the meeting and could still see the STA-1 segments in the storage yard. They still are not shipped to H-7.

January 27, 1986 Information

- Received a handwritten quote and non-dimensioned sketch of the fluorocarbon seal concept.
- Held a serious meeting concerning the launch of Solid Rocket Motor-25 [launch planned for January 28th] since it is so cold at the cape. After much discussion from 1:00 p.m. to 8:30 p.m. the engineering recommendation was to delay the flight until the seal was at 53° F to stay within our data base. NASA management did not like our recommendations and our management promptly caucused for further discussion. This resulted in our management (Jerry Mason) making the decision that it was a low risk based upon their assumption that temperature was not a discriminator since Solid Rocket Motor-22 [launched October 31, 1985 at 11:00 EST] had hot gas passage at essentially at room temperature. We (engineering) tried once again to impress that the timing of the event due to temperature was the problem. My thinking is that temperature is a discriminator and Solid Rocket Motor-15 [launched January 24, 1985 at 1:50 p.m. EST] and Solid Rocket Motor-22 [launched October 31, 1985 at 11:00 a.m. EST] do demonstrate this fact. Solid Rocket Motor-15 had a large order of more soot between the O-rings than Solid Rocket Motor-22 and this tells me that temperature is important and that the data does exist to lead us to our engineering recommendation. I sincerely hope that this launch does not result in a catrophy [catastrophe]. I personally do not agree with some of the statements made in Joe Kilminster's written summary stating that Solid Rocket Motor-25 is okay to fly.

[end of page 9 of notes]

[ ] brackets denote comments added to author's notes for clarity.

January 28, 1986, 8:00 a.m. Meeting - Meeting ended 9:00 a.m.

- The O-ring resiliency testing (or lack of testing) was discussed. It was suggested that we structure all our testing with an outside vendor (Parker Seal in Salt Lake) since our lab is not responding to our needs. We have been trying to get testing started since last October. I will investigate this with Arnie [Thompson].
- Also check to see where Tom [Gregory] is on the design of the seal dynamic test fixture. If no progress has been made then consider having Dave Sparkman designing the system to give us the opening rate speed to simulate the field joint parameters.
- One more comment about the Solid Rocket Motor-25 meeting that took place yesterday: This is the first time that I am aware of that NASA has taken a position that is outside of the program data base. I have had many arguments negated by them because they have insisted that my suggestions were not within the data base up to that time. It seems to me that they only allowed the data base to grow as a result of unforeseen conditions. At no time would they allow the data base to be expended by predetermination of a set of conditions. I remember well, the fights I had at the Solid Rocket Motor-16 [launched April 29, 1985] Flight Readiness Review when presenting the Solid Rocket Motor-15 [launched January 24, 1985] results. I was hammered hard about being outside the previous data base since Solid Rocket Motor-15 was and still is the worst hot gas passage indication past the primary O-ring seals.

January 28, 1986 Information

(added 2/9/86)
(51 Launch 11:38 a.m.)

- Solid Rocket Motor-25 blew up approximately one minute into the flight. Presently waiting for information on the cause of the disaster. I feel real sick about this but I did everthing possible to convince them not to fly.

[end of page 10 of notes]

[ ] brackets denote comments added to author's notes for clarity.

DRAWING/CALCULATION SHEET

PREPARED BY	DATE		PAGE
R. BOISJOLY	1/23/86	MORTON THIOKOL INC	1 of 1
CHECKED BY	DATE	DESCRIPTION	REPORT NO.
APPROVED BY	DATE	SRM-25 FIELD JOINTS	MODEL NO.

BASED ON MIN O-RING DIA (.277) STRETCH = 0.0057 (2.56%)
CMPSET = .0020
0.0077" SAY .008"

ASSUME NOMINAL O-RING IN JOINT FOR TEMP PURPOSES
O-RING DIA = .280"
$(DIA)_{nom}$ = 144.149 + .280 = 144.429" @ ℄ OF O-RING
$L = \pi D = \pi(144.429) = 453.737$"
$\alpha \cong -9.286 \times 10^{-5}$ in/in/°F
$\Delta L = \alpha L \Delta T \cong -9.286E{-5}(453.737)(75-25) = -2.11$"
% STRETCH = $\frac{2.11}{453.737-2.11} \times 100 = .467\%$
OR
$(ID)_{nom} = \frac{453.737-2.11}{\pi} - .280 = 143.477$" — CHECK OK
% STRETCH = $\frac{144.149-143.477}{143.477} = .468\%$

1. RED (0-3%) = $0.01 + 1.06X - .1X^2$
$= 0.01 + 1.06(.468) - .1(.468)^2$
$= .484\%$

RED IN X-SECT:
$\frac{.484}{100}(.280) = 0.0014$"

OR

% STRETCH TOTAL = .467 + 2.56 = 3.027%

Y-RED (3-25%) = $0.56 + .59X - 0.0046X^2$
$= 0.56 + .59(3.027) - 0.0046(3.027)^2$
$= 2.304\%$

RED IN X-SECT:
$\frac{2.304}{100}(.280) = 0.0065$
$\Delta = .0065 - .0057 = .0008$"

∴ USE $\Delta D = 0.0014$"

$\Delta D = \alpha D \Delta T \cong -9.286E{-5}(.280)(75-25) = -0.0013$"

TOTAL CHANGE IN DIA DUE TO ΔT = 50°F } $\Delta D = 0.0014 + 0.0013 = 0.0027$" SAY 0.003" REDUCTION

— THIS ASSUMES THAT THE LAUNCH TEMPERATURE IS 25°F.
∴ PRESSURE ACTUATION OF JOINT IS STILL WITHIN MIN SQUEEZE CRITERIA IF MIN SQUEEZE ON FIELD JOINTS IS 0.023" AT R.T.

— THE REDUCTION IN LAUNCH TEMP FROM THE PREVIOUS LOW OF 51°F IS AWAY FROM GOODNESS CONCERNING THE TIMING FUNCTION TO SEAL THE PRIMARY O-RING. I WOULD NOT BE ONE BIT SURPRISED TO FIND MUCH SOOT BEHIND THE FIELD JOINT PRIMARY O-RINGS THAT HAVE A HOT GAS JET AT IGNITION, SIMILAR TO SRM-15 LAUNCHED 1/85.

[Ref. 2/14-17 9 of 22]

SRM SEAL EROSION TEAM MINUTES.

AN ATTEMPT TO FORM THE TEAM WAS MADE ON 19 JULY. THIS ATTEMPT VIRTUALLY FAILED AND RESULTED IN MY WRITING MEMO 2870:FY86:073. THIS MEMO FINALLY GOT SOME RESPONSE AND THE TEAM WAS FORMED OFFICIALLY. THE FIRST MEETING WAS HELD ON 8/15/85 @ 2:30. THE TEAM LEADER IS DON KETNER

8/15/85 MINUTES (2:30 P.M.)

- PHIL SHADOCESKY GIVEN ACTION TO COMPILE ALL S.C.P. HOT TEST DATA INTO A REPORT
- DON ASKED PHIL TO ASSIGN SOMEONE TO DO THE HEAT XFER ANALYSIS IN THE SHORT SAKE.
- SCOTT ASSIGNED TO FOLLOW TEST HARDWARE TO SEE THAT IT IS BEING BUILT AND ASSEMBLED — STARTING WITH 6.1.3.8
- MARK TO DEFINE S.C.P. TESTS TO RUN TO OBTAIN EROSION INFORMATION BETWEEN .125" AND .195" IN SUBSCALE TEST RIG. ALSO, DEFINE A COLD & HOT TEST TO SIMULATE CAVITY UPSTREAM OF SECONDARY O-RING (FACE SEAL) IN NOZZLE JOINT

8/19/85 MINUTES (8:00 A.M.)

TEAM ASSIGNMENT DON, SCOTT, BRIAN, PHIL, ROGER, KARL, MARK
TEAM ASSIGNMENTS:

100% • BRIAN — FOLLOW ALL PUTTY CONCEPTS AND RETURN SEARCH AND MSFC COORDINATION
100% • ROGER — STRUCT. EVALUATION — COORD ACTIVITIES IN ADHESIVE GROUP HELP WRITE TEST PLANS & REVIEW TEST DATA. DESIGN CONCEPTS INPUT & REVIEW. ESTABLISH DATA SEARCH FORMAT FOR QUALITY SEARCH
100% • SCOTT — P.E. ACTIVITIES — GET ALL TESTS UNDER CONTROL. FOLLOW HARDWARE, DESIGN AND TESTING
5-50% • PHIL — COORD ALL H.T. ANALYSIS
100% • MARK — COORD ALL GAS DYNAMICS ANALYSIS

[Ref. 2/14-17 10 of 22]

JAN 27, 1986 8:00 A.M. MTG

- 6.5-GA [illegible] WITH THE [illegible] O-RING IS PLANNED TO BE FIRED TODAY. THIS IS A 40LB MOTOR WITH TEST SECTION
- GAVE STATUS ON FLUOROCARBON SEALS
- LOOKED OUT THE WINDOW AFTER THE MTG AND COULD STILL THE STA-1 SEGMENTS IN THE STORAGE YARD. THEY STILL ARE NOT SHIPPED TO H-7

JAN 27, 1986 INFORMATION

- RECEIVED A HANDWRITTEN QUOTE AND NON-DIMENSIONED SKETCH OF THE FLUOROCARBON SEAL CONCEPT.
- HELD A SERIOUS MEETING CONCERNING THE LAUNCH OF SRM-25 SINCE IT IS SO COLD AT THE CAPE. AFTER MUCH DISCUSSION FROM 1:00PM TO 8:30PM THE ENGINEERING RECOMMENDATION WAS TO DELAY THE FLIGHT UNTIL THE SEAL WAS AT 53°F TO STAY WITHIN OUR DATA BASE. NASA MANAGEMENT ~~WAS~~ DID NOT LIKE OUR RECOMMENDATIONS AND OUR MANAGEMENT PROMPTLY CAUCUSED FOR FURTHER DISCUSSION. THIS RESULTED IN OUR MANAGEMENT (JERRY MASON) MAKING THE DECISION THAT IT WAS A LOW RISK BASED UPON THEIR ASSUMPTION THAT TEMPERATURE WAS NOT A DISCRIMINATOR SINCE SRM 22 HAD HOT GAS PASSAGE AT ESSENTIALLY AT ROOM TEMPERATURE. WE (ENGINEERING) TRIED ONCE AGAIN TO IMPRESS THAT THE TIMING OF THE EVENT DUE TO TEMPERATURE WAS THE PROBLEM. MY THINKING IS THAT TEMPERATURE IS A DISCRIMINATOR AND SRM 15 AND SRM 22 DO DEMONSTRATE THIS FACT. SRM 15 HAD A LARGE ORDER OF MORE SOOT BETWEEN THE O-RINGS THAN SRM 22. AND THIS TELLS ME THAT TEMPERATURE IS IMPORTANT AND THAT THE DATA DOES EXIST TO LEAD US TO OUR ENGINEERING RECOMMENDATION. I SINCERELY HOPE THAT THIS LAUNCH DOES NOT RESULT IN A CATASTROPHY. I PERSONALLY DO NOT AGREE WITH SOME OF THE STATEMENTS MADE IN JOE KILMINSTER'S WRITTEN SUMMARY STATING THAT SRM-25 IS OKAY TO FLY.

[Ref. 2/14-17 11 of 22]

JAN 28, 1986 8:00 A.M. MTG: MTG ENDED 9:00 A.M.

- THE O-RING-RESILIENCY TESTING (OR LACK OF TESTING) WAS DISCUSSED. IT WAS SUGGESTED THAT WE ~~[illegible]~~ STRUCTURE ALL OUR TESTING WITH AN OUTSIDE VENDOR (PARKER SEAL IN SALT LAKE) SINCE OUR LAB IS NOT RESPONDING TO OUR NEEDS. WE HAVE BEEN TRYING TO GET TESTING STARTED SINCE LAST OCTOBER. I WILL INVESTIGATE THIS WITH ARNIE.
- ALSO CHECK TO SEE WHERE TOM IS ON THE DESIGN OF THE SEAL DYNAMIC TEST FIXTURE. IF NO PROGRESS HAS BEEN MADE THEN CONSIDER HAVING DAVE SPARKMAN DESIGNING THE SYSTEM TO GIVE US THE OPENING RATE SPEED TO SIMULATE THE FIELD JOINT PARAMETERS.
- ONE MORE COMMENT ABOUT THE SRM-25 MEETING THAT TOOK PLACE YESTERDAY: THIS IS THE FIRST TIME THAT I AM AWARE OF THAT NASA HAS TAKEN A POSITION THAT IS OUTSIDE OF THE PROGRAM DATA BASE. I HAVE HAD MANY ARGUMENTS NEGATED BY THEM BECAUSE THEY HAVE INSISTED THAT MY SUGGESTIONS WERE NOT WITHIN THE DATABASE UP TO THAT TIME. IT SEEMS TO ME THAT THEY ONLY ALLOWED THE DATA BASE TO GROW AS A RESULT OF UNFORESEEN CONDITIONS. AT NO TIME WOULD THEY ALLOW THE DATABASE TO BE EXPANDED BY PREDETERMINATION OF A SET OF CONDITIONS. I REMEMBER WELL, THE FIGHTS I HAD AT THE SRM-16 FRR WHEN PRESENTING THE SRM-15 RESULTS. I WAS HAMMERED HARD ABOUT BEING OUTSIDE THE PREVIOUS DATA BASE SINCE SRM-15 WAS AND STILL IS THE WORST HOT GAS PASSAGE INDICATION PAST THE PRIMARY O-RING SEALS.

JAN 28, 1986 INFORMATION. ADDED 2/9/86 51L LAUNCH 11:38 A.M.

SRM-25 BLEW UP APPROX 1 MINUTE INTO THE FLIGHT. PRESENTLY WAITING FOR INFORMATION ON THE CAUSE OF THE DISASTER. I FEEL REAL SICK ABOUT THIS BUT I DID EVERYTHING POSSIBLE TO CONVINCE THEM NOT TO FLY.

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[Ref. 2/14-17 12 of 22]

FEB 14, 1986 INFORMATION CONT'D

THE FOLLOWING OUTLINE WAS USED BY ME WHEN I TESTIFIED BEFORE THE PRESIDENTIAL COMMISSION TODAY

THIS IS THE ACTUAL OUTLINE I USED WHEN I TESTIFIED BEFORE THE PRESIDENTIAL COMMISSION ON FEB 14, 1986

Roger M. Boisjoly
2/14/86

NOTE: THE ITEM CROSSED OUT WERE CROSSED OUT AND CORRECTED BEFORE I TESTIFIED. THESE NOTES WERE A SHORT SUMMARY MADE FROM THE NOTES ON THE LAST 3 PAGES OF THIS NOTEBOOK IN ACCORDANCE WITH THE ADVICE FROM MORTON THIOKOL'S ATTORNEY THE NIGHT BEFORE,

OUTLINE USED FOR 2/14/86 MEETING WITH
ROGER M. BOISJOLY PRESIDENTIAL COMMISSION

1. FIRST HEARD OF COLD TEMP 11:00 AM JAN 27 COLD FOR SEVERAL DAYS BEFORE
2. SPENT REST OF DAY RAISING QUESTIONS ABOUT LAUNCHING DUE TO LOW TEMP
3. WAS SUCCESSFUL UP TO EARLY EVENING — RESULTED IN INITIAL CONCLUSIONS & RECOMMENDATIONS
4. PRESENTED MY 8 CHARTS IN TELECON AND EXPLAINED
   a) I PREPARED AND PRESENTED CHARTS — (2-1)(2-2)(2-3)(4-1)(5-1)
   a) VERY CONCERN ABOUT LOW TEMP
   b) LOW TEMP AFFECTS SEALING TIMING FUNCTION
   c) LOW TEMP AWAY FROM GOODNESS AND EXPERIENCE BASE
   d) DISCUSSED SRM-15 SOOT BLOW-BY — LOWEST LAUNCH TEMP
   e) SOMEONE COMMENTED ABOUT SRM-22 LAUNCHED AT 75°F
   f) I WAS ASKED FOR DATA TO SUPPORT MY POSITION — NONE YET — TRYING TO GET SINCE OCT
5. TELECON CONTINUED ~~[illegible]~~ ENDED WITH CONCLUSIONS & RECOMMENDATIONS BY MCLUND —
6. LISTENERS ON OTHER LINES NOT PLEASED WITH RECOMMENDATION
7. MR MACOY WAS ASKED WHAT HE THOUGHT — HE SAID HE WAS APPALLED AT MTI'S DECISION, HOWEVER HE WOULD NOT GO AGAINST MTI'S RECOMMENDATION.
8. SHORT DISCUSSION — THEN A 5 MIN CAUCUS CALLED FOR BY MTI
9. THOSE OPPOSED TO LAUNCHING CONTINUED TO PRESS THEIR CASE TO MTI MANAGEMENT
10. ARNIE THOMPSON AND I STILL VIGOROUSLY OPPOSED THE LAUNCH AND CONTINUED TO PRESENT OUR CASE,
11. DISCUSSED THE SRM-15 & SRM-22 PHOTOS AS MY EVIDENCE TO BACK MY POSITION.
12. THE MANAGEMENT CONCLUSION WAS THAT THE DATA WAS NOT CONCLUSIVE SO ARNIE & ME BACKED OFF
13. I HAD NO DIRECT INPUT TO THE FINAL RECOMMENDATION TO LAUNCH
14. BOB CRIPPENS COMMENT WAS VERY APPROPRIATE IN ASSESSING THE MTG

[Ref. 2/14-17 13 of 22]

MORTON THIOKOL INC.
Wasatch Division

Interoffice Memo

2871:FY86:141
22 August 1985

TO: S.R. Stein, Project Engineer

CC: J.R. Kapp, K.M. Sperry, B.G. Russell, R.V. Ebeling, H.H. McIntosh, R.M. Boisjoly, M. Salita D.M. Ketner

FROM: A.R. Thompson, Supervisor Structures Design

SUBJECT: SRM Flight Seal Recommendation

The O-ring seal problem has lately become acute . Solutions, both long and short term are being sought, in the mean time flights are continuing. It is my recommendation that a near term solution be incorporated for flights following STS-27 which is currently scheduled for 24 August 1985. The near term solution uses the maximum possible shim thickness and a .292 +.005/-.003 inch dia O-ring. The results of these two changes are shown in Table 1. A great deal of effort will be required to incorporate these changes. However, as shown in the Table the O-ring squeeze is nearly doubled for the example (STS-27A). A best effort should be made to include a max shim kit and the .292 dia O-ring as soon as is practical. Much of the initial blow-by during O-ring sealing is controlled by O-ring squeeze. Also note sacrificial O-ring material is available to protect the sealed portion of the O-ring. The added cross-sectional area of the .292 dia O-ring will help the resilience response by added pressure from the groove side wall.

Several long term solutions look good; but, several years are required to incorporate some of them. The simple short term measures should be taken to reduce flight risks.

Arnold R. Thompson
A.R. Thompson

ART/jh

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Distribution
Page 2
20 August 1985

The other identified participants who will serve at the direction of the chairman on an as needed basis are:

- Karl Eckhardt, Insulation Design
- Stan Boraas, Gas Dynamics
- Phil Shadleskey, Heat Transfer
- Eldon Bailey, Manufacturing Engineering
- Fred Brasfield, Sr., Quality Engineering
- Tom Gregory, Space Booster Project Engineering

It is the charter of this task force to investigate the SRM case and nozzle joints, both materials and configurations, and recommend both short term and long term solutions.

The task force is specifically chartered to organize and direct the required investigations, tests, and analyses, and will act in the roll of Project Engineers to direct activities in supporting organizations.

We will appreciate your continued support of this activity.

R. K. Lund

A. J. McDonald

[Ref. 2/14-17 19 of 22]

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22 July 1985

PROGRESS REPORT

APPLIED MECHANICS DEPARTMENT
Cost Center 287x

SPACE SHUTTLE

SRM O-Ring Erosion Problem

This problem has escalated so badly in the eyes of everyone, especially our customer, NASA, that NASA has gone to our competitors on a propriety basis and solicited their experiences on their joint configurations.

This whole week has been spent concepting ideas on how to eliminate the O-ring erosion problem. The new ground rule is to present every idea regardless of impact to cost weight schedule or whatever. Eleven hours of just group meetings has been spent discussing the problem and potential solutions. This does not include the many hours of informal meetings held with smaller groups.

One thing is increasingly obvious as time passes on this problem. If the company and/or Engineering does not assign specific people to this task (with no other work allowed - this being an absolute requirement) to secure a timely solution, then we stand in danger of having one of our competitors solve our problem via an unsolicited proposal. This thought is almost as horrifying as having a flight failure before a solution is implemented to prevent O-ring erosion.

Roger M Boisjoly
7/22/85

[Ref. 2/14-17 20 of 22]

COMPANY PRIVATE

MORTON THIOKOL INC.

Wasatch Division

Interoffice Memo

31 July 1985
2870:FY86:073

TO: R. K. Lund
Vice President, Engineering

CC: B. C. Brinton, A. J. McDonald, L. H. Sayer, J. R. Kapp

FROM: R. M. Boisjoly
Applied Mechanics - Ext. 3525

SUBJECT: SRM O-Ring Erosion/Potential Failure Criticality

This letter is written to insure that management is fully aware of the seriousness of the current O-Ring erosion problem in the SRM joints from an engineering standpoint.

The mistakenly accepted position on the joint problem was to fly without fear of failure and to run a series of design evaluations which would ultimately lead to a solution or at least a significant reduction of the erosion problem. This position is now drastically changed as a result of the SRM 16A nozzle joint erosion which eroded a secondary O-Ring with the primary O-Ring never sealing.

If the same scenario should occur in a field joint (and it could), then it is a jump ball as to the success or failure of the joint because the secondary O-Ring cannot respond to the clevis opening rate and may not be capable of pressurization. The result would be a catastrophe of the highest order - loss of human life.

An unofficial team (a memo defining the team and its purpose was never published) with leader was formed on 19 July 1985 and was tasked with solving the problem for both the short and long term. This unofficial team is essentially nonexistent at this time. In my opinion, the team must be officially given the responsibility and the authority to execute the work that needs to be done on a non-interference basis (full time assignment until completed).

COMPANY PRIVATE

[Ref. 2/14-17 21 of 22]

R. K. Lund 31 July 1985

It is my honest and very real fear that if we do not take immediate action to dedicate a team to solve the problem, with the field joint having the number one priority, then we stand in jeopardy of losing a flight along with all the launch pad facilities.

Roger M Boisjoly

R. M. Boisjoly

Concurred by:

Jack R Kapp

J. R. Kapp, Manger
Applied Mechanics

COMPANY PRIVATE

[Ref. 2/14-17 22 of 22]

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TESTIMONY OF ARNOLD THOMPSON, DESIGN ENGINEER, MORTON THIOKOL, INC.

MR. THOMPSON: Arnold Thompson, Design Engineer, in the case design structural engineer.

CHAIRMAN ROGERS: The comments I made to Mr. Boisjoly apply, of course, to you. We know that.

You are not expected to make a presentation, but go ahead.

MR. THOMPSON: I understand.

I need to start out first by "Amending" what Mr. Boisjoly has said. His thoughts parallel mine very closely.

I would like to add some more features to this.

As many of you know—and I think it answers some questions that were presented during the course of this morning, and initially this case was thought to not open up. You've seen the pictures where the gap opens up, and the secondary and primary, and the analysis revealed that it, in fact, was not supposed to open up. After running some hydroproof, and, in fact, the first attempt at a burst pressure of the standard weight case, it leaked. In fact, it leaked at about 1,300 - 1,400 psi.

We ran some additional analysis, and

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re-boundaried the pins, and it turns out that the pins and how you boundary those is very, very critical to the response of this opening up that we are talking about.

CHAIRMAN ROGERS: Could you give the dates of when you ran those—roughly?

MR. THOMPSON: Two or three years ago.

DR. WALKER: What do you mean, "boundaried the pins?"

MR. THOMPSON: The boundary conditions of how you follow the load lines through the pins indicates if you're modeling the axisymmetric case that we smear the pins out in it and give the stiffness appropriate to make it approximate the case.

Now, if the pin doesn't follow the load line, that affects very much the results.

DR. COVERT: In other words, they cannot make the model, the math model, they cannot grid it up with these 177 pins? They have to model it some other way? Is that correct?

MR. THOMPSON: We're now modeling with a three-dimensional analysis to try to attempt to follow the load line better. And so we didn't know it was going to open up. And it, in fact, did. And we have shown that, and by remodeling in some, we were able to get 10/1,000ths, and then, by that time, the STA-1 came on the scene. That is where we first got our first

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definitive measured data that opened up on the order of about 42/1,000ths. And I think the MEOP at that time was 936.

MR. BOISJOLY: That was the static. Not "STS-1"; STA-1.

MR. THOMPSON: STA-1. Thank you.

VICE CHAIRMAN ARMSTRONG: When you said it leaked, did it leak water?

MR. THOMPSON: It didn't leak. It opened up. And we had one pressure port that was down at the bottom. We put into it a pressure transducer so that we could follow the motion of it.

VICE CHAIRMAN ARMSTRONG: You said you had some tests leak at 1,300 - 1,400 psi?

MR. THOMPSON: Yes. We ran a hydroburst because it leaked very badly at that point.

VICE CHAIRMAN ARMSTRONG: And that was water?

MR. THOMPSON: Yes, it was water. It leaked so badly, we just shut the test down.

But that gave us a very, very close to our safety factor, our 1.4 safety factor, with 936, so that was sufficient at the time.

Later on, we ran the STA-1 test.

MR. SUTTER: I didn't understand your last statement.

You said you ran a test and you got a load and it was very close to 1.4.

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Is that good or bad?

MR. THOMPSON: We have been asked to produce a 1.4 factor of safety and have a positive margin.

MR. SUTTER: But the load you got in this test was how close to that 1.4?

MR. THOMPSON: We were at, I don't remember all the numbers exactly, but it was 1385 psi, and it was 936. So it would be 936 times 1.4, would be the required number, and the 1,300 number came very close to that.

DR. WALKER: Pretty much at the 1.4?

MR. THOMPSON: Yes, very close.

DR. COVERT: 1.4 if I did it right, is 1320, and I'm not good at arithmetic.

MR. THOMPSON: But I think we were just slightly below the 1.4. And so, we proceeded in doing better modeling. And then, of course, the light-weight motor came in, where we took about 41/1,000ths off the stiffeners and attachers, and about 20/1,000ths off the cylindrical wall. And we then ran a test with those light weight cases. And we ran a verification where we attempted, once again, to measure the joint motion. Because, as you can see, it is very critical in the operation of this motor. And we, because of some bracketry problems on that, we got the measurements

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that were somewhat high, we tried but the brackets were rotating, too, and we tried to go back and correct for that. But it was somewhat high.

And then subsequently we have run a bunch of tests where we have confirmed what we feel good about, on the order of 42/1,000ths between the sealing diameters at 1004 psi. So these are kind of new things that have come on the scene.

We have tried to adjust for these things.

We put shims—first of all, we adjusted the tolerances. We looked at the tolerances of the case, the O-ring groove depth, and so forth, and we found that most of the population was within tighter tolerances. So we changed the drawing.

We found out that we could, by twisting some arms with the O-ring people, we could reduce the tolerances on the O-rings also, and so we did that.

And then, the next thing we did, we put shims in on the outside to force the tang over against the clevis, and these are some of the things that we have been doing in an attempt to counteract that, what you are seeing.

DR. WALKER: That was to increase the squeeze on the O-ring?

MR. THOMPSON: Increase the squeeze. It has

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two squeezes. One is when it is just sitting there, static, with the various pre-launch loads on it, and so forth. That is what we call a gathering squeeze. That, on a worst, is like 33/1,000ths. And then, once the cases are pressurized, they go around, but not necessarily concentric, and so that can get as bad as 8/1,000ths.

So we are really better off pressurizing.

But the issue that we're speaking of today is that of the opening and the rate of opening.

All of you I think are familiar with the pressure trace. You recall that it has kind of a toe on it, and then it comes up over the inflection point, and then it has a saturated curve on top.

Well, of course, the movement of this gland or the separation of this gland is very close to that same response.

I would like to speak first to the primary O-ring.

The primary O-ring has lots of energy to activate it and it probably does it very quickly. But it is kind of dependent, as Roger has said, on the stiffness of the material.

If you look at the relaxation data for rubber at cold temperatures and very high rates of loading, it

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has a very stiff response, versus a higher response at higher temperatures. And we were loading on a fairly fast response. So we are in a transition range of this elastic material, and it has to flow. And if it has to flow, it has to deflect, and the deflection would be dependent upon the stiffness of the rubber.

If it is colder, it will move slower. Before an O-ring can seal, it has to go into the squeeze, into the extrusion gap. It has to fill in the little pores in the metal, in fact, even the lay of the lathe turnings, as it is turned in the machine.

Until it does that, you're going to get some blow-by.

There is just nothing that you can do about it until it is finally extruded into the gap.

So we have two things that are happening. One would have to translate a little bit. That's only on the order of 20/1,000ths across the O-ring groove, maybe less. And then it has to form into the O-ring groove. Until such time, you are probably going to get some flow; at least you can't guarantee that you're not.

Now, on this particular motor, we know that when we launched with the O-ring temperature predicted to be 53 degrees, we had considerable blow-by. It was blackened for about 180 degrees.

1217

Now we're proposing—and I'm thinking of the next day now—of launching at 29 degrees, and it was quite a ways out of our experience realm, as has been said many times today.

Now, let's talk about the secondary O-ring. I started to talk about it a minute ago.

The departure of those two surfaces is very similar in shape to the pressure trace. In the beginning, during the pressure buildup, and during the separation of these two things, it is rather slow. On a comparative rate, it is on the order of, maybe, about 1.2 inches per minute. And then it increases as you get to the inflection point.

It is as high as 10 inches per minute.

Then, when you go over the top, it backs off to 1 or 1.5 inches per minute.

The reason we talk about inches per minute is because we are going to use an instron to do some work, and that is the way those people talk—rather, in inches per second.

What we had done, and the data we had available to us that night, was two inches per minute. Now, if we take a secant from time zero to the max pressure of 1,000 psi in 600 milliseconds, that secant is about 3.2 inches per minute. That was shown on the chart.

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We tested at 2 inches per minute, because that is how instrons work, generally in decades of two, and at two inches per minute the resiliency of the O-ring is not sufficient to follow that velocity at room temperature. It will separate. But it won't separate until in the low velocity sections. So you've probably got, as has been mentioned here today, somewhere between 100 and 125 milliseconds while you're in the low velocity movement of this gland. And in that time you do have a secondary O-ring, as Roger indicated in the bracketing of various sequences during the rise in pressure. But in the early parts, you do have a secondary O-ring, and in the latter parts you do not.

Now, what has been alluded to earlier. We have devised a machine now where we can control the instron to give us exactly the shape, and we're doing that work now. In fact, a lot of that work has been done—which confirms much of the things that we're discussing here.

So, I guess, in a final summary, without talking too much, my view was that we were now launching at a lower temperature; we were going to get more blow-by; at 53 degrees we had considerable blow-by and now we're going to get more of it; and after the first hundred milliseconds, 125 milliseconds, we do not have a

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secondary O-ring that is functional.

MR. RUMMEL: I have a question for clarification.

As I recall, the wall of the motor, I distinctly recall it was reduced in thickness during the course of this program. That would tend to increase the spread in the joint, would it not?

MR. THOMPSON: Very little. It is on the order of 1-2 mils and I'm speaking now from finite element analysis, after we had learned to boundary it, so that we could predict the correct STA-1 structural test article results. We used that type of boundary conditions and applied it to both wall thicknesses. It was very small. It was on the order of a few mils of additional opening.

MR. RUMMEL: That was actually tested? Or this is predicted?

MR. THOMPSON: Those were predicted numbers.

MR. SUTTER: These improvements resulted in tighter tolerances in the shims? Were they in some of the flight vehicles?

MR. THOMPSON: I was trying to remember if they were, in fact, in all of the flight vehicles. I think shims were. It's about a 30/1,000th shim that take up some of that slop.

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MR. SUTTER: Do you know when they were introduced—which flights?

MR. THOMPSON: I don't know.

Maybe Mr. Boisjoly recalls.

MR. MASON: Those were right from the first flight.

MR. SUTTER: So these were improvements from the beginning, and the joint basically has been the same?

MR. MASON: Yes.

DR. WALKER: Did you consider using a larger O-ring?

MR. THOMPSON: You can go to about a 292 O-ring, without getting—and then, of course, you have to have a tolerance on that of plus 5, minus 3. And anything above a 292 with plus 5 on it, and if you go to the wall on the land, that is all that will fit in. We have considered a 295. In fact, we have assembled shells with a 295 in hardware, after firing the DM-1, our first static firing, and they went together fine.

DR. WALKER: Why didn't you go to the larger O-ring, then?

MR. THOMPSON: One problem in going to larger O-rings is in field joints—plant joints, excuse me. In the plant joints, if you put in the 295 and you take

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the worst on worst, when the joint is raised to a temperature of 325 degrees during the curing of the insulation, it is an overfill condition because of the alpha problems with the case, and the rubber.

DR. WALKER: There is no reason why a field joint and a plant joint had to have the same O-ring, is there?

MR. THOMPSON: There were some QC people that were afraid of the confusion that might be developed between two nearly the same sized O-rings.

CHAIRMAN ROGERS: A couple of questions, if I may.

Was any consideration given to the possibility of water? I notice you said earlier on that there were some experiments with water. Any consideration given at this time to the possibility of water being dangerous?

It had been raining a good deal prior to this launch.

MR. THOMPSON: Well, it had been raining, and also we know that at one time we disassembled a motor when we had to make a nozzle change; and when we pulled out the pins, water came out. And we know that water does get in there, and we know that the temperatures most likely were below freezing. So there was a chance

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of some ice.

I'm not sure that was a consideration that night, though.

CHAIRMAN ROGERS: Were there other engineers that would take issue with you on this matter, when it came to the launch question of whether to go or no go? Did you have others who opposed your point of view?

MR. THOMPSON: No, sir.

I have 24, 25 people, gals and guys, working for me, and I know none of them that would have opposed this viewpoint that are involved in the case, the case/nozzle joints.

CHAIRMAN ROGERS: All of those people would have said no to the launch?

MR. THOMPSON: My judgment is yes, that is true.

MR. ACHESON: Question, and maybe this question runs against the engineering culture. But what I'm wondering is when people run test programs that show these significant anomalies, some think related and some think maybe not related to temperature, why wouldn't MTI say to NASA we have some tests going on that show some inconclusive, but troubling, results, and we're not sure we know what to think about it. But until we decide what to think about it, you ought to be damn sure you

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don't launch below X temperature, and that is an order. I mean, you are to accept that with our product.

Do contractors ever talk that way to the customers? If not, why not?

I don't quite see why this spirit of scientific inquiry with neutral and anti-conclusory culture surrounding it should be allowed to continue so long when the worst case is pretty obvious and it had pretty bad results.

Of course, we don't know yet that this was the case, but if you believe that it could have been a catastrophic category in the worst case, when your test results were troubling, but not scientifically proven, I really don't understand why the customer isn't told to hold everything below some threshold condition.

MR. MASON: Sir, I probably should address that.

MR. ACHESON: And I don't mean just this launch.

MR. MASON: I understand. It's more of a policy issue, so I should probably address it, instead of Arnie. Let me try to do that.

I believe that we have provided full visibility of the testing, the analysis, and there has been a joint continuing review with NASA on the whole

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question of the joint, and on every flight we have assessed that issue, looking at our most recent history or our total history, to see if there would be a concern on the subsequent flight.

We were really trying to acquire enough information to be able to identify if there was a threshold.

I think the fundamental answer to your question is we didn't know what the threshold was, we knew we needed more test data, and we are getting that so that the threshold could be identified.

We were looking at both what the capability of the existing joint was as well as at the same time looking for improvements.

While I've got the floor, we have in the motor that is about to be static tested, we are testing larger O-rings. That is one of the candidate improvements. And I will add to that that a lot of you already know that when you have something that is working and you have to contemplate a change, you have to be absolutely certain that the change is, in fact, an improvement; because the history of the business is that if you aren't very, very careful, what you think is an improvement may end up being worse. It has happened a number of times.

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So I know there is concern about the time that we have taken to look at these improvements. But you have to be certain that they are improvements before you make the change.

CHAIRMAN ROGERS: I would like to go to a little more specific point. I mean, I think Dave's point is a good one. But the thing that is most troubling to me is if you had Mr. Thompson, who is a supervisor, and he says all of his people had doubts about whether this launch should take place, and it has been a matter of major concern all of this time, and you have evidence from the flight a year ago that there was blow-by, what was it that caused you to go ahead and approve it?

MR. MASON: Well, let's see. First off, to say that all of his people didn't even know that there was an issue, and so, I mean, it is really conjecture to determine what their position would have been. But, as far as what motivated us, it was logic said that 53 degrees is not a threshold.

If you ask anybody would 50 degrees be okay, I think everyone would sign up at 50 degrees. So, every flight which the program has had, has had to break some frontiers.

But when you go from STS-1 to STS-2, you went

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into a temperature regime that you weren't in, and as you move down through the program, you are working to some degree in an extrapolated area.

DR. RIDE: The time you go through frontiers is during testing, not during the flights. That's the way it's supposed to work.

MR. MASON: Well, "demonstration" is a better word, at the demonstration. You fall on your analysis when you make your first flight, and your second flight you're falling on your analysis. You have not demonstrated everything that you're going to see.

I hope that is clear.

I'm trying to say that when we qualify in the first place, we don't test the full range of every piece of item, and we therefore take some data and extrapolate it by analysis.

So we don't have actually demonstrated capability on every facet of the motor.

DR. WALKER: We are talking about 25 degrees away from your experience.

MR. MASON: And that is certainly the reason that it had the extensive debate that it did.

DR. WALKER: And just recently, apparently, the previous launch was a situation where something was 2 degrees out of spec and the decision not to proceed might

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have averted a catastrophe. So 25 degrees out of your experience is really rather a large extrapolation.

MR. ACHESON: It seems to me not a case where you are trying to set a threshold, but a case in which you have established a point which is 53 degrees, and everybody, as you say, would agree that's all right. And you've got some people who say anything substantially outside of that is troublesome and anything colder than that is more troublesome. And we think we see a parallelism between increasing coldness and increasing unreliability. That is the state of mind at MTI.

What I really don't quite see is why everybody took it so calmly.

MR. MASON: Well, it was not calmly nor was there a lack of concern.

We were trying to, we listened to all of the arguments, and we found ourselves in a position of some uncertainty that we were not able to quantify. And we had, as opposed to the 53 degree joint that had a blow-by, we had five joints that did not in that very same flight. And so, we, on a statistical basis, we were working with random occurrences. And taking a couple of random occurrences and trying to say there's a clear correlation didn't hold up.

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Our intuition and, of course, some data, says the colder it is, the more risk there is. The real issue was how much colder before you run into a regime where you will get three times the erosion of the worst one we had seen. It wasn't as if what we had seen before was on the verge of failure. It was not. The blow-by, by itself, was not a failure mode. It has to lead to erosion. And the worst erosion we had seen was a third of what we were tolerating.

So our first thought was that, even if we had more blow-by, and it led to erosion, it would have to be three times as much as anything we had seen, and that seemed extremely unlikely.

MR. ACHESON: It seems to a layman that it comes down to a point of view that says the burden of proof is on the people who want to launch, this cold; and another point of view says the burden of proof is on the people who want to stop the launch. Then nobody can really decide how to resolve that. And so, it just got resolved either by persuasion or by some presumption in favor of launching or—I don't know how.

But I don't see either a mathematical or a logical process by which it got resolved.

MR. MASON: Well, certainly since the incident we have been searching our minds and our souls on the

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question of did we address it properly. You know everyone has said if I could have stopped it, I wish I had.

But here we are, trying to present the thought process that we went through that day, rather than what we could do over.

CHAIRMAN ROGERS: Well, I think everybody is sympathetic, and we understand. The problem that I'm having is when you talk about frontiers, I think we all understand that. I mean, this whole program has had a lot of frontiers, and we understand. But it doesn't seem to me that you were saying to yourself now we want to test this equipment because we're going to establish a new frontier.

It seems to me that the question was should we delay this launch or scrub it. And so, you really weren't thinking about a new frontier.

MR. MASON: That was a poor choice of words on my part.

What I was trying to convey was that we are always working somewhat in the extrapolated area.

CHAIRMAN ROGERS: But that is not this case.

I mean, here you have a lot of warning, and you've all been discussing the O-rings and seals, and you've all had concerns, and it's all over the papers.

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All you have to do is look at it, as we have, and you can see that's been a major concern. And now you have a situation where there is a lot of concern and a lot of your people expressed a negative vote, and that our people in NASA don't even know about it.

I mean, that is unbelievable to me. At least, if there were a calculated decision and everybody said yes, we all know about it and we're willing to take the risk, and there is some risk, and I suppose you have to do that. Every flight has some risk. But this one seems to be so difficult to explain. It must be difficult to ask about it, as Bob said.

Well, do we have any other questions?

MR. REINARTZ: Mr. Chairman, I just wanted to ask Mr. Thompson one question.

Arnie, had you said during the end of your discussion that after 100 to 125 milliseconds you do not have a functional secondary O-ring?

MR. THOMPSON: Yes, sir.

MR. SUTTER: I have one question.

I'm really confused as to whether the secondary seal was considered by anybody to be effective or not. But the statement that was used states if the primary seal does not seat, the secondary seal will seat. I think that is a key issue.

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What the hell is going on there—and was this criteria that was used—did the people know that stated that criteria—did they know this? And why didn't MTI and NASA have better communications on that very critical item?

I just don't understand it. And I have listened for two hours and I still don't understand it.

I think I hear two different things here.

It's sort of a hell of a way to run a railroad on a critical item like this.

CHAIRMAN ROGERS: Well, shall we go to the next item on the agenda? I guess that's all for today.

MR. McDONALD: Since I caused this meeting to come about, I would like to testify, I guess.

CHAIRMAN ROGERS: Surely.

VICE CHAIRMAN ARMSTRONG: I was just going to say that other Thiokol people might want to testify and should be given the opportunity.

CHAIRMAN ROGERS: Well, we have a little scheduling problem. Everybody will have an opportunity, but I think you should particularly have an opportunity, Al, and we will assure you you will all have an opportunity later on.

We do have a schedule we want to try to keep to. We're a little out of sync already. But, please,

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go ahead.

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TESTIMONY OF AL McDONALD, MORTON THIOKOL, INC.

MR. McDONALD: Since I was very involved in getting the issue brought to a head through the telecon and from the concerns at the plant, and I was at the meeting for Thiokol at Kennedy, since I was the senior member at Kennedy from Thiokol for this launch, as the presentation was being made by the members of Thiokol, there were some comments that were made, I think, that influenced some of the decisions that were made later.

Besides the comment being made that NASA was appalled by our recommendation, but they said they wouldn't fly without our concurrence, Mr. Mulloy jumped in and said that you guys are trying to establish new launch commit criteria, and you can't do that on the spur of the moment; those are predetermined constraints. I think that influenced some of the thinking.

In addition, Mr. Reinartz turned to me and said your 53 degree temperature recommendation isn't consistent with what I understand is the qualification temperature for the rocket motor, which I believe is 40 to 90 degrees, isn't it? And I told him yes, I think it is 40 to 90 degrees, but that the recommendation in this particular case was being made on our experience base,

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and that the temperatures that were predicted were well below that.

I was very supportive of the decision that was made by the plant initially at 53 degrees, and, while they were off-line, reevaluating or reassessing this data, because the chart that Mr. Boisjoly had made that I was looking at, and I presented from last August, which was my grave concern about the whole situation, I got into a dialogue with the NASA people about such things as qualification and launch commit criteria.

The comment I made was it is my understanding that the motor was supposedly qualified to 40 to 90 degrees.

I've only been on the program less than three years, but I don't believe it was. I don't believe that all of those systems, elements, and subsystems were qualified to that temperature.

And Mr. Mulloy said well, 40 degrees is propellant mean bulk temperature, and we're well within that. That is a requirement. We're at 55 degrees for that—and that the other elements can be below that; that, as long as we don't fall out of the propellant mean bulk temperature. I told him I thought that was assinine because you could expose that large solid rocket motor to extremely low temperatures—I don't

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care if it's 100 below zero for several hours—with that massive amount of propellant, which is a great insulator, and not change that propellant mean bulk temperature but only a few degrees, and I don't think the spec really meant that.

But that was my interpretation because I had been working quite a bit on the filament wound case solid rocket motor. It was my impression that the qualification temperature was 40 to 90, and I knew everything wasn't qualified to that temperature, in my opinion that we were trying to qualify the case itself at 40 to 90 degrees for the filament wound case.

I then said I may be naive about what generates launch commit criteria, but it was my impression that launch commit criteria was based upon whatever the lowest temperature, or whatever loads, or whatever environment was imposed on any element or subsystem of the shuttle. And if you are operating outside of those, no matter which one it was, then you had violated some launch commit criteria.

That was my impression of what that was. And I still didn't understand how NASA could accept a recommendation to fly below 40 degrees. I could see why they took issue with the 53, but I could never see how they would take or accept a recommendation below 40

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degrees, even though I didn't agree that the motor wasn't fully qualified to 40. I made the statement that if we're wrong and something goes wrong on this flight, I wouldn't want to have to be the person to stand up in front of board of inquiry and say that I went ahead and told them to go ahead and fly this thing outside what the motor was qualified to.

I made that very statement.

I was still very upset because when they came back on the line and said that we, Thiokol, would go ahead and fly after the caucus, I was bothered enough, because I believe, and Roger and I—and I have worked with him, I believe the same as he does—that I wanted to have one more reconsideration. I asked the folks at NASA for one more reconsideration not to fly. In fact, I said, can we fly in the afternoon? It was my impression when I came down here that the original launch schedule was 3:45 in the afternoon. It seems like it's an available window. According to the weather report I heard, it was going to be 48 to 50 degrees in the late afternoon.

The comment was made that, well, that was considered but there was some problem with one of the transatlantic abort landing sites, with bad visibility in late afternoon. So then I said well, if you don't accept

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the discussion we had as a good enough reason not to fly, there are three good reasons not to fly, and those three, together, ought to be a good enough reason not to fly.

And I said we just discussed the O-ring as I left Titusville, from Mr. Carver Kennedy's house, who happens to be our Vice President of the Space Services for LSOC here. I was staying at his house. He had just gotten a report from the recovery ships. The recovery ships reported to him that they were in an absolute survival mode, that they were headed toward shore, and had been for some time, there was 30 foot seas, winds of 50 knots sustained, gusting to 70 knots, they were pitching 30 degrees. They even thought they may have done some damage to some of the retrieval equipment on the back of the ship. They were doing about three knots toward shore and they would not be in the recovery area to support the launch in the morning. And they did not dare even turn around or try to turn because of the tremendous seas.

And I said you know, that is, in my opinion, putting the boosters at risk, and I think we ought to remember also that this flight is the very first flight that we were going to sever the exit cone at apogee from the solid rocket motors, and it also was the very first

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flight that we were going to sever the parachutes from the boosters on impact. And in my opinion, we were putting the recovery, the boosters, at risk. But we were certainly just throwing

away the parachutes, the frustums. You would never find those because the recovery ships will never get back in the area. And I think that is an important consideration.

The third consideration that we ought to consider is I know damn well in the morning there's going to be ice all around that place, and water, because we've got this sound suppression system and I don't understand how it all works. But I do know that maybe we ought to, maybe it will change the acoustics on the vehicles, the structures, or debris. I don't know. But it seems to me that that is another unknown that we shouldn't be delving into.

I was told that, you know, these are not your concerns. And I said well, I am concerned about all of these, and I think those combined should be absolute criteria not to launch this thing because if I were the launch director, I wouldn't do it.

That is what I told them.

Well, I was waiting for the fax to come back because when the final recommendation was made by Thiokol to fly, they were told to put that in writing.

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I told Mr. Mulloy I wouldn't sign that, it would have to come from the plant because normally I am responsible for telling whether the flight goes or not.

And so, I had deep concerns about that.

So I was told to stay and wait for the signed fax to come in, to deliver it to the attendees at the meeting here, and that is when we had a lot of this discussion.

So I believe the NASA people that were here decided that there were enough concerns that they would at least pass those on in an advisory capacity, as I was fairly emotional about it. And I went. They asked me where the fax was because it was just like the five minute caucus. The fax took another half hour or more.

And Mr. Houston told me where it was at. It was at the other end of the building. And they asked me where it was at, and so I left the end of the building to get the signed fax. And I waited. I wondered if the machine wasn't working or not. And I waited for it. And it took a while. And it finally came through.

I came back, and I believe they were in Mr. Houston's office or something. And they were on a telecon. I believe it was with Arnie Aldrich, but I'm not sure. I thought somebody said Arnie, and I heard them discussing the concerns and problems with the

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recovery ships, and the survival mode at sea. And the discussion went along these lines, that the conclusion was that the ships were heading toward shore and would be way out of the launch area, and could not support a morning launch, and that if a decision were made to launch, it would have to be made on the basis that the recovery ships would not be in the area. And the problem associated with that Mr. Mulloy recognized there was a fairly high probability that because of that, we would lose the parachutes and the frustums, and he was asked, I believe, what the value of all that was. He gave some value like $660,000, where the parachutes and frustums—it was someplace, I guess, of around a million dollars of hardware.

He was asked if he could continue to support the flight schedule if we lost that much hardware, and he said yes, that he had sufficient. And then he was asked about whether we were putting the boosters at risk, and he said that he didn't think there was any significant risk of putting the boosters at risk, that we did have airplanes in the area and they've got beepers and things, and we could get them later.

And he was told well, for darn sure, don't have the ships try to turn around in the condition they're at, to go back there, and risk the ships, to be

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in the area. They must continue on their course back in.

And then the next discussion that came up that I heard part of was about the water system and the freezing and the ice. I heard some comment basically that that was considered earlier and discussed. But I didn't hear anything about the O-ring discussion, and I presumed that that discussion happened while I was down at the fax machine, waiting for the fax to come in. And the gentlemen did make it clear that they were acting in an advisory capacity only, to make sure he was aware of this information.

And, at that time, I had given them, I went to Jack Buchanan's office and had copies of it made, of the fax that was signed, and gave it to them. And then I went home that night.

CHAIRMAN ROGERS: Thank you very much.

Any questions?

MR. ALDRICH: I would like to comment on that phone call.

I do not remember that with relation to this incident. There was a phone call to me that night of the status of the launch facility with respect to the temperatures and the discussion about the recovery ships not being able to hold station, and being well off station; and they headed into the weather, which was

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taking them away from their home position—was discussed.

My first question was if it was safe for them to be out there at all for some other procedure than with respect to the ships themselves and the people on board. We should be concerned with their safety. And I was told that the ships had dual engines and that they were considered safe, but they couldn't guarantee to be in the position the following day.

We discussed a delay in recovering the rockets and the discussion proceeded that we would lose some of the equipment, but probably not the rockets themselves. And I determined at that time that that was not cause to call off the launch or to not proceed with the tanking.

The other discussion I had was about the facility, and it had to do with the earlier sessions we had, that is, that the temperatures were cold, they were proceeding slowly, more slowly than normal, and that there was expected to be ice on the facility, perhaps there already was ice on the facility.

That was the total content of that discussion, and there was no inference of threat to flight hardware, other than the potential for some difficulty or delay in recovering the solid rocket boosters.

The following morning, I went to the Control

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Center, as I have described previously. I spent a large amount of time dealing with the question of ice on the launch facility and its implications and concerns for it.

At that time, I also checked on the status of the recovery ships. In fact, the seas offshore had subsided, and it was reported to me that it would not be a problem, that they would be in the proper position for launch.

CHAIRMAN ROGERS: Are there any questions?

DR. WALKER: I have a question. It is a question that I would like perhaps to have a response to later on.

I would like to understand how you go about developing criteria for launch, and if these always are initiated within NASA or whether the contractor can initiate things, what the proce-

dure is, and, in particular, why there was not a criteria for the O-ring seals, as opposed to the bulk temperature of the propellant?

MR. ALDRICH: We will clearly have to present that. The process is that each project develops for their equipment and hardware with that contractor. It is submitted to a review at the integrated project level and is approved up the line, and what we have in place is what has been entered into that project review and

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approved.

CHAIRMAN ROGERS: Okay.

I would like us, if there is no objection, I would like to recess the meeting now, and I think the Commission should consider the testimony that we have heard this morning very carefully and then decide our next step.

We will be in touch with all of you as we make some decisions.

DR. FEYNMAN: Sir, don't you think it is necessary to get all of the testimony from the Thiokol people that are here now, who wish to, that is, who wish to make a statement?

CHAIRMAN ROGERS: Well, I think—obviously, as far as the Chairman is concerned, we are prepared to listen to anybody that wants to testify. We are going to have to have a lot of testimony. We can't take it all today.

It may be if any Commission members or anyone else want to say something, fine. But we have had a pretty full morning, and there is a lot of material to digest, and there are a lot of questions to be asked and a lot of answers to be given. I guess I'm just saying that I'm not sure that, unless there is any particular——

DR. WHEELON: Mr. Chairman, may I propose a

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procedural thing, and that is that we turn individually to each one of the Thiokol people and establish today whether they have additional information that they can provide whenever available or pass at this point—just to get an explicit disclaimer from each one of them that they have nothing to give?

CHAIRMAN ROGERS: That's a good idea.

Does anybody from Thiokol want to add anything, either in this session or to the Committee staff?

Dr. Keel will be here and his assistants will be here. If you have anything you want to pass on, fine. If you want to say anything now, fine. But we do assure you that you will all have a chance to be heard more fully at a later date. We're not sure what that later date will be, but we will decide that pretty soon.

MR. ACHESON: Could I ask, for the record, what the requirements and constraints are presently governing the preservation of Thiokol records of this launch, and the system hardware and testing preceding this mission?

Is there an order outstanding or an impoundment?

MR. MASON: We have been instructed to impound all of the data having to do with the launch and have

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done so. Much of it has been copied and sent to Marshall as part of the process, but it is all impounded in one area.

MR. ACHESON: It will physically be at Marshall?

MR. MASON: Right now it's impounded at the plant and we're sending copies of whatever anyone wants for the investigation.

CHAIRMAN ROGERS: Is there any material in NASA on this general subject matter that we haven't seen?

MR. HARDY: Not that I'm aware of.

MR. ACHESON: How broadly does the impoundment order run?

MR. MASON: It covers anything to do with this specific launch, and the complete history of the motor, the fabrication and anything to do with that motor.

MR. ACHESON: Including test procedures and test records?

MR. MASON: Where they are applicable to that motor.

MR. ACHESON: Do you mean the SRB type or do you mean just the segments involved in this launch?

MR. MASON: Just the segments involved, the data for this specific launch is what is impounded right now.

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DR. WALKER: I think our question would extend to all of the data, such as the test data, which is described on the O-rings; all data referring to tests concerning the operation of the O-rings, particularly their response to temperature. All of those data should be made available to the Commission.

MR. HARRINGTON: I'm Jim Harrington, Secretary of Jess Moore's Task Force. We, the day of the accident, instituted our contingency plans, which are covered under NASA documents. Our first action was to impound all data.

And so we instructed all of the NASA centers, and they, in turn, instructed all of the contractors to impound all data pertinent to the incident.

Now, specifically, some data that you talked about is test data run years ago, and may not have been covered by impoundment because it doesn't relate specifically to the incident.

MR. ACHESON: Well, isn't it clear that we want eventually to look at all of the data relating to the O-ring problem, at least, regardless of what sections it affected?

DR. WALKER: Could we request that NASA ask the contractors to preserve all such data and make it available?

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MR. ALDRICH: Yes, sir.

We'll take that action.

DR. RIDE: I guess I would also like to ask the people from Marshall why they decided not to advise Mr. Aldrich of Thiokol's concerns?

MR. REINARTZ: I think, Sally, I would be glad to go through it again, but I think I indicated earlier through the basis for the decision not to bring that up as being a Level III item that did not violate waivers or constraints to the launch, and that was the basis for not going ahead and bringing it up at that time.

CHAIRMAN ROGERS: So, you have nothing to add to what you have testified?

MR. REINARTZ: No, sir.

DR. KEEL: Mr. Chairman, could I just ask one question, just not to embarrass anyone or put anyone on the spot at all, but just for the sake of establishing the record, so it is not left to inference?

The inference, Mr. McDonald, from your testimony is that you were under pressure, perhaps unusual pressure from NASA officials, to go ahead with the launch. Is that an accurate inference or not?

MR. McDONALD: That is an accurate inference, yes.

CHAIRMAN ROGERS: And did I understand, too,

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that you did not sign off on this one?

MR. McDONALD: No, I did not.

CHAIRMAN ROGERS: Was that unusual?

MR. McDONALD: I believe it was.

Yes.

CHAIRMAN ROGERS: Thank you.

Let's adjourn for lunch.

Thank you very much.

[Whereupon, at 12:35 p.m., the meeting was adjourned.]

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PRESIDENTIAL COMMISSION ON THE SPACE SHUTTLE CHALLENGER ACCIDENT—TUESDAY, FEBRUARY 25, 1986

Dean Acheson Auditorium
Department of State
Washington, D.C.
The Commission met, pursuant to recess, at 9:40 a.m.
PRESENT:
WILLIAM P. ROGERS, Chairman, Presiding
NEIL A. ARMSTRONG, Vice Chairman
DR. SALLY RIDE
DR. ARTHUR WALKER
DAVID C. ACHESON
DR. RICHARD FEYNMAN
MAJOR GENERAL DONALD KUTYNA
DR. EUGENE COVERT
ROBERT HOTZ
JOSEPH SUTTER
ROBERT RUMMEL
ALSO PRESENT:
AL KEEL, Commission Executive Director

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PROCEEDINGS

CHAIRMAN ROGERS: The Commission will come to order, please.

Today the Commission will focus on the events leading to the decision to launch the Challenger. The Commission has already reviewed a good deal of information about the seals and the O-rings on the solid rocket boosters. It should be noted, however, that it is not yet clear that the joint area was the originating problem.

Therefore, it is important that all potential causes of the accident, including the external tank, be actively pursued.

In our sessions today and tomorrow, we want to present in a thoughtful and orderly manner the facts relating to the decision to launch the Challenger. Because of its importance I will ask witnesses to identify the time and place of any event that they are addressing and the names and positions of persons who participated.

The Commission wants to be fair in the presentation of the facts because the subject matter may involve possible human error, as distinguished from equipment failure. The Commission will attempt to give a right of reply as soon as possible to any person who

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believes he has been unfairly criticized or whose actions may have been inaccurately portrayed.

During the last two weeks, the Commission has encouraged NASA to disclose a large number of facts and documents relating to the launch, which has been done. As a result, we believe the public is better able to understand and assess many aspects of the accident. We hope that this process will continue until all the facts are fully known and as much as possible fully understood.

While the Commission has the responsibility under its mandate from the President to investigate the accident and report its findings, the media plays a key role in the process by keeping the public informed. We believe it has performed this role well and with a high sense of responsibility.

If the Commission effectively performs its duties and the media performs its role of accurately reporting the facts as they develop, the public will be well served.

Witnesses who appear today will be able to comment on or clarify their testimony, as long as the substance is not altered. Of course, any additional evidence or additional material that might assist the Commission in the performance of its duties and

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responsibilities will be welcome.

The first witness this morning will be Allen McDonald. Following Mr. McDonald and at the request of Morton Thiokol, the Commission will next hear Jerry Mason, who is executive vice president of Morton Thiokol.

Now I would like to call upon our Executive Director, Dr. Keel, who will make a few comments before we hear Mr. McDonald.

DR. KEEL: Mr. Chairman, for the benefit of the Commission we have put together a chronology of events relating to the period when the first temperature concerns were raised with respect to the Challenger, indicating the activities and also the participants as the Commission understands them, based upon testimony and documentation provided to the Commission.

This chronology has been provided to all of the witnesses, Mr. Chairman, so they will have an opportunity to understand our reconstruction of those events and to clarify them, and also, as appropriate, to identify the nature of the discussions at each of these meetings and activities, starting approximately from the scrub of the originally planned launch of Challenger for January 27th at 9:38 a.m., ranging up to the launch of the Challenger on January 28th at 11:38 a.m.

CHAIRMAN ROGERS: And if there are corrections

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that have to be made on this document, we will make them as we go along. It is as accurate as we can make it at the present time.

Now I would like to call on Mr. McDonald, please. Mr. McDonald, I think probably if you take the middle chair it might be best. And I think to be consistent, let's swear all the witnesses in again.

(Witness sworn.)

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TESTIMONY OF ALLEN McDONALD, MANAGER, SRM PROJECT, MORTON-THIOKOL, INC.

CHAIRMAN ROGERS: Mr. McDonald, you're now employed by Morton Thiokol, Inc.?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: And how long have you been employed by them?

MR. McDONALD: I've been with Morton Thiokol a little over 26 years.

CHAIRMAN ROGERS: And you're an engineer?

MR. McDONALD: Yes. I have a bachelor's degree in chemical engineering from Montana State University and a master's degree in engineering administration from the University of Utah.

CHAIRMAN ROGERS: And can you give us some history of your employment with Thiokol?

MR. McDONALD: I am currently the director of the space shuttle solid rocket motor project. My responsibilities are for technical, cost, and schedule performance of the steel case motors that are flown out of Kennedy, and also the development and qualification of the filament wound case solid rocket motor to be flown out on Vandenberg Air Force Base.

I have had this position for a little less than two years. I started as director in March of 1984

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of the shuttle SRM project. Prior to that, I was the manager of the project engineering division at Morton Thiokol, at which I had technical responsibility from a project engineering standpoint for all of the programs in the plant, with the exception of the space shuttle.

That included the Peacekeeper, the MX stage one, Trident 1 C-4 production, Trident 2 development, the HAARM Mark 104 standard missile, MD-2, the improved performance space motor 2. I finished a contract on qualification of a long life motor for SHRAM. I had all of the independent research and development, advanced technology programs with the rocket propulsion laboratory, and also some work on air bag, air cushion restraint systems.

I had that job I guess for about three, four years prior to coming into the shuttle program, and prior to that I was the manager of the propellant development department for a few years. And prior to that I was the manager of the project engineering group for advanced development and development projects.

I started with Thiokol in 1959, after graduating from Montana State University, and my first job was involved in designing the external insulation for the stage one of the Minuteman. I did that design and I was then chosen as a group leader for flight test

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of the Minuteman, and participated in the flight tests, about the first 20 flights of the Minuteman, out of the Cape.

Subsequent to that, I spent considerable time working as the chief engineer on development of some controllable solid rocket motor concepts for several years, and worked on a lot of advanced development programs, before I became involved in the Trident program in the early seventies.

CHAIRMAN ROGERS: For the last three or four years, where have you spent your time?

MR. McDONALD: The last couple of years, of course, I spent my time as director of the solid rocket motor project, which involves a considerable amount of coordination relative to production of the shuttle solid rocket motors, assessment of the flight readiness reviews for the space shuttle solid rocket motors.

I am the chairman of the senior material review board for the SRM. That senior material review board has to review all of the discrepancies on any of the hardware that's considered as criticality one or any that is outside the experience base of our previous experience.

As chairman of that board, there are members from our quality group, our engineering group, NASA

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Marshall representatives, but I have to sign every one of those that they're ready for flight.

And I am also a co-chairman of the problem review board with Marshall Space Flight Center, relative to any problems that are identified from returned hardware that come back to the Cape and what actions are taken to understand those problems and provide fixes for those and prevent them from occurring.

My activities in the past year to a large extent have been involved with design certification of the filament wound case solid rocket motor. I am also the co-chairman of the design certification team, represented by Thiokol and Marshall at the SRM level for that, and that has taken a considerable amount of my time this past summer in a series of reviews for certifying the graphite composite case for Vandenberg.

CHAIRMAN ROGERS: Now, how much time did you spend in the performance of those duties at Kennedy Space Center?

MR. McDONALD: Well, I have been alternating with my supervisor, the vice president, space booster programs, Mr. Joe Kilminster.

CHAIRMAN ROGERS: Mister who?

MR. McDONALD: Joe Kilminster.

CHAIRMAN ROGERS: And what is his title?

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MR. McDONALD: He is the vice president of the space booster programs. And he and I have been alternating going to the Cape, supporting the launches of the SRM's.

CHAIRMAN ROGERS: Is he your immediate supervisor?

MR. McDONALD: He is my immediate supervisor, yes.

CHAIRMAN ROGERS: And you alternated going to the Cape?

MR. McDONALD: Yes.

CHAIRMAN ROGERS: Now, how did that work?

MR. McDONALD: That works out fairly well. We both, of course, have a fairly heavy demand on our time, with all of the reviews in the shuttle process, primarily with the trips to Huntsville and the Marshall Space Flight Center.

I attended, of course, the STS 51-L. He was at the prior flight. And our charter is to attend the L minus one meeting.

CHAIRMAN ROGERS: 51-L is the Challenger launch?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: And you were there at that time. Was he, Mr. Kilminster, there at the previous

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launch?

MR. McDONALD: He was at the previous launch, yes.

CHAIRMAN ROGERS: What about the one before that?

MR. McDONALD: The one before that I was at, and he was at the one, I think, before that. We have been fairly well alternating.

CHAIRMAN ROGERS: And what were your responsibilities when you were there at the Cape at the time of the launch?

MR. McDONALD: Well, at the time of the launch I was in the firing room two launch control center. There are seats there for the monitors of many of the functions on the solid rocket boosters, as well as a TV monitor.

The Marshall Space Flight Center has a group at their console that involves the primary contractors for all of the subsystems. The SRB has one monitor, and they have people from USBI who provide part of the hardware for the SRB and for the shuttle.

We also have a monitor from our support to the space shuttle processing contractor, that is right next to that monitor, and Mr. Carver Kennedy, our vice president of our space operations at the Cape, was at

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that monitor that's right next to the Marshall monitor, and I was sitting with him at the time of the launch.

CHAIRMAN ROGERS: I wasn't speaking so much about the Challenger launch as I was generally speaking. Generally speaking, when you or Mr. Kilminster are at Kennedy, you speak for the company, is that it?

MR. McDONALD: Yes. Generally, the process is, before launch there is an L minus one meeting the day before the launch, at which time all of the problems that were still open prior to the L minus one review are reviewed with Jess Moore and his board, and are assured that they are all closed out and we are ready for the launch.

Subsequent to that meeting, there is a poll that is taken. He takes an oral poll of everyone that is involved in the launch, including, as well as the propulsion systems, but the payload and everyone else. And every contractor that is involved in that has to answer to the poll as to whether they are ready to go ahead and launch.

I attended that meeting. As you know, the L minus one meeting was conducted, I believe, on Saturday or something.

CHAIRMAN ROGERS: Yes. I would like to, if you don't mind, come to that chronologically in just a

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moment.

But, so at those meetings when you were there, you spoke for Thiokol?

MR. McDONALD: Yes, I spoke for Thiokol.

CHAIRMAN ROGERS: And how long had you been there prior to the launch of the Challenger, physically located at Kennedy?

MR. McDONALD: I was at Marshall Space Flight Center earlier in the week on some negotiations for a subsequent contract, and had went down to Kennedy on, I believe it was, the Thursday before the launch.

CHAIRMAN ROGERS: Could you give some explanation of the relationship between yourself and Marshall before you went to Kennedy?

MR. McDONALD: I'm not sure what you mean by "relationship."

CHAIRMAN ROGERS: Well, what function was Marshall performing at that time, and what were you doing in representing Thiokol?

MR. McDONALD: Well, Marshall has overall responsibility for all of the propulsion subsystems on the shuttle, and we are part of the SRB team. I work directly with Larry Weir, who is the project manager for Marshall on the solid rocket motor, and he works for Larry Mulloy, who is the project manager on the SRB,

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which includes the solid rocket motor and the aft skirt, TVA system, and the parachute recovery system that makes up the whole SRB.

And I always attend or Joe Kilminster attends the launches with Mr. Mulloy, and he has the representatives also from the other subcontractors for USBI, and in case there's any questions that come up, that we can either resolve there or get resolution to prior to launch.

CHAIRMAN ROGERS: Just for the record, where is Marshall located?

MR. McDONALD: Marshall is located in Huntsville, Alabama.

CHAIRMAN ROGERS: And when did you leave Huntsville, Alabama, to go to Cape Kennedy on this occasion?

MR. McDONALD: I think it was Wednesday or Thursday afternoon before the launch.

CHAIRMAN ROGERS: Did you go by yourself or with someone?

MR. McDONALD: I went by myself.

CHAIRMAN ROGERS: Now, I want to give you an opportunity to give a full description of the events the day before, the 27th, and the day of the launch. But before I do that, I understand that you made some notes after the accident which put on paper some of your

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recollections of the events preceding the accident. Is that correct?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: And you have those notes now?

MR. McDONALD: Yes, I have those notes. [Ref. 2/25-1]

CHAIRMAN ROGERS: I would like to explain to you that you are at liberty to refer to those notes in any way you want to.

And now, beginning with the first recollection you have of the weather problem and how it came to your attention, give us a full account of the events of that day and the following day, the 27th and the 28th. And we will try not to interrupt you, except possibly for clarification.

And I want you to feel free to tell everything you recall about it, and then after that we may ask some questions to try to amplify any answers that you may give.

MR. McDONALD: Well, I had first become aware of the concern of the low temperatures that were projected for the Cape, it was late in the afternoon of the 27th. I was at Carver Kennedy's house. He is a vice president of, as I mentioned, our space operations center at the Cape, and supports the stocking of the

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SRM's.

And I had a call from Bob Ebeling. He is the manager of our ignition system and final assembly, and he works for me as program manager at Thiokol in Utah. And he called me and said that they had just received some word earlier that the weatherman was projecting temperatures as low as 18 degrees Fahrenheit some time in the early morning hours of the 28th, and that they had some meetings with some of the engineering people and had some concerns about the O-rings getting to those kinds of temperatures.

And he wanted to make me aware of that and also wanted to get some more updated and better information on what the actual temperature history was going to be predicted, so that they could make some calculations on what they expected the real temperature the O-rings may see.

CHAIRMAN ROGERS: He was calling from Utah?

MR. McDONALD: He was calling from Utah.

I told him that I would get that temperature data for him and call him back. Carver Kennedy then, when I hung up, called the launch operations center to get the predicted temperatures from pad B, as well as what the temperature history had been during the day up until that time.

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CHAIRMAN ROGERS: And pad B was the area where the launch was going to take place?

MR. McDONALD: Pad B was the pad the Challenger was to fly off of, and this was the first time it would fly off of that pad.

He obtained those temperatures from the launch operations center, and they basically said that they felt it was going to get near freezing or freezing before midnight. It could get as low as 22 degrees as a minimum in the early morning hours, probably around 6:00 o'clock, and that they were predicting a temperature of about 26 degrees at the intended launch time, about 9:38 the next morning.

I took that data and called back to the plant and sent it to Bob Ebeling and relayed that to him, and told him he ought to use this temperature data for his predictions, and that I thought this was very serious and to make sure that he had the vice president of engineering involved in this and all of his people; that I wanted them to put together some calculations and a presentation of material.

CHAIRMAN ROGERS: Who's the vice president of engineering?

MR. McDONALD: Mr. Bob Lung is our vice president of engineering at our Morton Thiokol facility in

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Utah.

To make sure he was involved in this, and that this decision should be an engineering decision, not a program management decision. And I told him that I would like him to make sure they prepared some charts and were in a position to recommend a launch temperature, just don't recommend a launch, but recommend a launch temperature, and to have the rationale for supporting that launch temperature.

I then hung up and I called Mr. Mulloy. He was staying at the Holiday Inn in Merritt Island and they couldn't reach him, and so I called Cecil Houston—Cecil Houston is the resident manager for the Marshall Space Flight Center office at KSC—and told him about our concerns with the low temperatures and the potential problem with the O-rings.

And he said that he would set up a teleconference. He had a four-wire system next to his office. His office is right across from the VAB in the trailer complex C over there. And he would set up a four-wire teleconference involving the engineering people at Marshall Space Flight Center at Huntsville, our people back at Thiokol in Utah; and that I should come down to his office and participate at Kennedy from there, and that he would get back with me and let me know when that time would be.

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I waited for a short period of time, I don't know exactly how long. It wasn't very long, and he called back and told me that he had contacted the parties and it was all set up for 8:15 p.m. eastern time for that teleconference.

I relayed that message on back to the plant and told them that we have to have charts at that time faxed out to Kennedy as well as Marshall that we could discuss for that teleconference.

CHAIRMAN ROGERS: Could you explain what "charts" means?

MR. McDONALD: Well, their charts, any data that we had for showing why we had concerns on the O-rings at low temperatures, what our history has been, what, any calculations we might be able to make relative to their performance, supporting rationale for what is an acceptable temperature, what might not be an acceptable temperature, and to review that. [Ref. 2/14-3]

And these would all be on charts that we would send by a fax machine so that people could read those and we could discuss those as the rationale for either recommending a launch or not.

I arrived at the Kennedy Space Center at about 8:15, and when I arrived there at the Kennedy Space Center the others that had already arrived were Larry

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Mulloy, who was there—he is the manager, the project manager for the SRB for Marshall. Stan Reinartz was there and he is the manager of the shuttle project office. He's Larry Mulloy's boss.

Cecil Houston was there, the resident manager for Marshall. And Jack Buchanan was there. He happens to be our manager, Morton Thiokol's manager of our launch support services office at Kennedy.

The telecon hadn't started yet. It came on the network shortly after I got there. But I was told to hold on because the charts had not been received either at Marshall or at Kennedy at that time, and we waited I guess for probably another half-hour before those charts finally came through and we could reproduce them.

In fact, they hadn't all even been received yet. There were some conclusion and recommendation charts that didn't come for about a half hour or even later than that.

CHAIRMAN ROGERS: Was it essentially a telephone conference, or was there actually a network of pictures?

MR. McDONALD: It was a telephone conference, just telephone. You could hear the voices from the other two places as well as our own. However, the way those

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teleconferences work, there are buttons that you can push and that will mute out you speaking if someone else is speaking, and you can usually hear it better when you don't have your own mikes open. So there is a lot of conversations, I am sure, at all of the facilities at one time which you can't hear.

But I will relay on what I heard on the conference as best I can. The teleconference started I guess close to 9:00 o'clock and, even though all the charts weren't there, we were told to begin

and that Morton Thiokol should take the lead and go through the charts that they had sent to both centers.

The charts were presented by the engineering people from Thiokol, in fact by the people that had made those particular charts. Some of them were typed, some of them were handwritten. And they discussed their concerns with the low temperatures relative to the possible effects on the O-rings, primarily the timing function to seal the O-rings.

They presented a history of some of the data that we had accumulated both in static test and in flight tests relative to temperatures and the performance of the O-rings, and reviewed the history of all of our erosion with any O-rings in the field joints, any blow-by of the primary O-ring with soot or

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products of combustion or decomposition that we had noted, and the performance of the secondary O-rings.

And there was an exchange amongst the technical people on that data as to what it meant and discussions. But the real exchange never really came until the conclusions and recommendations came in.

At that point in time, our vice president, Mr. Bob Lund, presented those charts and he presented the charts on the conclusions and recommendations. And the bottom line was that the engineering people would not recommend a launch below 53 degrees Fahrenheit. The basis for that recommendation was primarily our concern with a launch that had occurred about a year earlier, in January of 1985, I believe it was 51-C.

It was our motor number SRM-15, and that particular motor had a couple of field joints that not only had some erosion, but they had some fairly severe blow-by of the primary seals, fairly heavy soot over a fairly large arc, very deep and black.

And even though we could see no measureable erosion on the secondary O-ring, it was a heat effect, and by that, the sheen was gone off of the O-ring seal. That was, you couldn't measure any, but the sheen was gone, and because of that we were concerned with launching beyond our experience base, below that

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temperature.

Well, that temperature brought a lot of strong comments and reaction from several of the NASA officials. I believe it was Mr. Mulloy made some comments about when we will ever fly if we have to live with that some time in the future; and also commented that, we are trying to establish new launch commit criteria and we can't do that, you don't do that the night before a launch, that is a predetermined set of constraints and we can't do that.

And other comments were made about whether we could ever fly out of Vandenberg 24 flights a year, because it wasn't uncommon to have 53 degrees in the early morning hours where a lot of the launches occur.

One of the comments that came—and this is by voice recognition; I believe it was from Mr. Hardy at Marshall Space Flight Center—was that he was appalled at that recommendation. However, he also said that he certainly wouldn't fly without Thiokol's concurrence.

CHAIRMAN ROGERS: He said he would not fly——

Mr. McDONALD: He would not fly without Thiokol's concurrence, even though he was appalled at that recommendation.

I believe it was Stan Reinartz made a comment that he was under the impression that the solid rocket motors were qualified from 40 to 90

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degrees, and that 53 degree recommendation certainly was inconsistent with that.

Some place in the conversation about the impact of 53 degrees, I believe Cecil Houston at some time commented that it wouldn't be until about Thursday that we would have morning temperatures probably in the fifties. So he didn't seem to be as alarmed about when we could get to that temperature.

There were several challenges relative to, it was felt how conclusive the effect of temperature was on this whole problem of O-ring erosion or soot blow-by. And the challenge came from looking at the total data, because even though we were as concerned about this flight, which was the coldest ever at that time, from Florida a year earlier there was a flight back in the late fall of '85, I think it was probably October, 61-A, was our SRM-22 set, that had some blow-by of the O-rings, no erosion of the primary O-rings, in fact a couple of them.

And, this happened to be a launch that had the highest temperature, and so there was some concern that the data was inconclusive; and also that we had some motors that were static tested as low as 36 degrees Fahrenheit, DM-4 I believe, and it showed not only no O-ring erosion, but no blow-by.

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Well, some of the comments that came back from that, and I believe it was Roger Boisjoly commented that he felt that there was a significant difference in the observation of the actual soot that passed the primary O-ring on the SRM-15 set, that was the cold one, versus the one that was warm; that there was a much larger arc between the two O-rings from the effect of the soot, it was much blacker. It penetrated all the way up to the secondary O-ring, and of course we had some heat effect there.

And he thought there was a significant difference, and that well could be the difference just due to temperature.

I commented at that time about the static test history. I told him I did not feel that that was a valid data, for a couple of reasons. One, on the static test motors, we keep the static test motors in an environmental building essentially during the buildup, assembly, and checkout of all the instrumentation, and that environmental building is kept at 70 to 72 degrees, and that building is then rolled back on a track about six hours before the static test. So it wasn't exposed very long to the cold temperatures.

Secondly, in the static test motors we went in and actually repaired and filled holes in the putty.

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These holes are formed by the assembly of the rocket motor. As you form the tang and clevis, there's air trapped in there, and that air has got to go someplace. And sometimes that air will go up through the putty. We have seen this.

And also, when you run the leak check on the two seals, if you get any blow-by during the leak check it may tend to propagate those. And in the static test motors, some of these were observed and they went in and filled those inside the motor prior to static test.

So I told him I didn't feel that that data was conclusive and they shouldn't use that for deciding what the O-ring performance was.

At that time there was other discussion on some of the charts that we had that was somewhat inconclusive. We had a chart that had some sub-scale data, where we had run some tests on blow-by with some sub-scale O-rings that actually had the full-scale diameter, 280 thou-

sandths, but they were smaller. That was the cross-section of the O-ring, they are smaller diameter hardware.

And they were cold gas tests, and we were attempting to try to measure what kind of blow-by one might get with the pressurization rates you see in the motor. And we were using argon as the working fluid and

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also some freon, and we had run some tests at around ambient 30 degrees in those and did not see any difference.

And so there was some conflicting data that said that it wouldn't make any difference. But that, I want to remind everyone, was a cold test.

At that time, I commented at the time that I felt that lower temperatures were in the direction of badness for both O-rings, because it slows down the timing function for both of those, but the effect is much worse for the primary O-ring compared to the secondary O-ring, because the leak check forces the primary O-ring in the wrong side of the O-ring groove, while it forces the secondary O-ring in the proper direction; and this fact should be weighed and considered in making an evaluation as to what the recommended temperature should be.

I was looking at a chart at that time that we had. In fact, it is a chart that I had made with some help of our engineering people back last summer, in a presentation I had made here in Washington to the headquarters people on August 19th.

And it was a chart that showed that there is really three phases or three regions of concern during the ignition transient relative to the performance of

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the O-ring. The first phase of that is a condition where the O-ring, the primary O-ring, is pressurized and starts the energizing process and actually has to move from the forward face of the O-ring groove, because that is where it's at as a result of the leak check, and then it has to move back and seal on the back face.

And this process we felt took place in the early part of the ignition transient, someplace before it got to 200 psi. And in cold weather what's going to affect that, of course, is the grease that is in that area also is very viscous and stiff. The O-ring itself is stiff.

We knew that the cold temperature shrank the O-ring some, and from our resiliency tests, which are tests that basically show how the O-ring responds when you have it under some compression and release that load, it shows that it gets cold and stiff, it doesn't want to respond very well.

And I looked at it something like a flat tire on the bottom. I remember when we used to drive nylon tires; when it was cold I would get out and there was a flat spot on them.

And that O-ring having to move, it either has got to slide or it has got to roll somewhat. It is not a very big dimension it moves in. It is 20 or 30

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thousandths. But certainly it can't be as good as when you don't have that.

Also, being hard, it then has to extrude into the gap between the two O-ring seals, and the harder it is I'm sure it's harder to extrude in the gap, which means it may take a higher pressure to do that, which also takes a longer time.

I felt that, based upon what we had done in the past, we had concluded that this blow-by phenomenon had really occurred in this first part of the pressurization cycle, and I think there was good evidence that it had because the soot that appeared between the two O-rings was exactly that, it was soot; and we analyzed it, but it didn't see any heat effect on the metal at all.

In some cases it discolored the grease, but didn't really burn it. Some of it was the products of the grease. You could not see any real effect on the secondary O-ring. So it couldn't have happened very long and it couldn't happen under very severe conditions.

But at the same time, the temperature effects were concerning, that maybe we were going into another timing regime. However, if we felt that we had a good margin there and we felt that we do have a good

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secondary seal during this time—I think there has been some misconception about the redundancy of the secondary seal. The secondary seal is indeed redundant until the metal parts between the tang and the clevis actually rotate. It is a full redundant seal and during this time period it is redundant, and that is important.

But once those metal parts rotate and you have a problem of resiliency, it may not be. And so that is why I was concerned about that area. But if we could convince ourselves that the data said it wouldn't make that much difference in that part of the phase, then it would be a reasonable recommendation to say that we wouldn't expect much different performance.

Shortly thereafter, Thiokol was asked about their recommendation. I believe Joe Kilminster was asked himself what his recommendation would be, since it was engineering that recommended not flying at 53. And he said that he would not go against our recommendation, he couldn't go against it.

But based upon the controversy relative to how conclusive the effects of temperature actually were on this phenomenon, we were asked to reassess and re-evaluate that data, and we decided we would do that. And the people at Utah said that, well, they would like

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to have a caucus for about five minutes and go off the line.

CHAIRMAN ROGERS: Could I ask you to stop there just for a moment and go back. We will come to the caucus in a minute, but go back and explain what was said to convey the decision of no launch? I gather at that point the decision by Thiokol was to recommend against a launch?

MR. McDONALD: That is correct, that it was at that point, the recommendation was not to launch below 53 degrees.

CHAIRMAN ROGERS: Who did the talking on that subject?

MR. McDONALD: That was Bob Lund, vice president of engineering, who presented that position.

CHAIRMAN ROGERS: Were you able to ascertain from that conversation how the engineers as a group stood on that?

MR. McDONALD: Well, the engineers as a group, I can't speak for the group. I was not there, but I did hear the engineers that presented the charts, that they actually presented as part of that, that supported the 53 degree recommendation. And I felt they were very strong in their conviction as to why they felt uncomfortable to go outside that experience base.

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CHAIRMAN ROGERS: And who were they?

MR. McDONALD: Roger Boisjoly I think was one of the strongest ones, and Arnie Thompson, that presented those positions and presented the charts.

CHAIRMAN ROGERS: And Mr. Lund himself at the time?

MR. McDONALD: Yes. Mr. Lund himself at the time didn't present the detailed technical charts, but he did present the conclusions and recommendations, and that was his recommendation as vice president of engineering.

CHAIRMAN ROGERS: Was anyone who was on the telecon from Thiokol's side recommending launch?

MR. McDONALD: At that time, no, there was no one that recommended launch. And I don't recall there was anyone at either Marshall or Thiokol from NASA that didn't agree that cold temperatures went in the wrong direction, didn't help anything. But no one from Thiokol at that time recommended launch.

CHAIRMAN ROGERS: And what were the comments by NASA officials about that recommendation, as you recall?

MR. McDONALD: Well, as I recall, there were some fairly strong comments about being appalled by the recommendation, about trying to institute new launch

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commit criteria at the last minute, about when we will ever get this launch off.

I thought those were fairly strong comments.

CHAIRMAN ROGERS: And those are comments, according to your recollection, that were made by whom?

MR. McDONALD: Well, the comments relative to the launch commit criteria and when we will ever get this off was made by Mr. Larry Mulloy. The comment about being appalled was George Hardy, who was at Marshall. I think I recognized his voice, but that was by voice recognition.

CHAIRMAN ROGERS: And so it was decided—and are there any other questions that any other member of the Commission has up to that point?

GENERAL KUTYNA: Mr. McDonald, I have one question. Before you went off the net, did you ask or make a comment about the secondary O-ring seal seating?

MR. McDONALD: Was I asked to make one?

GENERAL KUTYNA: No, did you make a comment?

MR. McDONALD: I did make a comment, yes.

GENERAL KUTYNA: Could you recall that comment, please?

MR. McDONALD: Yes. I think I read it to you, and I would be glad to do that again. I made the comment that lower temperatures are in the direction of

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badness for both O-rings, because it slows down the timing function, but the effect is much worse for the primary O-ring compared to the secondary O-ring because the leak check forces the primary O-ring into the wrong side of the groove, while the secondary O-ring goes in the right direction; and this condition should be evaluated in making the final decision for recommending the lowest acceptable temperature for launch.

That was the comment I made.

GENERAL KUTYNA: That confused some people. Some of the witnesses I spoke to thought that that was a comment in support of the launch, the fact that the secondary O-ring seal would seat.

MR. McDONALD: Well, that comment is a good news-bad news comment. There is good news and there is bad news. The good news is that the secondary seal is in the right position, but that is not unique to temperature. It is always that way.

The bad news is that the primary seal is the one we are depending on for the full ignition transient, and it is going to be a lot worse than it was. But even the secondary, as I mentioned, wouldn't be as good cold as it would be normally.

GENERAL KUTYNA: But by this comment you were not supporting the launch?

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MR. McDONALD: No, not by that comment I wasn't supporting the launch. I was just saying it is an important consideration and I felt that if we could run some calculations to show that the temperature did not affect the timing, then that would be supportive of the launch.

If it did—and that was a concern, if we push that timing out long enough, we had a chart in there that said if that timing goes beyond that 200 psi regime while you're still eroding the primary O-ring, that for whatever reason if you ever bypass it at that time you can't depend on the secondary, and that is what is important.

GENERAL KUTYNA: Thank you.

CHAIRMAN ROGERS: So at the time of the caucus, then, you never favored launch?

MR. McDONALD: No.

CHAIRMAN ROGERS: And you made it clear that you were opposed to launch?

MR. McDONALD: Well, I never said I was opposed to the launch. I just made a few comments about why I thought some of the data was not appropriate, like the static tests, for saying the O-rings were good to 36 degrees. I made this comment about the lower temperatures affecting both O-rings, but it affects them

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a little bit differently because one of them, it is a dynamic O-ring, it moves. One of them has a lot more movement and effects on it than the other one does, because it has to move across the groove.

I made those comments.

CHAIRMAN ROGERS: But you accepted the recommendation?

MR. McDONALD: I accepted the recommendation, yes. I thought it was the appropriate recommendation.

CHAIRMAN ROGERS: In other words, you accepted the recommendation of no launch that was made by your company?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: Mr. Armstrong.

VICE CHAIRMAN ARMSTRONG: Would you be surprised if your comments were interpreted by both your own company personnel and Marshall personnel as being supportive of the Marshall position?

MR. McDONALD: Yes, I would be surprised at that, yes. I wouldn't be surprised that that would be evaluated as the effect of that, but I would be surprised that it was interpreted as supporting.

VICE CHAIRMAN ARMSTRONG: Thank you.

MR. RUMMEL: Mr. McDonald, you mentioned a 40 to 90 degree qualification limit. Was that referring to

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ambient air temperatures?

MR. McDONALD: Well, I had a discussion about that later. It was my impression that the motor itself was qualified from 40 to 90 degrees for operating, and we got into a discussion in

fact during the caucus, that was supposed to last for five minutes, that lasted for about a half an hour, on that very subject.

I told the people that were there from NASA that I, first of all, didn't agree that the motor was even qualified for 40 to 90 relative to all of its elements and subsystems. I wasn't there in the qualification of the steel case motor, but I just recently went through that process in the filament wound case SRM, and I am not aware of the testing or analysis that the O-rings were good to those temperatures, and that therefore it is part of the elements.

And the way I interpreted the spec is that all of the components and elements that make up the SRM should be qualified to those temperatures. Larry Mulloy at that time told me: Well, no, the 40 degrees refers to a propellant mean bulk temperature, and the propellant mean bulk temperature was being predicted to be 55 degrees for that launch; and that as long as the propellant mean bulk temperature wasn't below 40 degrees that you could expose the other parts of the motor to

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lower temperatures, as long as you didn't drop the propellant mean bulk temperature outside of the 40 degrees.

I responded, I told him I thought that was absolutely ridiculous, because you could expose that motor to very severe cold temperatures, subzero temperatures, for a fairly long period of time, and you wouldn't change the propellant mean bulk temperature at all. It is just such a massive amount of propellant and insulator there that it takes a long time to do that, and I'm sure that the spec really didn't mean that.

And so my interpretation was certainly different than his.

MR. RUMMEL: Was there in fact a minimum temperature established by specification or by rote or in some manner that related to the O-rings or the joint at that time?

MR. McDONALD: Well, I wasn't aware of one for the O-rings. I found out later that there was—our specification refers to a higher level specification, which is level two at Johnson. I think that's the 07700 spec, that says that the shuttle vehicle has to be capable of launching in 31 to 99 degrees, or something like that; and therefore, since it is a higher level spec, that we should be able to comply with that.

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But I'm not aware that all of the elements and subsystems were ever qualified to that.

MR. RUMMEL: What do you mean by "higher level spec"? Could you explain that?

MR. McDONALD: That specification comes down from the Johnson Space Center, who is responsible for the overall preparation of the vehicle, the shuttle vehicle, and what it is capable of operating in, the environments, both pre-flight and flight. And that specification for the overall vehicle gets incorporated as part of the lower level specifications that go through level three, which is Marshall Space Flight Center, to the various elements.

And they refer to that, and when they refer to that the way the system is supposed to work is, you're supposed to comply with your own specification plus any higher level specifications that may involve the entire shuttle system.

And I was unaware of that, frankly, that that criteria was in there. I'm still a little puzzled about it, because it doesn't have any timing limitation on it, either, whether it's 31 degrees for five minutes or 40 days.

MR. RUMMEL: Was there a Thiokol design temperature limit at the time this motor was designed

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with respect to low temperatures?

MR. McDONALD: I really can't answer that, because I wasn't involved in the original design qualification.

CHAIRMAN ROGERS: If I may, I would like to continue, and we can come to some of these other questions. I would like to continue the chronology, because we were going fine and you stopped about the caucus.

Mr. Armstrong has a question.

VICE CHAIRMAN ARMSTRONG: One question, involving your answer that you just gave. Had Morton Thiokol to your knowledge ever informed NASA that the launch commit criteria were inadequate or did not in fact cover the kinds of conditions that you were concerned about?

MR. McDONALD: I am not aware that they ever did, not to my knowledge.

VICE CHAIRMAN ARMSTRONG: And so we're really talking about an event that was within the launch commit criteria, but outside what your experience base was?

MR. McDONALD: Well, I guess I don't even—I'm not convinced of that, either. I didn't learn about the 31 degree thing until some time afterwards.

But we were told to make this evaluation on

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the basis of launching at 0938 in the morning, where the predicted temperature was 26 degrees Fahrenheit. That was the predicted temperature, and that was the decision that was made, whether we could launch at that time. So I'm not sure where that came from either.

CHAIRMAN ROGERS: Dr. Ride.

DR. RIDE: Yes, just one follow-up to Neil's question. Are you aware of NASA ever asking Thiokol to qualify the SRM or the SRB to 31 degrees?

MR. McDONALD: I'm not aware of it, but again, I wasn't in that part of the program. They may have. I can't say.

CHAIRMAN ROGERS: If we may now, let's go back. You said that a suggestion was made that you have a recess for five minutes. Who made that suggestion?

MR. McDONALD: That suggestion was made from someone at Thiokol. I can't recall whether that was Joe. I think it was probably Joe Kilminster, but I'm not sure.

CHAIRMAN ROGERS: Okay. Now what happened? You said that that lasted almost a half an hour instead of five minutes?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: And during that time what did you do and who were you with?

MR. McDONALD: Well, I was in the conference

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room at Kennedy with Mr. Reinartz and Mr. Mulloy and Mr. Houston and Jack Buchanan still. And I gave you my conversation about interpretation of the qualification temperature there.

I also commented at that time that, I suggested that maybe we consider a late afternoon launch. I didn't feel good about the low temperature launch, because when I had first come down to Kennedy the original schedule was to launch that in the late afternoon, I think quarter to 4:00 or something like that. And based upon the weather report I heard, the temperatures would be 48 to 50 degrees in the late afternoon.

So I said, why don't we go to a late afternoon launch, and I was told that was considered, but it was rejected because of some problem either with visibility or weather at one of the trans-Atlantic abort sites. I think it was Dakar or Casablanca, one of those.

I really expected—and the reason it was taking so long for this five minute caucus is that we were either trying to find some more information to support our recommendation or that we were trying to run some kind of calculations to determine how far away from that we could go, because clearly 26 degrees is a long way from 53.

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I personally felt we certainly wouldn't go below 40, because I thought my interpretation of the qualification was correct and we wouldn't do that. But the reason it took so long was the engineers were reassessing all of the data they had and finding any more that they may have, and trying to quantify more. A lot of that data was very qualitative.

And finally, the people from Thiokol in Utah did come back on the line, after about a half an hour, and I believe it was Joe Kilminster who came on the line and said that, even though we had some concerns about the lower temperatures, that we would recommend that they proceed with the launch, based on the fact that we felt the temperature data that we had was not totally conclusive.

And he outlined several concerns still that we had relative to the effect of temperature, but also some rationales why we felt it was safe to proceed.

CHAIRMAN ROGERS: Can you relate what he said?

MR. McDONALD: Well, it was the information basically that ended up finally on the faxed statement relative to our concerns about the O-rings being colder and harder, but that we also had some data that was inconclusive relative to temperature, and I'm sure it

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was the warmer temperature launch when we had some blow-by.

I didn't see anything that I recognized that was new information, but maybe they had some. And I felt all the engineers were certainly there that had generated the original data and evaluated it, and maybe they had reconsidered or re-evaluated the data they had and tried to quantify it and felt it was probably okay.

When he completed that he was asked, I believe it was by Mr. Hardy, I'm not sure, to put that rationale in writing and to sign it, make sure they get it down to the Cape, I think by morning, early morning.

And I was sitting across the table from Larry Mulloy at the time and I said I felt that I was the one who was going to have to sign it, because I was at the Cape; and I said I wouldn't sign it. I couldn't; it would have to come from the plant.

Joe Kilminster said that he would draft a letter or a statement and send it down, and he would do it tonight. He wouldn't wait until the morning. And I was instructed to stay there until that came down; and that he would also send it out to Marshall, and it would be a few minutes before that would come down. [Ref. 2/25-2

He went off line at that time, and I asked Cecil Houston where the fax was, and he told me it was

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at the other end of the building. I had a conversation with people at Thiokol just before they went off the line: Would they send that information in to Jack Buchanan's office, which was the fax machine right next to that we had, or were they going to send it in to the Marshall office, they had a Pitney-Bowes machine or something?

And they said: We will send it on the same one we sent all this other material, because they had the right number, and it happened to be the Marshall one, which was at the other end of the building. And so I said, fine, I would wait.

Again, it took some time for that fax to get there, and so all of the parties who were in the meeting were still sitting there. And we started to talk about some things.

I told them I didn't feel very good about this recommendation, and I recognized it is very difficult to quantify at which temperature these seals may be acceptable and where they aren't acceptable based upon that data. Some of it certainly was inconclusive, there was no doubt in my mind, and that is a difficult thing to quantify.

But even though I didn't agree with the 40 degree qualification of the motor, that all elements

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were qualified to that, it was my understanding that there were a lot of people at both NASA and Thiokol signed up to that, the design certification process, the critical design reviews, and I was absolutely surprised that NASA would accept any recommendation below 40 degrees Fahrenheit, especially when the predicted temperature was as low as 26 degrees Fahrenheit.

I told them: I may be naive about what generates launch commit criteria, but I was under the impression that that was generated based upon the qualification of all elements or subsystems of the space shuttle, and that anything that was outside that qualification was a launch commit criteria, and we never went outside that envelope, and I don't know why NASA would ever launch below 40 degrees Fahrenheit if that is what the SRM was qualified to.

In fact, I made the direct statement that if anything happened to this launch, I told them I sure wouldn't want to be the person that had to stand in front of a board of inquiry to explain why we launched this outside of the qualification of the solid rocket motor or any shuttle system.

When I made that statement, no one commented on that. I was still very upset, and so I asked that they reconsider this decision, for three reasons not one

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but for three. And I said, if I were the launch director I would cancel this launch, for three reasons, not just one:

The first one being the concern of the cold O-rings that we just discussed, but there were two others. I had just left Carver Kennedy's house in Titusville, Florida, and he's responsible not only for stacking of the SRM's, he's responsible for the retrieval operations.

And he had been in communication with someone that was at hangar AF, I believe, that contacts the booster recovery ships at sea, and they had told him that the booster recovery ships were in an absolute survival mode, was how they put it; that they were in seas that were as high as 30 feet. There were winds at 50 knots sustained, gusting to 70 knots, pitching the boat as much as 30 degrees.

They even felt the rough seas may have damaged some of the retrieval equipment on the back of the ship. They were steering directly into the wind, heading for shore at about three knots, and they had been doing that for some time. There was no way that they would be able to support an early morning launch, because they wouldn't be in the recovery area.

I then reminded everyone in the room that

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there are some firsts on this launch. This was the first time that we were going to use the new electronic control system for separating the nozzle extension cone on the SRB's at apogee, rather than just before water impact, while it is under full main parachute, and we were going to separate the parachutes at water impact for the very first time on this launch; and that, based on

the sea states that I had just heard, it appeared to me that it was going to be nearly impossible to recover that hardware, either the parachutes or the thrustums.

That there was also, I felt that they were putting the boosters at some risk as far as recovery was concerned, because the ships were steering away.

I also said that the third reason for not launching is the formation of ice. I knew that the sound suppression system was a water system, and I felt there was probably a lot of ice around there. And I'm no expert on all these matters, but I do feel that there may be a chance that that changing acoustics may be a problem with debris. It may have some effect on the structures. I didn't know, but I didn't think it was prudent to launch under that kind of a condition.

I was told that you know, there really weren't my problems and I really shouldn't concern myself with

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these. But I said, you know, all three of these together should be more than sufficient to cancel the launch, if the one we had discussed earlier wasn't.

The NASA people who were there said that, well, they would pass these on, and they could tell I was disturbed and they would pass those on as concerns, and that they would do it in an advisory capacity.

I was then asked by Mr. Mulloy where the signed fax was, because some time had transpired since the teleconference had ended and it still wasn't there. And so I said, okay, I will go check on that, and I went down to the other end of the building, to this fax machine.

And there was nothing there that had come out yet, and I really wondered if it was working. And it was getting kind of late, and so I stayed down there I guess for about ten minutes. And it finally came in.

I brought the fax back—it was a single sheet of paper—to Jack Buchanan's office, where we reproduced copies for everyone. And I walked into, I think it was, Cecil Houston's office there and there was a telecon being conducted, and I believe it was with Arnie Aldrich.

And they were in the middle of this telecon and they were discussing the conditions of the booster

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recovery ships and the high sea states and the fact that they were in a survival mode. And I recall that Arnie Aldrich asked Larry Mulloy if he felt there was any risk to recovering the boosters because of this. And he said, no, he didn't think there was any significant risk in recovering the boosters, because they had beacons and monitors, and they were going to have aircraft in the area also; but that there was a high probability that they would not recover the parachutes or the thrustums.

And the conclusion was that, I guess, they would have to make a decision to launch on the basis that they would have a high probability of not recovering that hardware.

But they weren't compromising the recovery of the boosters significantly. And I remember Arnie asking Larry the value of that hardware, and I think he gave some number close to a million dollars and asked him if he could afford to lose it and support the schedules of the program. And he said that he had sufficient inventory to do that.

But Arnie also told him that in no way have those ships attempt to turn around in those kinds of conditions, and the safety of the ships was important, to try to support that launch, and to turn around too soon, so to tell them to continue on towards shore until

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it was really safe to turn around.

They then briefly discussed the ice issue, about the ice, and there was a concern raised there. And Arnie, I believe it was Arnie, responded that they had discussed that issue earlier in the day. So it was a fairly brief conversation.

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CHAIRMAN ROGERS: Arnie is Mr. Aldrich?

MR. McDONALD: Yes, Mr. Aldrich. They also made it clear that they were acting in advisory capacity only they weren't making some recommendations, they were acting in advisory capacity, but they wanted to be aware of this information. I didn't hear anything discussed about the O-ring seal problem. I presume that was done while I was down waiting for that fax, because that was the first concern.

CHAIRMAN ROGERS: But you're not sure of that?

MR. McDONALD: I'm not sure, I presumed it was because I got in on the middle of the conversation on the boosters and heard the ice. So I don't know. And finally the conversation concluded and the recommendation was to proceed on with the launch.

I stayed around a few more minutes and talked to Jack Buchanan for a few minutes, and then went back to Carver's house in Titusville, where I stayed, and got in there, I guess, a little before 1:00 sometime. And I guess that's all I have to say.

CHAIRMAN ROGERS: A couple of questions.

One, when you first appeared before the Commission I remember your explaining the fact that the company originally had recommended against the launch. And then you testified that after the caucus they came back and changed their mind and said go ahead with the launch. You said it was based upon inconclusive data.

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I remember asking you how could they have changed their mind based upon inconclusive data. Can you explain that?

MR. McDONALD: Well, I can't explain that. That was the comment that was made, that the data was inconclusive relative to the effects of the temperature, and I prefer you ask those questions of those that made that decision. I don't know. I wasn't there. I wasn't there when all of that data was reassessed and evaluated and what the conclusions were drawn.

CHAIRMAN ROGERS: One other question. Dr. Keel asked at the conclusion of your testimony in executive session whether you felt that you were under pressure or had been under pressure or the company had been under pressure to reverse its decision, and I think your answer was yes. Do you remember that?

MR. McDONALD: Yes, definitely. There was no doubt in my mind I felt some pressure. I feel that I have a responsible management position, and I felt pressure.

CHAIRMAN ROGERS: Would you explain the reasons for feeling pressure?

MR. McDONALD: Well, I have been in many flight readiness reviews, probably as many as anyone, in the past year and a half at Thiokol, and I have had to

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get up and stand before, I think, a very critical audience at Marshall, and a very good one, justifying why our hardware was ready to fly. I have to get up and explain every major defect and why we can fly with that defect.

And for the most part they are very minor—very, very minor. And I have been hassled about how I'm sure that that is okay to fly with. You know, such things as losing vacuum in a carbon cloth part in the nozzle while the part is basically cured. It is a critical process.

There is a lot of those critical processes, and I have to address every one of those in great detail as to why I am sure that that part has not been compromised. And it has been that way through all of the reviews I've ever had, and that is the way it should be. And it is not pleasant, but that is the way it should be.

And I was surprised here at this particular meeting that the tone of the meeting was just the opposite of that. I didn't have to prove that I was ready to fly. In fact, I think Bob Crippen made the most accurate statement I ever heard. His conclusion from that meeting was the philosophy seemed to have changed because he had the same impression I did, that

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the contractor always had to get up and stand up and prove that his hardware was ready to fly.

In this case, we had to prove it wasn't, and that is a big difference. I felt that was pressure.

CHAIRMAN ROGERS: And can you explain a little more what source the pressure came from in your mind?

MR. McDONALD: Well, I think the strong statements that were made by Mr. Mulloy, and even some of those, the people at Marshall that were on there—Mr. Hardy—were I think fairly strong statements that I took as pressure about when we will ever fly this thing and the launch-commit criteria that we can't generate at the last minute, and appalled by our recommendation to fly at temperatures as high as 53 degrees. And that, to me, that was pressure to me.

It may not have been interpreted by others, but it was pressure to me.

CHAIRMAN ROGERS: Any other pressure that you want to refer to at this time or at any time?

MR. McDONALD: No. I just felt that the way the comments were made, as strong as they were made, and the fact that the conditions for justifying this launch were so much different than anything I'd been involved with before.

CHAIRMAN ROGERS: As far as the Telefax was

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concerned, would you normally have signed that Telefax, or was the procedure that was followed the normal procedure?

MR. McDONALD: Well, I'm not sure, I guess, what the normal procedure is. I felt that since I was there representing the senior official at the Cape that I'm the one that usually is responsible for that. I know at the L-1, when we have a normal launch, there is a poll that is conducted by Jess Moore of all the contractors, whether they are ready to fly, and I am the guy there that has to get up and say yeah, Thiokol's hardware is ready to fly.

I felt that was my responsibility. That's why I'm there. I can't recall whether we were asked to sign anything like that before. We have a presentation that is given at every flight readiness review that is signed off by the principal parties. Joe Kilminster usually signs all of those on the formal review, but if anything comes up afterwards it has been done on an oral basis.

But I don't know. I've never been put in that position, and I don't know if Joe has. I don't believe he has.

CHAIRMAN ROGERS: So, the fact that a written decision was requested, as far as you were concerned, was not a normal way to do it?

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MR. McDONALD: It was not normal as far as I was concerned.

CHAIRMAN ROGERS: And, as I recall your testimony, you testified that you made it clear that you would not sign a written statement approving the launch. Am I correct?

MR. McDONALD: Yes, that is correct. And I think that has been misinterpreted, at least by the press. They said that I was overruled by my supervisor. That is not true at all. I chose not to sign that. He didn't overrule me. I felt that that decision, when I started, was an engineering decision by the people that understood the problem the best, that had all of the data and facts, and they are the ones who should recommend it. And that is why I made that.

It wasn't that I was overruled.

CHAIRMAN ROGERS: Thank you very much. I am sure other Commission members will have questions.

Mr. Armstrong?

VICE CHAIRMAN ARMSTRONG: You were talking about the 40-degree qualification of various components of the solid rocket booster, and I was aware of the 40-degree limit on the mean bulk temperature. But was there anything else that the 40 degrees referred to?

MR. McDONALD: Well, in the spec, going back

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after the fact and looking at it, that is what it had. What it refers to is the propellant mean bulk temperature.

I guess that is, in my opinion, it's an oversight, maybe, in the spec, or the launch temperature of 31 to 99 should be stronger in there to apply to all components, and by qualification it either has to be qualified by test or some analysis, and that means every element. And I'm not aware that all of those elements were qualified to that temperature.

I know we bought O-rings that said they are good to minus-30, but I never saw the analysis, and the application that we used them in that says that they are good to that, and that is a difference. It is a material problem versus a design problem.

VICE CHAIRMAN ARMSTRONG: I understand that, and I understand the 40 degrees was intended to protect against grain cracking and the consequences of that. But what I didn't quite understand is why you said you wouldn't recommend any time launching below 40 degrees.

MR. McDONALD: Well, the reason I said that is I felt that is what everyone had signed up to, as what this thing was really qualified from an operating standpoint. Now the motor does get exposed to much lower temperatures. We have a criteria in our

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specification to expose that to much lower temperatures, sub-zero temperatures in the 60-mile winds coming across Wyoming, because we ship these things from Utah down to the Cape. So in the transportation mode they are subjected to some very severe temperatures for some periods of time.

And there has been analysis to show that they will withstand those temperatures as long as you don't operate them at those temperatures. The 40 degrees, they won't crack. There is no problem there. That is a, primarily a performance standpoint. You lost total impulse and performance the lower the temperatures go. If you get below 40 you start losing performance out of the shuttle and that is why that number came about.

As far as the capability of—grain structural capability, it can withstand a lot colder temperatures than that, and we have analysis where the transportation and storage shows that, and it has good margins of safety, very high margins of safety for exposure to lower temperatures.

It is just that things like the O-rings and some of these other components, I am not aware that we have really analyzed or tested those well enough to understand how they would operate in that kind of a temperature. There is a difference between exposing

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them and storing them versus operating them.

VICE CHAIRMAN ARMSTRONG: Thank you.

DR. WALKER: I have a question just referring back to the static test that you mentioned at 36 degrees which was used as a justification. Was there analysis to indicate what the O-ring temperature during the test was?

MR. McDONALD: Well, it was done after the fact, and I believe in the presentation that was made on the night before the launch they calculated like 47 degrees is what they presented as what they expected the O-ring temperature to have really been when that was static tested.

DR. WALKER: I have a couple of other questions regarding the conference. Was there a specific request by NASA to reconsider, or was the reconsideration as a result of the implications of the remarks made by the NASA people in the conference?

MR. McDONALD: Do you mean the reconsideration of what?

DR. WALKER: Of the initial decision not to launch. The initial recommendation of Thiokol was not to launch, and then that was reconsidered. Did anyone from NASA explicitly ask for reconsideration, or did the reconsideration occur because of the negative remarks

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and comments on that decision?

MR. McDONALD: Well, I think it was the latter. I can't fully recall whether they directed us to do that or not, but they had concluded that the temperature data was inconclusive, and I don't know whether we volunteered to reassess it or they said we need to. It is not clear to me on that. I'm not sure.

DR. WALKER: One further question in regard to the signature on the sheet which was faxed from Mr. Kilminster. Was there a specific request for that to be signed, and who made that request?

MR. McDONALD: Yes, there was a specific request for that to be signed. I believe it was George Hardy. It may have been Larry Mulloy. But I think it was George Hardy had requested that.

MR. ACHESON: Mr. McDonald, did you consider bringing your concerns about the final recommendation to the personal attention of Mr. Moore or Mr. Aldrich or Dr. Lucas?

MR. McDONALD: Well, I'm very familiar with the process by which these things are reviewed, and I was absolutely positive and sure that they were brought to their attention, because that is the way things go. I talked to the SRB project manager. I talked to his boss, the shuttle project manager, the shuttle project

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office, and I was assured that those all went through those reviews.

I had no doubt in my mind that they had.

MR. ACHESON: Thank you.

DR. FEYNMAN: You indicated that NASA folk indicated that they would pass your concerns along, and I presume that you thought that—there were three concerns that you were talking about—the O-rings, the ocean, sea and the ice. But could they have simply meant the last two concerns, the sea and the ice? That is one question.

And the other is who were the NASA folk that promised to pass your concerns along?

MR. McDONALD: I guess you could maybe interpret that I thought it would be all three, but the people were Mr. Mulloy and Mr. Reinartz that would pass those on, and I presume they passed them all on. I didn't see any reason why they wouldn't.

MR. HOTZ: Mr. McDonald, you mentioned earlier that you thought this decision on launching should be an engineering decision and not a program management decision. How would you characterize the final decision to launch from Thiokol? Was it engineering or was it management?

MR. McDONALD: I guess I would have to

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characterize it as a management decision, the final decision.

MR. HOTZ: Thank you.

DR. RIDE: I have got a couple of questions on Criticality 1s. Did I understand you to say that you chair the board on Criticality 1s on the SRM from Thiokol?

MR. McDONALD: Yes, on all defects that are affected by that, yes.

DR. RIDE: Could you give us your understanding of the meaning of Criticality 1 and just an estimate of how many parts in the SRM system are classified as Crit 1?

MR. McDONALD: I don't have the number for you, but there is a tremendous amount of Crit 1s on the SRM, primarily because the motor doesn't have very many redundant features. The solid rocket motors don't. And, of course, a lot of it is structure and structure all becomes Crit 1 if it fails.

DR. RIDE: Could you define a Crit 1?

MR. McDONALD: Crit 1 is a single point failure that if that element fails there is no recovery. You lose the hardware and it is catastrophic. There is a lot of elements in the SRM that are under that category. Relative to the O-ring

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seals, the redundancy was built in, because that was a critical element, as the pressure seal, in order to provide that redundancy.

I think there is some confusion of late as to what that Crit 1 and 1R was relative to the seals and how it was removed. I wasn't involved at the time, but it was found that because this joint rotated that if you took all of the worst engineering tolerances and dimensions that were allowed by the prints—and that means the maximum clevis opening you could have to start with, the smallest and thinnest tang that you could put into that clevis opening, the minimum O-ring that you could put in there with the maximum size of the grooves you could possibly put in there—that you put all of those combinations together, you could show that you would actually lose squeeze on the secondary O-ring once you pressurized the system and the joint rotated.

And, therefore, if you ever had to call upon that O-ring as a redundant system during that time period, you couldn't depend on it.

Now I went back and had an assessment made of have we flown any hardware in that condition, because I was a bit shocked about that, as much as anyone, when I first found that out, which was, by the way, last August

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when I made that presentation here at NASA headquarters. I was unaware of it myself, that we had such condition, that we were flying with a Crit 1 on that part of the hardware.

Based on the information that I have received on all of the joints ever flown on the shuttle, there was only one time where we had a tolerance stackup of a joint that would have fallen from a Criticality 1R to a 1, and that was on STS 4. We had never had one since. We didn't have one before that. We always had adequate squeeze from the hardware when it rotates to maintain it as a Criticality 1R.

DR. RIDE: I'm not quite sure I understand that, because I thought that the piece of paper in the critical items list specifically lists the primary O-ring as a Criticality 1.

MR. McDONALD: Yes, it does, and the reason it does that is because the drawing tolerances allow you to install hardware that may come together on a worst case basis that cannot guarantee a secondary seal. And, therefore, if you use all of the hardware that is in the inventory or you could possibly have in the inventory and build to acceptable prints, you could end up with a very small fraction.

It is a very small fraction that could go

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together that way, and therefore that document was written to cover all that. But in reality other than that one case we never assembled hardware that way. We haven't had the thinnest tangs with the widest open clevises with the minimum O-rings ever put together. That is a concern, and the seal is a very serious concern, in my opinion, because that decision was made at the time based upon what we knew about joint rotation.

Since that time, we're getting this information on resiliency and the effects of temperature, which were never put into that at that time, that brings that question back out in front. I told you we didn't fly from a Criticality 1R and 1. Well, that is true, based upon the decisions and the information that was known that changed it from a 1R to a 1 just upon the tolerances and dimensions.

As far as the resiliency thing, I can't assess that yet well enough to know, but I do know we never stacked hardware together that had tolerances so bad that we didn't have contact of the secondary O-ring other than one joint.

DR. RIDE: But, just to be clear, I guess, you don't on a joint-by-joint basis go back to the NASA board and kind of reclamma the Crit 1 and turn it back into a 1R, do you? It is always classified as a

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Criticality 1 on the primary seal?

MR. McDONALD: That's absolutely right. You are correct. Now we do, as part of our flight readiness review, present those joints that we are mating and what those O-ring squeezes are on every flight.

DR. RIDE: Did any discussion of the primary seal as a Criticality 1 come up during your meetings on L-1?

MR. McDONALD: No, they didn't.

DR. RIDE: Thank you.

DR. FEYNMAN: You suggested that the secondary seal would not be much affected by the temperature, but now you are telling us that because of the complete or nearly complete loss of resilience—that is, the tendency to spring back—the secondary seal would require very little rotation to open. Do I understand that correctly?

MR. McDONALD: I said it wouldn't be as affected as much as the primary seal because it doesn't have to move from one end of the slot to the other. But as far as the effect of resilience, you are absolutely correct. It still has the same problem. As far as extruding in the gap, it has still got the same problem.

DR. FEYNMAN: We were talking about——

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somewhere there was a discussion about the secondary seal being redundant until the metal parts rotate. When the pressure starts to build up, it can't move the primary seal until there is some pressure, and then there's a very small pressure, perhaps, and a very small rotation.

Isn't it true that the rotation is more or less proportional to the pressure, or is there some delay of some sort? Why is there a time delay between the two?

MR. McDONALD: There is some delay because the joint is stiff enough that under certain pressures you just don't move it at all until you have to build up some pressure to make any rotation. So there is a delay in that time period. But at some point in time it becomes a direct function of the pressure.

DR. FEYNMAN: Isn't the laws of elasticity such that everything is proportionate to the force and all of the spaces are proportional to the force? Wouldn't it be true that at every pressure there is some rotation and the rotation is more or less proportional to the pressure?

MR. McDONALD: Well, I think when you are down to a few psi or 50 psi, I don't think you are rotating anything.

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DR. FEYNMAN: You are rotating it one-tenth as much as you rotate it at 500 psi; is that right?

MR. McDONALD: Well, the structure is so stiff that I can't believe you rotate it at all at 50 psi. You are moving the O-ring seal back into the groove, however.

CHAIRMAN ROGERS: Just to go back to the Criticality 1 so that I can understand it a little better, in, what, 1985, a change was made?

DR. RIDE: 1983.

CHAIRMAN ROGERS: I guess it was 1982 or 1983. At that point, it was listed as Criticality 1 with an R. Is that right?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: Now, does that mean that it was decided that if there was a failure in that seam, O-ring, that the mission and the crew would be destroyed, the whole thing would be a catastrophe? But at that point the R meant that there was a redundancy there, that you had two rings and each at that point was considered to support the other, or at least one was a backup for the other?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: That is what "R" meant?

MR. McDONALD: That is correct. The "R"

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means redundant.

CHAIRMAN ROGERS: Now at that time experience merely demonstrated that the analysis which you had been using up to that point may not have been correct, so that instead of having both a primary and a secondary seal which provided redundancy, you came to the conclusion you didn't have a redundancy. Therefore, it was changed to Criticality 1; the "R" was removed. Is that correct?

MR. McDONALD: That is correct.

CHAIRMAN ROGERS: And so from that point on, I believe it was in—what is the date—December 1982, those shuttles have been flown on the basis of Criticality 1, so if there was a failure of that seal a catastrophe could result because there was no redundancy; is that correct?

MR. McDONALD: Well, I guess that is the interpretation of that. But recognizing the actual hardware that was put together and what we knew at that time, I don't believe that that was true.

But what was true was that the drawings, as I mentioned, allowed the condition where that, you could fly that hardware in that condition.

DR. RIDE: But, just to be clear, what the CIL says is that the primary O-ring is a Criticality 1 and

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you're not allowed to consider the secondary O-ring as a backup to that?

MR. McDONALD: That is true, Sally. That's absolutely correct. That is what is meant.

CHAIRMAN ROGERS: And that was known by everyone, I assume, who was working on the program—or most of the top people working on the program. Would that be true?

MR. McDONALD: Well, I kind of thought I was one of the top people working on the program, and I didn't know that until August of 1985 when I put that presentation together. I presume it was. I did find out that there was some disagreement between both Thiokol and Marshall in interpreting that relative to the joint rotation.

The joint rotation that was used for concluding that was obtained from two different sources. One of those sources was from the structural test article 1, the first structural test article—and this was way before my time. But where this test article was taken to Marshall and it is basically a forward segment—it is unloaded—and an aft segment. And where they input both the prelaunch loads and they put flight loads and all that business into the test article.

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And this test is run horizontally and they measure through the leak check port, I understand, or some places, the actual rotation of these parts. And they got some fairly high numbers, up around 60/1,000ths, I believe, is what they said it rotated. We had run some tests ourselves in a vertical assembly during the hydro test, and measured some of the rotations and got numbers like 30 to 40/1,000ths. So there was some disagreement as to which numbers were correct.

Some of our people felt that the horizontal assembly may have made the structural test article numbers not representative and the vertical assembly was better. And in fact, because of that disagreement, we are in the process—and started early last summer at the direction of Marshall—to come up with what is called a referee test, to do some very careful instrumentation in a vertical assembly to get a better handle on the exact amount of joint rotation.

Those joint rotations that are used are those that are predicted for the maximum expected operating pressure, which, by the way, we have never obtained, fortunately—and I hope we never do. But that is a 3 sigma out there someplace. But that still is a point of controversy as to what that number really is, because

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the CIL, I believe, the critical items list, that changed that from a 1 redundancy to a 1 not only assumed the MEOP but I believe it assumed the larger rotation, which is about 50 percent more than some of the data that we got from vertical.

So I still don't fully understand that. What I do understand is that it was made at the time when temperature effects were not known. And the resiliency problem, as Dr. Feynman brought up, I think is a more serious one.

CHAIRMAN ROGERS: Going back, though, beginning in 1985, when you realized that the Criticality 1 was on the critical items list, you knew from that point on that a single failure would be a catastrophe as far as the mission was concerned?

MR. McDONALD: Well, as I mentioned earlier, you have to postulate what your failure mechanism is, and I felt very strongly that the blow-by that we had observed indeed was on the early part of the ignition transient. I think we all felt that way. We modeled that, by the way, I think fairly reasonably on the erosion.

And at that point in time, when the hardware really hasn't had a chance to rotate, taking the temperature effects out now, that we do indeed have a

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good, redundant seal, because if we lose the primary O-ring at that time that secondary seal is in a better shape than the primary to really seal. It hasn't come off its land at all, if you ignore the resiliency problem.

DR. FEYNMAN: That is your opinion. In a different scenario, suppose the pressure is increasing for some reason in the primary seal. Just to understand what the situation is, the primary seal begins to erode, and by the time the pressure gets to some figure like 600 or 700 psi, which is I think just below the maximum operating pressure, it finally erodes all the way through so that the gas can pass through the primary seal.

Would you think there was a reasonable probability that the whole thing would fail because the rotation by that time was enough that the secondary seal can't hold it?

MR. McDONALD: Yes, I think there was, and that was our assessment in August—that there was a reasonable probability.

DR. FEYNMAN: Okay. That you first knew or thought of in August 1985?

MR. McDONALD: That is correct.

DR. FEYNMAN: Not before?

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MR. McDONALD: That is correct.

CHAIRMAN ROGERS: Following up on that, wouldn't you assume that other people who had been working on the program and working on the seal and the O-rings would understand that, too?

MR. McDONALD: Yes. I think there is another important piece of information. Back in the end of April 1985—I forget the exact flight, but it was the last one in April—we had a problem with the O-ring seal in the nozzle.

That flight, after it came back, the nozzle was still attached to the aft segment and we disassembled the segments when they were brought back to the port. And then they are put on rail cars and sent back to Utah and we never got the segment back until, I don't know, sometime in the latter part of June or something. This flight was at the very end of April.

We disassembled that nozzle and we found that we had violated the primary seal, in fact in three locations, and burned completely through. The secondary seal also had eroded like 32/1,000ths, but it held and it did its job. That was in a nozzle joint.

The primary seal in the nozzle joint is a bore seal quite similar to the field joint. Tolerances are a little different. But the secondary seal is a face

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seal. They are both somewhat dynamic seals, but it is more of a static than the bore seals. The face seal is torqued down with bolts and it obviously did its job.

And our conclusion was, in that particular instance, was that even though we passed the leak check that the vacuum putty may have masked that leak check. We were using at that time a pressure, a stabilizing pressure, to run that leak check at 100 psi. We had had some data earlier that showed us that the consistency of that putty is quite variable, and you can actual-

ly—the putty will become a good sealant in many cases and it can hold as much as 100 psi for a good period of time. Sometimes it will blow through and sometimes it won't.

And so our concern was that maybe we were not really checking the O-ring seals. We were checking the putty and we wanted to make sure we were checking the O-ring seals. We jacked the pressure up to 200 psi and that was the last flight where we were still under the 100 psi, which was the previous requirements, to stabilize at 100. And then once you have stabilized it for a period of time there, you drop the pressure completely again and you bring it up to 50 psi, and that is what you run the leak check out for about ten minutes. And we only allow a one psi drop.

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In that particular set of hardware, since that was the last one we ran at 100 psi because the change hadn't gotten through with all of the paperwork system on what we understood about how putty behaves under pressure, as we went to 200 psi for the field joints at that time, we just hadn't gotten the nozzle, we concluded that the putty may well have masked that leak check, because it was at 100, and therefore we may have had some defect in that seal that we didn't detect.

And as a result, that seal leaked at ignition from time zero, and when that happens you get severe erosion in the O-ring, because you get the jet impingement that we have been calculating, which is directly onto the O-ring seal, like a flat plate. But when you bypass the O-ring in a leak like that, it actually chokes at that point, so you're forming a throat and it erodes very rapidly from underneath, as well as jet impingement, and you lose quite a bit of the seal.

We lost most of the seal, in fact. But the nozzle seal is very good, the secondary seal, because it is around the corner and it expands and goes down around the corner. And it eroded some of the secondary seal.

We ran a lot of analysis after that, because that was the first time that we had observed erosion of

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a primary seal. We developed our models to predict erosion, what would happen in blow-by erosion, and it was because of that that we found that hardware.

And it was late in June and it was into July that we got all that data together, and we went down to Marshall and had very detailed reviews, because that was a serious concern to everyone that we had violated the primary seal that was in the nozzle joint.

But we were asked to come to Washington, to headquarters, and review that. It was a very serious concern. It was at that time that I got together with the engineering people and said, we need to put the whole story together on all the pressure seals, because I and several others still felt that, even though that problem happened with the nozzle, the field joint was a more serious concern, because I felt very good about the secondary seal and the nozzle; I didn't with the field joint.

And I put together a presentation for August that I think showed where our real concerns were and why we felt the highest priority was the field joint even though we had just experienced that problem with the nozzle, we felt we had corrected the problem with the nozzle because all subsequent flights had the 200 psi leak check, so that we could not miss a possible defective seal or a contamination that may have

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prevented that seal from seating in the first place.

But that was some of the background that I think really turned up the gain on the whole seal issue, from about April, really, and June on. Prior to that time, we were doing a lot of work to better understand this rotation business, but we have done a tremendous amount of work.

In fact, you may have read in the press, and it is true, that we actually went to an SAE conference last fall, a couple of our top engineers went there, to try to get help from the whole seal industry and SAE about the field joint: Is there a better way to seal that joint?

CHAIRMAN ROGERS: What does "SAE" mean?

MR. McDONALD: The Society of Automotive Engineers.

And we did a lot of work to try to get help from the outside, as well as do the work that we had inside. And I think that is all relevant to the discussion.

CHAIRMAN ROGERS: Were you surprised that some of the top people in the decisionmaking process didn't know about this at all?

MR. McDONALD: Didn't know about the CIL or the criticality one or what?

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CHAIRMAN ROGERS: Well, the whole thing, the concern on your part and on the part of Thiokol's engineers, that the original decision had been not to launch and all of that.

MR. McDONALD: I'm very surprised at that. I hardly believe that. The issue is so controversial I thought that I'm sure that they were aware of it. I have a hard time believing they didn't.

MR. SUTTER: Could I ask one question? In talking about criticality one and going to 1R, I think you made the comment that you would have to have all of the tolerances going in the wrong direction to meet that condition, but you did say that one unit did meet that condition?

MR. McDONALD: Yes, to the best of my knowledge. And I had some record search done. I was told that there was a condition in the—I think it was the forward field joint of the STS-4 someplace, that was in that condition.

MR. SUTTER: What if that launch had been done at a cold temperature? Wouldn't it have maybe taken it over the edge?

MR. McDONALD: I don't know. It is possible. I certainly don't feel good about that.

MR. SUTTER: The reason I ask the question, it

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seemed to me that if it was a Criticality 1 item then everybody should treat it as Criticality 1 and not rationalize that it may not ever happen.

MR. McDONALD: I agree with that.

MR. SUTTER: You think everybody else agrees with that?

MR. McDONALD: I think our engineering people agree with that, and that is why the recommendation we had made originally. That was the basis for that recommendation. We felt that we had observed a condition a year earlier that we did feel was attributed to temperature in some way or another and it was not a good condition, and we didn't want to go much beyond that, because, even though that one was successful, it certainly wasn't a good condition.

MR. ACHESON: Mr. McDonald, was it NASA or Thiokol that originally initiated the change from Criticality 1R to Criticality 1 for the primary O-ring?

MR. McDONALD: I have to tell you, based upon what I was told, because I wasn't involved in that change, but I was told that that was NASA-initiated.

MR. ACHESON: What experience led to that change, do you know?

MR. McDONALD: I am not sure of what

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experience led to the change. I presume it was the understanding from the joint rotation data and the drawing tolerances of all of these parts that that condition could exist someplace in the hardware, and therefore if we were going to fly the hardware as it was designed that, either through refurbishment or new hardware that was within those tolerances, may be matched together some time to give us a condition like that.

But I think you would have to ask the people that were involved in that particular decision.

MR. ACHESON: How do you account for what appears to be the fact that between December 1982 and the middle of 1985, when Thiokol became concerned about the erosion and the blow-by, that flights did not occur seemingly during that time that led to the same concern?

MR. McDONALD: I can't explain that. I certainly didn't know about it until August 1985. I don't know. I think that everyone was concerned, at least from the standpoint that we always had to present data on the joints that we were mating to show that we had adequate squeeze and all.

That data was presented, and I'm sure the people were concerned about that.

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MR. ACHESON: Thank you.

CHAIRMAN ROGERS: If there are no other questions, we will have a ten minute recess.

(Recess.)

CHAIRMAN ROGERS: Will the Commission come to order, please.

If I could ask Mr. Mason to take the stand.

(Witness sworn.)

Typed version of handwritten notes of A. J. McDonald, Manager Shuttle Rocket Motor Project, Morton Thiokol, Inc.

A. J. McDONALD
DIRECTOR SOLID ROCKET MOTOR PROJECT
MORTON THIOKOL, INC.

Notes from January 27, 1986

Late afternoon on January 27th

While staying at Carver Kennedy's residence in Titusville, Florida, I received a call from Bob Ebeling, Manager, Solid Rocket Motor Project Officer, Igniter and Final Assembly [Thiokol, Utah], about concern of low temperature predicted at the Cape for the launch of [shuttle launch] STS-51L the next morning. Concern was associated with performance of the field joint O-rings. I told him I would obtain the latest weather information and predicted temperatures up to launch time and call him back. Carver Kennedy, Vice President Space Services at Kennedy Space Center for Morton Thiokol, Inc., called the Launch Operations Center and received the latest recorded temperatures and temperature predictions up to the planned launch time of 0938 EST on 28 January 1986. Freezing was expected to occur before midnight with a minimum temperature of 22° F expected by 6:00 a.m. with around 26° F at launch time. I transmitted this data back to the plant and said I would set up a meeting on this subject as soon as possible with Kennedy Space Center and Marshall Space Flight Center. I told Bob to make sure Bob Lund, Vice President, Engineering [Thiokol, Utah] was involved in making this decision because it should be an engineering decision not program management. I felt that I was not in a position to make the assessment or recommendation. I told Bob Ebeling that engineering needs to prepare some charts on this matter and FAX [telefax] them to Kennedy Space Center and Marshall Space Flight Center. I told Bob to make sure engineering is prepared to recommend an acceptable launch temperature with the data and rationale supporting the

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[ ] brackets denote comments added to author's notes for clarity.

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recommendation. I then called Larry Mulloy, Solid Rocket Booster, Project Manager [Marshall Space Flight Center] at the Holiday Inn at Merritt Island [Florida] and they could not reach him. I then called Cecil Houston, Marshall Space Flight Center/Resident Manager Office at Kennedy Space Center and told him our concern about the low tempertaures on the O-ring seals. He said he would set up a 4-wire teleconference on this subject with Marshall Space Flight Center and Morton Thiokol, Inc. through his conference room in Complex C at Kennedy Space Center. The teleconference was set up for 8:15 p.m. EST and I told Cecil Houston I would support the conference at that time. I relayed this message to the plant.

8:15 P.M. EST.

I then left Carver Kennedy's house and went to Cecil Houston's conference room arriving about 8:15 p.m. Those present were Cecil Houston, Larry Mulloy, Stan Reinartz, Shuttle Project Manager [Marshall] Jack Buchanan, Manager Kennedy Space Center Operations for Morton Thiokol Inc, and myself. The charts from Morton Thiokol Inc. had not been received and the 4-wire call came in shortly thereafter and we suggested all parties hold because the FAX transmittal had not yet been received at Kennedy Space Center. The FAX started coming through around 8:40 p.m. (EST) and we waited several minutes until most of the charts were transmitted and copies made for distribution to attendees. The meeting started just before 9:00 p.m. (EST) before all the charts, were received. The conclusions and

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[ ] brackets denote comments added to author's notes for clarity.

recommendations were not received until approximately 9:30 p.m. (EST).

The presentation was made by Mr. Bob Lund, Vice President, Engineering at Morton Thiokol Inc. (I think). The charts that he used presented the history of O-ring erosion and blow-by of the primary seal in the field joints, reasons for concern at lower temperatures, results of subscale tests, previous flight and static tests of Solid Rocket Motors and conclusions and recommendations. The data presented by Morton Thiokol Inc. showed that the timing function to seal the O-rings would be slower due to lower temperatures (Chart 2-2) and that the worst blow-by part a primary O-ring occurred on SRM-15 (STS-51C) [Solid Rocket Motor-15 on Shuttle Flight STS-51C on January 24, 1985 1:50 p.m. EST] which was flown about a year ago and had the coldest O-rings of any of the flight motors (Chart 6-1). Morton Thiokol Inc. recommended not to fly STS-51L (SRM-25) 51L with Solid Rocket Motor-25 scheduled for launch next morning on January 28] until the temperature of the O-rings reached 53° F or higher. This temperature must be calculated based upon local ambient temperature and wind conditions. This recommendation was basically made on our successful experience base.

NASA (George Hardy (I think), Deputy Director Science and Engineering at Marshall Space Flight Center) said he was "appalled" by Morton Thiokol Inc.'s recommendation. Larry Mulloy then commented that we were trying to establish a new Launch Commit Criteria which we couldn't do: Launch Commit Criteria's are pre-established sets of constraints.

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Stan Reinartz, also commented that he was under the impression that the Solid Rocket Motor was qualified from 40° F to 90° F and the 53° F recommendation was not consistent with that. Larry Mulloy also commented that if we live to a 53° F requirement then we will never make 24 launches per year because many of the launches occur early in the morning where 53° F is not uncommon. Furthermore, we may never get a launch off at Vandenburg Air Force Base under the 53° F constraint levied by Morton Thiokol Inc. and we don't know when we will ever get STS-51L launched [planned January 28 launch]. Cecil Houston commented that it would probably be Thursday, January 30th before morning temperatures (i.e., 9:36) would reach that condition.

NASA challenged Morton Thiokol Inc.'s 53° F recommendation based on the data presented on Charts 1-1 and 6-1. These charts showed that the next worst blow-by of a primary O-ring occurred on SRM-22 (STS-61A) [Solid Rocket Motor-22 on Shuttle Flight 61A launched on October 31, 1985 at 11:00 a.m. EST] which had the highest temperature (75° F) and that we had static tested motors (DM-4) at even lower temperatures (36° F) with no observed blow-by or O-ring erosion. Based on these data, NASA felt the temperature effects on the O-rings were inconclusive. "I commented that I didn't believe that the static

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test history was a valid test for the O-rings because the blow-by holes observed in the zinc chromate putty after assembly and leak check were manually filled prior to static test." Someone from Morton Thiokol Inc. commented that even though SRM-22 [75° F] was the second worst blow-by observed, the severity of the blow-by was not nearly as pronounced as SRM-15 [53° F]. Soot observed behind the primary O-rings on SRM-15 was much blacker, covered a larger area, and there was some heat affect [effect] without any measurable erosion observed on the secondary O-ring of SRB-15.

Further discussion centered around the inconsistency of the data presented in the charts relative to temperature effects. The data presented in the charts, namely Chart 1-1, 4-3 and 6-1 were considered inconclusive and NASA suggested that Morton Thiokol Inc. reassess their recommendation of 53° F for a launch temperature based upon a re-examination of all the data. George Hardy (I think) said he would not consider flying without Morton Thiokol Inc.'s concurrence. "I commented that lower temperatures are in the direction of badness for both O-rings because it slowed down the timing function but that the affect [effect] is much worse for the primary O-ring compared to the secondary O-ring because the leak check forces the primary O-ring into the wrong side of the groove

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[ ] brackets denote comments added to author's notes for clarity.

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while the secondary O-ring goes in the right direction and that this condition should be evaluated in making the final decision for recommending the lowest acceptable launch temperature." Based on the data prescribed in Chart 2-1 I considered this very important because depending on how much delay one has in getting a good reliable primary seal affects the capability of the secondary O-ring to seal.

Morton Thiokol Inc. decided at that time to hold a caucus off-line to re-evaluate all the data they had at their disposal and come back with a re-assessment of the temperature conditions acceptable for launch. Morton Thiokol Inc. planned to be off-line for approximately five minutes but were actually in a caucus in the MIC No. 1 at Wasatch for nearly a half hour.

While we were waiting at Kennedy Space Center for Morton Thiokol, Inc. to Caucus, Mr. Mulloy, Mr. Reinartz and myself engaged in a discussion concerning the qualification temperature of 40° F to 90° F for the Solid Rocket Motor." I told them that I wasn't involved in the qualification of the steel case Solid Rocket Motor's but based upon my experience with the Filament Wound Case-Solid Rocket Motor,

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[ ] brackets denote comments added to author's notes for clarity.

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I don't believe that every component and element of the Solid Rocket Motor is qualified to 40° F. My interpretation of the Spec is that 40° F to 90° F is the operating temperature range and includes all components of the Solid Rocket Motor." Larry Mulloy commented that the 40° F requirement applies to Propellant Mean Bulk Temperature only and that we will be at 55° F for Propellant Mean Bulk Temperature at time of launch. He said that means other components could be below 40° F as long as the Propellant Mean Bulk Temperature never dropped below this value. I told him that is ridiculous because the propellant is such a massive insulator that it never changes Propellant Mean Bulk Temperature even with tremendous external temperature extremes and I'm sure the Spec didn't mean that.

While we were waiting for Morton Thiokol Inc.'s response, I suggested that NASA recommend delaying the launch until late afternoon when temperatures are expected to reach 48-50° F. I told them that I understand that this would be an acceptable launch window since the original launch time for STS-51L was set at 3:45 P.M. (EST.). I was told that this was considered and rejected because of problems with weather and/or [shuttle flight] visibility at one of the TAL [Trans Atlantic Landing] abort sights, I believe Dakar [Senegal].

[end of page 7 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 7 of 24]

Since the temperature data appeared to be inconclusive based on the charts presented, I expected Morton Thiokol Inc. to either find some more supporting information for the 53° F temperature requirement or evaluate lower temperatures and make a recommendation and that's why it was taking so long. I really suspected that the new recommendation would be 40° F because of the discussion concerning the qualification requirements for the motor unless some new calculations could be made to obtain a better number.

Morton Thiokol Inc. finally came back on and said that they had re-assessed the data and concluded that even though the lower temperatures were concerning that the temperature affects were inconclusive and therefore Morton Thiokol Inc. recommends launching. I believe Mr. Joe Kilminster, Vice President, Space Booster-Programs, at Morton Thiokol Inc. was the speaker. Mr. George Hardy (I think) suggested that Morton Thiokol Inc., put that in writing and send it by FAX to both Kennedy Space Center and Marhsall Space Flight Center to be available by morning. I told Mr. Mulloy that I would not sign the letter that it would have to come from the plant.

[end of page 8 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 8 of 24]

Joe Kilminster (I think) said that they would draft a letter and send it out right away on the FAX to both Kennedy Space Center and Marshall Space Flight Center. I was instructed to stay and receive the letter and deliver it to Mulloy, Reinartz, and Houston. I asked Cecil Houston where the FAX was and he told me it was at the other end of the building.

While we were waiting for the signed FAX, I made several comments to Mr. Mulloy, Mr. Reinartz, and Mr. Houston with Jack Buchanan present. I told them I didn't feel good about the recommendation and that based upon the data presented no one could really define an exact temperature at which this seal problem may be unacceptable. However, even though I did not agree with the position that all components of the Solid Rocket Motor were really qualified to 40° F, I understand that many people at both Morton Thiokol Inc. and NASA signed up to that based upon the CDR [Critical Design Review] and DCR [Design Certification Review] for the steel case motor. I was surprised that NASA accepted a recommendation to fly at a temperature well below the qualification temperature; predicted temperatures at launch time (9:38 a.m. EST.) were expected to be around 26° F.

[end of page 9 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 9 of 24]

I further stated that I may be naive about what generates a Launch Commit Criteria but I was under the impression that it was generated based upon launching within the qualified environments of all elements and subsystems of the Space Shuttle. Because of that, I don't understand why NASA would allow a Shuttle launch below 40° F. I told them that if anything happened, I sure wouldn't want to be the person who had to explain to a Board of Inquiry why it was all right to launch at a temperature below that at which the system was qualified to. No one in the room commented on that statement. I was still so upset at the decision that I asked for reconsideration for postponement of the launch because there were other considerations which were bad in addition to the O-ring seal problem. I stated that if I were the Launch Director, I would cancel the launch for tomorrow morning for three reasons:

(1) The possible effects of the O-ring sealing problem at low temperatures that we just discussed.

(2) I had just left Carver Kennedy's house to come to this meeting and had just received a report from the booster recovery ships.

[end of page 10 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 10 of 24]

The ships were in a absolute "survival" mode in 30 foot seas, sustained winds of 50 knots gusting to 70 knots, pitching 30 degrees and they thought the rough seas may have damaged some of the recovery equipment on the back of the ships. The ships were heading straight into the wind toward shore for survival and could not support tomorrows planned launch at 0938 A.M. (EST). The ships had been moving away from the booster impact area at approximately three knots for sometime and didn't know when conditions would be safe enough to attempt to turn around. I then reminded everyone that this was the first launch with apogee separation of the nozzle exit cone and separation of the parachutes from the boosters at water impact. Launching early the next morning would put the booster recovery at some risk and would most likely eliminate any real possibility of recovering the frustrums and parachutes.

(3) The third reason for not launching was the expected formation of ice in the Launch Complex area. I told them that I did not know what affects that this may have on accoustics, debris, or structures but it didn't seem prudent to launch under these unknown conditions.

[end of page 11 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 11 of 24]

I was told that these were not my problems and that I shouldn't concern myself with these. I responsed that maybe one of these problems was not being considered sufficient to cancel the launch but that all three of them combined should be more than sufficient to delay the launch. The NASA folks indicated that they would pass this information on in advisory capacity. Mulloy then asked me where the signed FAX was and I told him I would go check on it.

The FAX had not come in yet so I waited at the FAX machine for a while and wondered if it was working because it had been sometime since the teleconference had been completed. The FAX finally come in at 11:27 p.m. (EST) while I was waiting at the machine. I took the FAX to Jack Buchanan's office where he reproduced several copies for distribution to the attendees, of the meeting.

I returned to Cecil Houston's office where Mulloy, Houston and Reinartz were on a telecon. I believe with Mr. Arnie Aldrich. They were discussing the condition of the booster recovery ships at sea

[end of page 12 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 12 of 24]

and concluded that a launch decision would have to be made based upon the reasonably high probability that the parachutes and frustrums (approximately $1 million of hardware) would not be recovered but there was was no significant increase in risk of retrieving the boosters. Mulloy was asked if the could tolerate the loss of the parachutes and frustrums and still support the launch schedules and he said yes. A decision was made to proceed with the launch without the recovery ships in the area and to make sure we didn't jeopardize the safety of the ships by trying to return to the recovery area in such bad sea conditions.

They also briefly discussed the ice issue in the launch complex area and were told that the ice issue had been addressed earlier in the day. Mr. Mulloy and Mr. Reinartz made it clear that they were acting in an advisory capacity only and wanted to make sure that all this information was made available. The decision was to proceed with the early morning launch and the telecon was concluded. I did not hear any conversation on the O-ring concerns but I presume that discussion occurred while I was at the FAX.

[end of page 13 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 13 of 24]

I gave copies of the FAX signed by Joe Kilminster with the recommendation by Morton Thiokol Inc. to proceed with the launch of [shuttle flight] STS-51L on 28 January 1986. I was surprised that the FAX did not address <u>any</u> temperature conditions at all. It contained a breif summary of some of the Pro's and Con's associated with colder temperature and recommended proceeding with the launch on 28 January 1986 without reference to any particular temperature or time. The meeting broke up and I spent a few more minutes with Jack Buchanan and returned back to Carver Kennedy's house in Titusville arriving there between 12:30 and 1:00 a.m. (EST.) on 28 January 1986.

A.J. McDonald
(Signed)

2/13/86

Since recording the events the night before the launch I came across a piece of paper in my suit pocket that was written by me when Morton Thiokol Inc. made their final recommendation. I have included as page 14A in these notes.

A.J. McDonald
(Signed)

[end of page 14 of notes]

[ ] brackets denote comments added to author's notes for clarity.

[Ref. 2/25-1 14 of 24]

P-14A

Jan. 27

MORTON THIOKOL INC. RECOMMENDATION

Temperature data is inconclusive

Risks

(1) Cold O-ring moving into seating position and opportunity for gas going between two calities.

(2) Secondary O-ring problem after primary O-ring seated.

(3) Increased risks due to hardness not much different than on SRM-15 -- If took twice as long would get twice erosion = 0.070 - 0.080" can accommodate.

(4) Primary will seal - longer time - some gas possible. If primary doesn't secondary will. Proceed with launch under cold conditions.

WRITE THIS DONW, SIGN IT AND FAX OUT.

[signed A. J. McDonald in bottom right corner]

[Ref. 2/25-1 15 of 24]

HOW SHOULD WE ATTACK THIS PROBLEM? AJ McDONALD 4Feb'86 10:30 AM

(A) o LOOK AT ENVIRONMENT BEFORE LAUNCH

(B) o LOOK AT ENVIRONMENT DURING PRE-LAUNCH ie SSME BUILDUP

(C) o LOOK AT ENVIRONMENT DURING ~~[illegible]~~ SRB IGNITION AND LIFTOFF

(D) o LOOK AT ENVIRONMENT FROM LIFTOFF TO PROBLEM TIME

(E) o LOOK AT PROBLEM TIME ~ 58.3 SEC.

17 of 25

(F) o LOOK AT ENVIRONMENT AFTER 58.3 SEC PRIOR TO TANK EXPLOSION

(G) o LOOK AT EXPLOSION AND THIS ENVIRONMENT

(H) o LOOK AT SRB'S AFTER EXPLOSION AND THIS ENVIRONMENT

(I) o LOOK FOR SRB DESTRUCT

(J) o LOOK FOR OTHER POSSIBILITIES ie IMPACTS, DEFECTS, OR SOMETHING TOTALLY DIFFERENT

OBJECTIVE: CAN WE EXPLAIN ALL THE DATA AND FILM EVENTS WITH A SINGLE SCENARIO THAT MAY OR MAY NOT INVOLVE MULTIPLE FAILURES?

[Ref. 2/25-1 16 of 24] 1

A.J. McDONALD
4 FEB '86
10:30 AM

ENVIRONMENT BEFORE LAUNCH (A)

- O WEATHER FROM T-1 DAY AND EARLIER
  - O RAIN ON PAD AT VARIOUS TIMES
  - O HEAVY RAIN DAY BEFORE LAUNCH
  - O NEAR HURRICANE WINDS DAY BEFORE LAUNCH
  - → O WITH VEHICLE BENDING AND HEAVY RAIN PROBABILITY IS EXTREMELY HIGH THAT AFT JOINT WAS FILLED WITH WATER i.e. LIKE NOTED IN STS-9 DESTACK
  - → O AFT JOINT MOST LIKELY TO TRAP WATER DUE TO RUNOFF FROM ABOVE + BENDING
- O WEATHER DURING LAUNCH DAY
  - O SUB FREEZING TEMPERATURES FOR ~~10~~ 12 HR PRIOR TO LAUNCH
  - O WIND REPORTED FROM NORTH/NORTHWEST ?
  - O WATER IN AFT JOINT FREEZES FROM OUTSIDE TO INSIDE
    - → O SPREADS CLEVIS OPEN SLIGHTLY
    - → O PUSHES SECONDARY O-RING FORWARD OFF SEALING SURFACE
    - → O FILLS SECONDARY EXTRUSION GAP WITH ICE
- O WHAT IS DIFFERENT BETWEEN LH AND RH SRB ?
  - O IR TEMP. MEASUREMENT ON LH = 23-25°F at 7:00 AM
  - → • SAME IR TEMP. MEASUREMENT TAKEN ON RH = 7-9°F AT 7:00 AM
  - → O SUGGESTS CRYO LEAK IN AREA OF RH SRB OR PREVAILING WINDS BLOWING CRYO VAPORS ON RH SRB
    - → O COULD BE FROM VEHICLE
    - O COULD BE FROM NEW LAUNCH FACILITY
  - O COULD GENERATE LARGE THERMAL GRADIENTS AROUND RH SRB CASE AND ET ATTACH RING AND ET STRUTS

(2)

[Ref. 2/25-1 17 of 24]

A. J. McDONALD
4 FEB '86
11:00 AM

BNVIRONMENT BEFORE LAUNCH (A) Cont.

- O NEW LAUNCH PAB-B
  - O LAUNCH MOUNT STIFFNESS DIFFERENCES ?
  - O CRYO FEED LINES AND VENT LINE DIFFERENCES ?
  - ⇒ O LOCATION DIFFERENCES BETWEEN PAD-B AND PAD-A CHANGES LOCAL ENVIRONMENT DUE TO WINDS AND SKY RADIATION ?
  - O DYNAMIC RESPONSE CHARACTERISTICS AFFECTED BY TEMPERATURE ?
    - O LAUNCH MOUNT STIFFNESS ?
    - ⇒ O ORDNANCE DEVICES, i.e. EXPLOSIVE HOLD DOWN BOLTS
      - O DID NOT FIRE ALL BOLTS ?
      - O HIGH π_K OF PYROTECHNIC CAUSES SLOW RESPONSE
- O THESE DIFFERENCES COULD EXPLAIN HIGHER THERMAL AND/OR STRUCTURAL LOADS IN RH SRB AT LAUNCH

[Ref. 2/25-1 18 of 24]

A.J. McDONALD
4 FEB '86
11:15 AM

ENVIRONMENT DURING SSME BUILDUP (B)

- O INDUCES HIGH BENDING LOADS INTO SRB'S
  - O RH SRB AFT JOINT AREA
    - O HIGH TENSION LOADS AT 180°
    - • HIGH COMPRESSION LOADS AT 0° AREA

- O PRE-LAUNCH LOADS COULD FURTHER COMPRESS O-RING SEALS

- O PRE-LAUNCH LOADS WOULD PROBABLY CRACK ICE IN JOINTS

[Ref. 2/25 19 of 24]

A.J. McDONALD
4 FEB '86
11:30 AM

ENVIRONMENT DURING SRB IGNITION AND LIFTOFF (C)

- O SRB IGNITION
  - O PRIMARY O-RING LEAKS
    - O SLOW RESPONSE TO SEAL
      - O WRONG SIDE OF O-RING GROOVE DUE TO LEAK CHECK
      - O HARD O-RING DUE TO LOW TEMP (9°F)
        - O MOVES SLOW
        - O CAN'T EXTRUDE INTO GAP
      - O GREASE IN JOINT VERY VISCOUS SLOWS RESPONSE
      - O COLD PUTTY IN FRONT OF PRIMARY O-RING MAY AGGRAVATE PROBLEM
      - O CLEVIS OPENING DUE TO LOW TEMP. AGGRAVATES SEALING CAPABILITY
  - O SECONDARY O-RING CAN NOT SEAL
    - O ICE IN JOINT MAKES TEMPORARY SEAL FOR 0.5 SEC
    - O JOINT LEAKS AT IGNITION AND EXPELLS WATER AND/OR ICE FOR 0.5 SEC (CAN'T SEE ON FILM) UNTIL SMOKE APPEARS
  - O SMOKE OBSERVED FROM 0.5 TO 2.9 SEC
    - O COULD BE UNRELATED, i.e. PYROTECHNIC BOLT FROM ET ATTACH STRUTS OR HYDROGEN BURNING WITH DEBRIS.
    - O COULD BE FROM JOINT AREA
      - O ICE MELTS AND/OR BLOWN OUT BURNED GREASE, etc. APPEARS AS BLACK SMOKE
      - O SECONDARY O-RING LEAK MAY OR MAY NOT OCCUR BEHIND PRIMARY
        - O GAS TEMP. COOLS RAPIDLY CIRCUMFERENTIALLY AROUND CASE
        - O COULD SET UP CIRCUMFERENTIAL EROSION OF SECONDARY
  - O SMOKE STOPS AT 2.9 SEC
    - O EITHER PRIMARY OR SECONDARY O-RING SEALS
    - O DAMAGED O-RING SOFTENS FROM HEAT AND EXTRUDES INTO GAP

5

[Ref. 2/25-1 20 of 24]

4 FEB 86
11:50 AM

ENVIRONMENT FROM LIFTOFF TO PROBLEM TIME (D)

- CHAMBER PRESSURE STARTS PLANNED DECAY FROM 20 SEC TO 54 SEC
  - REDUCED CHAMBER PRESSURE REDUCES SRB STIFFNESS

POSSIBLE HYDROGEN LEAK THAT CAUSED LOW LIFTOFF TEMP (9°F) CONTINUES ~~[illegible]~~
IF LEAK IS ON VEHICLE
- UNBURNED CRYOGENIC HYDROGEN VAPORS COULD IMPINGE ON RH SRB AND ~~CAUSE~~ SUPERCOOL O-RING ~~[illegible]~~
- HYDROGEN FIRE COULD CAUSE O-RING AND JOINT TO OVERHEAT DUE TO EXTERNAL HEATING

VEHICLE ENCOUNTERS HIGH AERODYNAMIC LOADS
- HIGH SHEAR WINDS NEAR FAILURE TIME
- NEAR MAX Q AT FAILURE TIME
- HIGH GIMBAL PROFILE PUTS LOADS INTO AFT JOINT DURING THIS TIME
- HIGH VIBRATIONS AND POSSIBLE RESONANCE IN JOINT AREA
- POSSIBLE DEFLECTIONS IN JOINT AFFECTING SEAL CAPABILITY

[Ref. 2/25-1 21 of 24]

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4 FEB 86
12:15 PM

PROBLEM TIME (~58.3 SEC) (E)

- DAMAGED PRIMARY O-RING FAILURE
  - SECONDARY O-RING OFF SEAT AND CAN'T SEAL IF THIS HAPPENS
  - COULD ESTABLISH CIRCUMFERENTIAL FLOW TO SECONDARY IN ANOTHER LOCATION
- DAMAGED SECONDARY O-RING FAILURE
  - COULD ~~[illegible]~~ HAVE SUSTAINED ADDITIONAL CIRCUMFERENTIAL HEATING WHICH CAUSED FAILURE
  - COULD HAVE FAILED FROM STRUCTURAL LOADING
    - NIBBLED O-RING DUE TO DEFLECTIONS ~~[illegible]~~
    - VIBRATION AND/OR RESONANCE CAUSES LOSS OF DAMAGED SEAL IN EXTRUSION GAP
  - HYDROGEN FIRE COULD DESTROY SEAL INTEGRITY
    - FIRE ON TANG COULD ~~[illegible]~~ INCREASE CLEVIS OPENING
    - O-RING FINALLY BURNS AND LOSES MECHANICAL INTEGRITY
  - ~~CRYOGENIC~~ HYDROGEN COOLING COULD CAUSE SEAL FAILURE
    - THERMAL SHRINKAGE OF O-RING
    - CRACKING OF O-RING SEAL
    - THERMAL STRESSES IN CLEVIS GAP
    - CRYOGENIC COOLING OF SRB STRUCTURE CAUSES FAILU.

- FAILURE APPEARS TO OCCUR IN ~~[illegible]~~ VICINITY OF 6-7 ~~[illegible]~~ O'CLOCK LOCATION (NEAR 0° TOWARD ET)

[Ref. 2/25-1 22 of 24]

ENVIRONMENT AFTER SRB BURNTHROUGH 4 FEB '86
AND PRIOR TO E.T. EXPLOSION (F) 12:30 PM

- O APPEARS TO SPREAD CIRCUMFERENTIALLY AROUND JOINT
- O PRESSURE STARTS DROPPING (59 sec → 72.45 sec) $\Delta P = 20$ PSI
- O HYDROGEN TANK PRESSURE CHANGES (APPROX. 67 SEC.)
  - ⇒ O INDICATES HOLE IN TANK OR FEED LINE
  - ⇒ O IF CAUSED BY SRB TORCHING ACTION SHOULD EXPLODE?
  - ⇒ O ET DOESN'T EXPLODE UNTIL 6-7 SEC. LATER
  - ⇒ O ~~FO~~ INITIAL HYDROGEN LEAK COULD HAVE BEEN AWAY FROM INITIAL SRB FAILURE POINT AND NOT SEEN — EXPLOSION OCCURS WHEN JOINT FAILURE PROPAGATES AROUND TO THIS LOCATION.

[Ref. 2/25-1 23 of 24]

A.J. McDONALD
4 FEB '86
12:35 PM

OTHER POSSIBILITIES (J)

ORDNANCE FAILURES

LOX TANK OR FACILITY LEAKS

HYDRAZINE FROM SRB TVA, OMS OR IUS

N_2O_4 FROM OMS

PURGE LINES, COLD NITROGEN THROUGH ANY OF THESE

- O SOMETHING MUST EXPLAIN WHY RH SRB IS 9°F WHEN AMBIENT AIR IS 23°F
- O SOMETHING MUST EXPLAIN HOW SRB CUTTING HYDROGEN TANK REQUIRES 6-7 SEC TO EXPLODE

[Ref. 2/25-1 24 of 24]

MTI Assessment of Temperature Concern on SRM-25 (51L) Launch

- 0 Calculations show that SRM-25 O-rings will be 20° colder than SRM-15 O-rings
- 0 Temperature data not conclusive on predicting primary O-ring blow-by
- 0 Engineering assessment is that:
  - 0 Colder O-rings will have increased effective durometer ("harder")
  - 0 "Harder" O-rings will take longer to "seat"
    - 0 More gas may pass primary O-ring before the primary seal seats (relative to SRM-15)
      - 0 Demonstrated sealing threshold is 3 times greater than 0.038" erosion experienced on SRM-15
  - 0 If the primary seal does not seat, the secondary seal will seat
    - 0 Pressure will get to secondary seal before the metal parts rotate
      - 0 O-ring pressure leak check places secondary seal in outboard position which minimizes sealing time
- 0 MTI recommends STS-51L launch proceed on 28 January 1986
  - 0 SRM-25 will not be significantly different from SRM-15

Joe C. Kilminster

Joe C. Kilminster, Vice President
Space Booster Programs

Morton Thiokol Inc.
Wasatch Division

Information on this page was prepared to support an oral presentation and cannot be considered complete without the oral discussion

[Ref. 2/25-2]

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TESTIMONY OF JERRY MASON, SENIOR VICE PRESIDENT, WASATCH DIVISION, MORTON-THIOKOL, INC.

CHAIRMAN ROGERS: Mr. Mason, you requested that you appear after Mr. McDonald, and we are glad to welcome you, and go ahead and proceed any way you would like.

MR. MASON: Thank you, Mr. Chairman. We have given you a handout there, and I wanted to start on page 18. We had originally thought to provide some technical background, but that could be covered later.

(Viewgraph.) [Ref. 2/25-3

CHAIRMAN ROGERS: Could I say that we have of course received quite a lot of this information in executive session, and we will certainly be pleased to look at the information on this first 18 pages. But proceed in any way you would like.

MR. MASON: Thank you. My intent here is to explain the organizational structure and those people that participated in the discussions, and then go through the chronology of the occurrences that night on the 27th of January.

First off, I am the Senior Vice President of Wasatch Operations. We have on the chart you have there selected only those people that participated in

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the discussions.

I might point out that there are three divisions reporting to me: the space division, under Mr. Wiggins, who is listed there; and there are also a tactical and strategic division reporting to me. Mr. Lund, who shows there as vice president, engineering, provides engineering services for all three divisions.

We have covered just the space division in detail. You will see that Mr. Kilminster reports to Mr. Wiggins, and he is the head, he is the vice president of the space booster programs. And of course, you've just heard from Mr. McDonald, and reporting to him are Messrs. Ebeling and Russell. And then, from the KSC offices, Jack Buchanan, also reporting to Mr. Kilminster.

Within the engineering organization, there is a space shuttle project engineering group under Mr. Brinton, and Mr. Macbeth reports to him. Then the other part of engineering, engineering design organization, you will see the people listed there. I don't need to read them, but they were all participants in the discussion.

We added a couple of notes here we thought were pertinent. One is that we do have a flight readiness review at Morton-Thiokol before Marshall has

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their flight readiness reviews, and Mr. Wiggins chairs that flight readiness review group. And the results of that flight readiness review are signed off both by Mr. Kilminster and Mr. Brinton to indicate that we are ready to fly.

Then Mr. Kilminster then attends the flight readiness reviews of NASA.

One other point we made here, which was also discussed by Mr. McDonald, and that is that he and Mr. Kilminster have been alternating as the attendees at the launches.

Now, as I go through the chronology you will see the names of various people here, and you will find them all in this chart. But I will try to point out where they fit as I go through the chronology.

(Viewgraph.) [Ref. 2/25-4]

We have the times here listed both in the Eastern Standard and Mountain Standard time, and the significant occurrences as we move through it. At 1:00 o'clock Eastern Time, Mr. Macbeth and Mr. Arnie Thompson and Charlie Saderholm all received calls from Boyd Brinton, who was at Marshall.

And that is something I ought to point out, that throughout this discussion Mr. Brinton, who is the manager of space shuttle project engineering, was at

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Marshall Space Flight Center, as was Mr. Speas. And as you know, Mr. McDonald was at Kennedy, as was Mr. Buchanan. The rest of the people we have listed here then were all at Wasatch in Utah.

As a result of the call at 1:00 o'clock, Mr. Ebeling and Ketner were notified, and Mr. Saderholm started the bulk temperature analysis of the propellant and Brian Russell arranged a meeting in Mr. Ebeling's office.

They convened that meeting in Mr. Ebeling's office, and the attendees are as listed there. During that meeting they discussed the O-ring resiliency data at 50, 75, and 100 degrees. They talked about the amount of blow-by and erosion that had been seen on the 51-C, which was the previous coldest launch.

CHAIRMAN ROGERS: Were you at those meetings?

MR. MASON: I was not at that meeting, no, sir.

CHAIRMAN ROGERS: So this is hearsay as far as you're concerned?

MR. MASON: That is right. I do get to where I entered the picture here in a moment. This was an effort to get an overview of what happened and where, to try make the rest of the testimony flow a little better.

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They did look at the other launch and static test temperatures and concluded that they did have a concern. They broke that meeting up and started to do some work. Mr. Maw was to run the calculation of what the O-ring temperature would be if the ambient were 18 degrees, and Mr. Ebeling notified Mr. Kilminster of the concern that they had.

And Mr. Kilminster asked Mr. Ebeling to get the engineering management, including Mr. Lund, involved, and to notify both Mr. McDonald and Mr. Brinton of the Marshall concern.

CHAIRMAN ROGERS: I assume that there was no recording made of these meetings and this is reconstructed from conversations you have had with other people?

MR. MASON: That is correct. We did talk to all of the people involved, so we felt we had a consensus of the sequence.

Then Mr. Buchanan called to give us some temperature predictions at that time. They were predicting 34 degrees at launch time at 9:38.

Then Mr. Thompson briefed Jack Kapp on his concerns. Then Mr. McDonald called from Kennedy, hoping to have a telecon between Morton Thiokol and NASA on those concerns. There were people called to the

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conference room to support the telecon, and that required interrupting the meeting that was at that point under way in Mr. Lund's office.

(Viewgraph.) [Ref. 2/25-5]

CHAIRMAN ROGERS: Well, just for semantics, you say people called to support the telecon. Do you mean to take part in the telecon?

MR. MASON: Correct. The conference room we have there has microphones so that you can be in the conference room and anyone can talk and be picked up.

CHAIRMAN ROGERS: My question really has to do with the word "support." Where you say "conference to support telecon" and "interrupted to support telecon," do you mean take part in the telecon?

MR. MASON: To take part, that is correct.

That telecon then proceeded with the people that are listed there, some 13, including the people at Marshall and at Kennedy. At the time that started, they notified Mr. Kilminster that it was starting.

(Viewgraph.) [Ref. 2/25-6]

At the same time, essentially, he received a call from—that LSS—that means launch support services. That is the people who work for Mr. Buchanan down at Kennedy. They called and gave him an updated overnight temperature prediction on a two hour basis,

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every two hours they gave him what the temperature was going to be.

CHAIRMAN ROGERS: Were any notes or records kept of this conference, or is this all reconstructed from memory?

MR. MASON: Well, I don't know of any specific notes, although there may be. I guess I would have to answer it that way.

I know the calculations that were run. Those calculations exist, because they took those on an every two hour basis, and had started using 18 degrees and then proceeded to use the more updated temperatures, because ultimately we were told it was forecast to be a 29 degree O-ring temperature.

In any event, those concerns that existed were discussed, and they amounted to the fact that the previous coldest launch had given us the greatest blow-by and that, coupled with concern about the stiffer O-ring that would exist at a lower temperature, was the focus of the discussion.

It was just that, a discussion. And so it was decided, and NASA requested that we put together an organized analysis or organize the data and write down what our concerns were, so that it could be reviewed in a more careful fashion.

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So there was a telecon then scheduled for 6:15 to do that, and at that point Mr. Kilminster called Mr. Wiggins and me—we were in another meeting in our Brigham City plant—and told us that there were concerns and that the telecon was going to start at 6:15.

And so we——

MR. SUTTER: Excuse me. A quick question. You had data where you had reached a conclusion. What was the purpose of NASA asking you to better organize that data? Was that for them to review it and reach some of their own decisions, or just to see if your thinking was proper?

MR. MASON: At this point we had not reached a conclusion. During the part that I'm covering here, we had had the discussion.

MR. SUTTER: I still don't understand why they needed the data better organized at this point.

MR. MASON: Well, we were providing it verbally over the telecon. They didn't have it in front of them. And so we were talking about things like curves and that sort of thing, that they didn't have the direct visibility of.

And we had not yet reached any conclusion. We were just saying that we were concerned because it was

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cold. So the request was to get that data and fax them down, so that they could look at it at the same time that we were looking at it.

And when Mr. Wiggins and I were notified, they were in the process of putting together what data was needed for the discussion. And at the same time, Mr. Lund was trying to prepare some conclusions and recommendations. So the chart preparation and the conclusions and recommendations were going on simultaneously.

And Mr. Wiggins and I then arrived as that was going on and, as it turned out, we were not able to get all of the charts together in time to start the telecon. We got the first charts faxed down there by 6:30 and the last charts didn't get down there until 7:20.

And so we were working this in a real time basis. We did start the discussion again at 7:00.

(Viewgraph.) [Ref. 2/25-7]

The engineers that had prepared some of the charts discussed their charts, and ultimately Mr. Lund discussed the conclusion and recommendations chart. The fundamental issue, however, was the concern about what effect the cold would have on getting the primary O-ring to move into its sealing position and seal.

And if it needs clarification later, Mr. Lund

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has some charts to explain that. But when we run the leak test, the primary O-ring is moved into the furthest point away from its sealing position, and that is maybe 20 thousandths of an inch or so, whereas the secondary is moved towards its sealing position.

And the concern was whether the cold and the stiffness of the O-ring and the grease would delay the movement of the primary O-ring so that it would not seat properly and there would be blow-by and perhaps damage to the secondary O-ring.

We looked at the erosion history we had to see what correlation there was with temperature, and there really wasn't any correlation. There was also a review of the blow-by history, which Mr. McDonald has covered very well, and the correlation that appeared there was that we had blow-by in both 75 and 53 degrees, but it was much more severe at 53 degrees, and so it was thought that that was evidence that the cold did in fact affect the performance of the O-ring.

Now, we looked at data on the hardness of the O-ring as an indication of how stiff and difficult it might be for the O-ring to move.

(Viewgraph.) [Ref. 2/25-8]

And we determined that from 50 degrees to 30 degrees, a 20 degree drop, it would increase the

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hardness by about ten percent. In fact, that put it in the range of—when we discussed this that night, early in the program we were deciding what hardness we wanted and we had used some 90 durometer O-rings in some of the hydrostatic tests. We ultimately ended up with the 80 durometer O-ring.

But it was pointed out that we had some experience with that O-ring which was harder.

DR. FEYNMAN: What you mean by hardness is it's difficult to squash it into the tiny crack that it is supposed to go into? What is the feature of hardness? It is not resiliency; it is the difficulty of pushing it into a corner?

MR. MASON: Well, it was thought that that stiffness might make it move more slowly, moving across the gap, and then it would be harder to extrude it in. However, once it sealed, being slower to extrude per se wouldn't be a problem, once it sealed.

The points we're talking about, of course, were the points that were discussed back and forth that night.

We did look at the temperature history on our previous tests and flights, and we discussed the fact that we had run a sub-scale blow-by test at 30 degrees which had shown no blow-by. That was only an indicator,

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because that was a cold test and it was with a fixed joint gap, and so it was just another point of data that we had to put in our heads.

MR. HOTZ: Mr. Mason, was that a vertical or a horizontal test rig that you ran the subscale test on?

MR. MASON: I don't know.

MR. HOTZ: Could you find out for us, please?

MR. MASON: Yes, sir. There are several fellows here that know, I'm sure.

VOICE: It was a vertical test rig.

MR. MASON: Out of that discussion, we got to the point where we just—we couldn't make a clear conclusion of the effect of temperature on the O-ring performance. And a number of the engineers felt that on that basis we ought to just stay where we were in our flight history and, with the information we had at that point and as far as we had proceeded in our thought process, that appeared to be the right thing to do.

And so we proceeded to present that information.

CHAIRMAN ROGERS: Isn't it fair to say, though, that all of the engineers felt that way?

MR. MASON: No, sir.

CHAIRMAN ROGERS: Which ones favored——

MR. MASON: At that point it was not a pro-con

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sort of discussion. It was, where are we in our thought process, and where we were in our thought process was that what we've discussed to date says let's stay with our experience.

CHAIRMAN ROGERS: But what I mean is, didn't all the engineers agree with that?

MR. MASON: I would say that at that point we all agreed with that, that it wasn't engineering or anyone else. We had just looked at what we had and went through our thought process, and at that point we felt that that was the recommendation that we got.

CHAIRMAN ROGERS: The position, though, that you took unanimously was the engineering position, was to base the recommendation on previous flight history, and you cite 51-C, which

was the coldest launch that had been made and where you had had the most trouble, isn't that right?

MR. MASON: That's right.

(Viewgraph.) [Ref. 2/25-9]

The next comments then were essentially, as Mr. McDonald said, that Mr. Mulloy said he disagreed because he didn't feel that we had shown any correlation between blow-by and erosion with temperature, and that therefore the rationale that had been used when we decided to fly in the first place was still a valid

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rationale.

And Mr. Hardy expressed surprise at the recommendation because it was above the qualification temperature. However, he said he wouldn't recommend flying against our recommendation.

And then Mr. McDonald came on and said that he too had a concern about the cold, and he pointed out that the secondary O-ring was in the position to seal, where it didn't have to move as the primary did, and that this point ought to be considered.

With that discussion, we were—let's say we felt that we needed to have further discussions to see if these comments would in fact cause us to have any change in our position. And so we decided to do that off the net, and we asked for a caucus.

(Viewgraph.) [Ref. 2/25-10]

Now, in the caucus we revisited all of our previous discussions, and the important things that came out of that was that, as we had recognized, we did have the possibility that the primary O-ring might be slower to move into the seating position and that was our concern, and that is what we had focused on originally.

The fact that we couldn't show direct correlation with the O-ring temperature was discussed, but we still felt that there was some concern about it

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being colder.

We then recognized that, if the primary did move more slowly, that we could get some blow-by and erosion on the primary. But we had pointed out to us in that caucus a point that had not come across clearly in our earlier discussions, and that is that we had run tests where we deliberately cut large pieces out of the O-rings to see what the threshold of sealing was, and we found we could go to 125 thousandths of a cut out of the O-ring and it would still seal.

DR. FEYNMAN: Mr. Mason, was that a static test or a dynamic test?

MR. MASON: It is done by blowing the pressure against the O-ring, and it was done both cold and hot.

DR. FEYNMAN: But not in a moving joint?

MR. MASON: That is correct. And I might follow that. Through the whole discussion we were concerned about or we recognized that the O-ring, either primary or secondary, would have to seal before the joint had time to expand.

Now, the gas pressure gets to the O-ring before the joint has a chance to expand. But the question was, with the slower movement of the O-ring, would it still be there and seat before the joint expanded.

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MR. HOTZ: Mr. Mason, what kind of time frame were you talking about there, in the time it takes to get there?

MR. MASON: Well, the charts that we had said that if you seat in 160 milliseconds that—well, let's see. This goes back to that 1R versus 1 discussion we had, that what that boiled down to was that during the first 160 milliseconds the joint is still closed enough that the secondary O-ring actually provides redundancy.

Up to 300 milliseconds, there is less certainty, and above that there is a serious question about that.

Now, again, that chart was based on the worst conditions as far as joint conditions were concerned, that is tolerances. And what we had here we knew was a fairly nominal compression condition.

DR. RIDE: Excuse me. Did you just say that is the first 160 milliseconds that joint is officially classified as a Criticality 1R?

MR. MASON: It is not officially, no, ma'am.

DR. RIDE: That's right. It is just a Criticality 1, so even during the first 160 milliseconds you are not allowed to consider the secondary seal a backup to the primary seal by the critical items list.

MR. MASON: Well, I don't certainly want to

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debate that point. What we did, what we considered, we considered that when it was established that it went from 1R to 1 it was identified that we were losing redundancy after the ignition transient.

And we did consider, and that is where I'm headed, that there was some protection during the ignition transient from the secondary O-ring.

CHAIRMAN ROGERS: I'm not sure I understand your answer to Dr. Ride's question.

MR. MASON: Well, let me try again.

DR. RIDE: Well, it sounds to me like you're trying to exercise some kind of engineering judgment on whether you can consider this a 1R or a 1, and I just don't think that the system allows that. I think that once something is classified as a Criticality 1, that sets a red flag in everyone's mind that that is an extremely dangerous situation.

And I'm just not sure that you're allowed to go back and rationalize it as, well, during this 100 milliseconds we may have a secondary seal, we may be allowed to consider it a redundant seal, we may have protection to a Criticality 1 system.

MR. MASON: Well, I really can't address the propriety of it as far as the system is concerned. I would have to state what we did, and we had recognized

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when the criticality was changed from 1R to 1 that it was done because we didn't have a full-time redundant seal.

And at the time of this discussion we were not addressing the thing from a 1R/1 viewpoint. We were addressing it from what we knew of how the joint performed, and what we knew was that early in the ignition transient that the secondary in fact did function until the joint rotated.

CHAIRMAN ROGERS: But isn't that in effect changing the critical items list? I mean, as Dr. Ride says—and her list shows there was no redundancy, and if I understand your answer you're saying that, we as a management team decided there was redundancy.

MR. MASON: We did consider that as a factor in our decision process. And let me say first, I didn't get to the thing in the right sequence. Our primary concern and the thing we addressed in our earlier conversation was getting—making sure the primary O-ring would seal.

And when we had this further discussion it was pointed out that we had run the test which showed that we could tolerate about three times as much erosion in the primary O-ring as we had previously experienced, in

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fact experienced in that 51-C, I should say, in that cold test.

So we felt we had a substantial margin because we had the ability to tolerate much more erosion than we had, and that the erosion was driven by the volume of gas that could be passed past that O-ring, and that that we felt was limited, so that it was very unlikely that it could get beyond the three times the erosion, or beyond the three times the erosion we had seen before.

DR. FEYNMAN: Could I ask some questions. Doesn't the erosion partly depend upon the speed of the gas, the size of the hole through which it comes, and other matters? And do you have some kind of calculation that shows that you couldn't get more erosion than the amount that you got in 51-C?

MR. MASON: I don't know whether we have that calculation that tells us what that limit is or not.

DR. FEYNMAN: What made you think that 51-C was the maximum erosion that you could reasonably expect?

MR. MASON: We didn't feel that that was the maximum we could expect. We said that if there was a temperature effect that made it move slower, we might get some more erosion. And we felt that the factor of three times was not likely to be exceeded.

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DR. FEYNMAN: Well, let me explain. I'm not interested at the moment in the temperature. I'm going to criticize the original decision that you can operate at 53 degrees, which is a different point of view than what we're talking about, based upon the idea that you've already seen that you have one-third the amount of erosion necessary to split the primary ring at a time when the secondary has a high probability of not being seatable, as Mr. McDonald said.

And I would like to know how you judge, by having seen one-third of an O-ring disintegrated in one flight, that it is very unlikely that you would get three times that erosion on another flight under the same circumstances?

MR. MASON: Well, that is the judgment issue that we finally got down to.

DR. FEYNMAN: I am trying to understand your judgment.

MR. MASON: When we had the previous erosion, to get that we had to fill all of the cavity that was there, all of the annular cavity. So there wasn't any way to get any more, substantially any more gas flow.

DR. FEYNMAN: Doesn't it depend upon the speed of the gas and the temperature? Doesn't it depend maybe upon the complexity of the hole that goes through the

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putty, so that it would be a matter of accident how much you got?

Could it, for example, have been that you got one-third the amount of erosion, for instance, because the putty hole was smaller and longer with a reasonable probability?

MR. MASON: Well, the engineers, which are here, have—I don't have off the top of my head the numbers, but we did run tests with focused jets, which focused the most amount, to see how much erosion we would get. And I believe they can do a better job of correlating that than I can.

I can comment that my understanding of it was that it would appear that, because there is a limited volume and we had looked at what a highly focused jet would erode, that the combina-

tion of that knowledge made us feel that the probability of getting three times the erosion was very unlikely.

DR. FEYNMAN: Thank you.

MR. WALKER: Sir, isn't it true that the amount of erosion you can tolerate actually depends upon the particular rocket motor you're dealing with, because the amount of compression can vary on the particular O-ring depending upon where you are within the tolerances of the two pieces which make up that rocket

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motor?

And had you analyzed the particular amount of compression on 51-L, these particular joints, to see whether or not that was the average amount of compression and how much erosion of the O-ring you could actually tolerate?

MR. MASON: Yes, we did look at the compression on that testimony joint, and we compared it, as a matter of fact, with the compression that we had had on the previous cold launch. And the compression was essentially nominal. As I recall, it was around 40 thousandths. And so we did consider that.

DR. RIDE: Let me go back to this for just a second. We have got a Criticality 1 system and there is some question in your minds whether it will perform at the temperatures that are predicted at launch. Around 29 or 30 degrees is what you are basing your judgments on.

What test data did you have to show, or any test data at all on joint rotation or the timing function, or even seal erosion, down at 30 degrees, that would allow you to make a reasonable engineering judgment that this Criticality 1 item was safe to fly?

MR. MASON: Well, it was the various items that I mentioned. We had the durometer of the O-ring

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down to below 20 degrees. I don't remember exactly, but it was down well below the temperatures. And we could see what the change in durometer was with temperature.

We did have the two tests we had run with the fixed joint at 30 degrees to indicate that the O-ring would seal if it moved in in the right timing.

DR. RIDE: But you said that at the time you weren't putting much emphasis on those tests. For one, they were fixed plates, and for another thing, you were using the argon gas and not the freon by that time.

MR. MASON: We were trying to avoid overemphasizing that test. We wanted to make sure we recognize it wasn't a totally representative test because it didn't have the joint rotation. On the other hand, we felt, because it was at the temperature and did indicate that the O-ring would seal if the timing was proper, so that the O-ring was in position before the joint rotated, we expected the seal—we expected it to seal.

DR. RIDE: Did you have data that indicated how the joint rotated at those temperatures?

MR. MASON: We know how the joint rotates. I don't believe there is any real effect of temperature on the joint rotation. If anything, it would slow it down.

DR. RIDE: Had that analysis been done?

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MR. MASON: The joint rotation, the analysis to see what the temperature effect on the joint rotation is?

DR. RIDE: Yes.

MR. MASON: I do not believe that has been done.

DR. RIDE: How about any tests or analysis to determine what the timing function would be at those temperatures? Had those been done at the time?

MR. MASON: Well, they had not been done at the time. We were in the process of devising the tests to try to establish that.

DR. RIDE: So those tests hadn't been done when you made this decision?

MR. MASON: They had not been done. Well, I had better not go beyond my knowledge of the subject. I don't know personally how far we had progressed.

I can say this. We didn't have enough information that we could quantify the effect of the cold.

DR. RIDE: You said that previously you had a factor of three safety in seal erosion. That sounds to me like you were willing to accept damage to a Criticality 1 system. Did you have any prediction of what the seal erosion would be or any, again, test or

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analysis on what seal erosion would be under these conditions, or were you extrapolating from the 53 degree temperature, and did you have a model to do that extrapolation?

MR. MASON: Well, that is a lot of questions. We had done some modeling of the flow in the—the flow effect on the O-ring, and I'm not qualified to really talk about that in detail, but the engineers when they get here can provide more detail on that.

I have lost track. I would try to answer all of your questions. I have lost track. Could you ask me again?

DR. RIDE: Okay. The first one I guess I was asking about was the seal erosion. Do you know whether you had any test data on the seal erosion?

MR. MASON: Well, I know we had a lot of information, as I said, where we had focused, deliberately focused hot gases to see how fast it would erode and what kind of gas flow rates were necessary. And that is on erosion.

DR. RIDE: Let me ask a different sort of question. Were you willing to accept damage to a Criticality 1 system? It sounds as though you were willing to allow erosion on the primary O-ring.

MR. MASON: The answer has to be yes, and let

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me explain that. The position we were in is, the first thing that has to happen is you have to have the blow hole through the putty, and our experience is that that happens like five percent of the time. I don't have that number exactly, but if you look at our history in the flight motors that is like five percent of the time.

When that happens, you can either get no effect, just simply hot gas pressurizes the O-ring, or you can get blow-by and then the O-ring seats, or you can get erosion.

And we had erosion, if memory serves me, four times in the some 144 joints that we had had. And so we recognized that we might get some erosion. We also recognized that it was a low frequency event, but the possibility existed, and that if it did happen, we had erosion, that it would in our opinion seal because of the extra margin that we had identified by running the tests on the amount of flow it would take to get enough erosion to actually keep the primary from seating.

DR. RIDE: I guess what I'm really trying to understand is whether you really had the engineering data or an engineering analysis to back up the decision that this Criticality 1 system was safe to fly at those temperatures.

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You know, what we've seen in the charts so far is that the data was inconclusive and so you said go ahead.

MR. MASON: I'm sorry, I hope I didn't convey that. But the reasons for the discussion was the fact that we didn't have enough data to quantify the effect of the cold, and that was the heart of our discussion, is how much effect is the cold going to have on the performance.

MR. SUTTER: Could I ask a question? Your engineers with the data they had and with the concern with the temperatures did reach a tentative conclusion at least that, why not wait for at least a temperature that had already been flown. I guess you were responsible, your engineers were responsible, for the design and the testing and qualification of that design, and I would assume the interpretation of the data.

And therefore I am extremely puzzled why a NASA person could disagree and ask you to review the decision your engineers had reached, since it seems to me theirs was the responsibility for the design.

MR. MASON: Well, let's say the request to reassess was not a major factor in my view. The fact that we were picking a temperature based purely on the one test or the one flight, and we had had static tests

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at other temperatures, and Mr. McDonald explained why we didn't consider those conclusive—but it was difficult to say that 53 degrees was exactly the temperature that you ought to fly at.

And when Mr. McDonald made the point about the secondary O-ring being in a favorable position, we felt that it was appropriate to consider that point, and that is the reason that we said, well, let's make sure that we have considered everything.

And the two things that came out of that assessment were: One, that we did have—we believed that we had a margin, a substantial margin, of allowable erosion and still have that primary ring seat and seal properly.

We didn't know for sure what the effect of that cold was on the time to move, but we said if in fact it does delay moving and it cause blow-by, then the secondary ring is already in position and the blow-by has to occur—if it occurs, it occurs immediately. The blow-by occurs on pressurization.

And so under those conditions, if you were to have blow-by you would be seating the secondary.

Now, I understand the point about the 1R versus 1. I simply have to be honest and say that that is the way we were looking at it then, that during the

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ignition transient that the secondary really could function, and we had never said that it couldn't function on the ignition transient. Even when we wrote the CIL, we had indicated that.

In any event, we did expect the primary to seat. We expected it to seat because we had some tolerance of its ability to erode and still seat and seal. And so that was our thought process.

Now, the discussion was a free and open discussion with all of the people there, and I believe that it was not—well, at that point it was clear to me we were not going to get a unanimous decision. And so the question was, did we have a reasonable position to go to 53 degrees or did we have confidence that we could fly with a 29 degree O-ring.

And the people who were there had heard all of the discussion, and so I concluded it was appropriate to talk, to get a poll of the chief engineer and the chief program manager, and Mr.

Wiggins, who has the division responsibility, to see how they felt, whether they felt that we could safely fly with all of the information that had been presented.

And we did conduct that poll, and we did conclude that it was safe to launch.

CHAIRMAN ROGERS: Could you tell us what the

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poll showed? You say that you and Mr. Kilminster and Mr. Wiggins and Mr. Lund were unanimous. How about the others?

MR. MASON: We only polled the management people, because we had already established that we were not going to be unanimous, and we had already heard—

CHAIRMAN ROGERS: That wasn't the question. The question was what about the others. You testified before that you thought probably all of the engineers were against the launch, and now you say you took a poll and you only polled the managers. You didn't ask the engineers how they felt after their review of the data?

MR. MASON: In that discussion I felt that everyone had represented their opinion, and there were a number that I felt indicated, as we did, that with the consideration of the erosion margin and, well, the very factors that I just talked about, that they also considered that to be a reasonable—

CHAIRMAN ROGERS: Did they express a change of mind? Did they say, well, I've changed my mind?

MR. MASON: Not per se, no. Excuse me, I just feel that it is the kind of discussion that we frequently have, in which all of the people express their opinions and they make them clear.

CHAIRMAN ROGERS: Did you ever have an experience

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where all the engineers voted one way and management voted the other?

MR. MASON: That wasn't the case here.

CHAIRMAN ROGERS: What was the case here? It looks that way. It looks as if you said the managers you polled were unanimous and the engineers who were opposed to the launch were still apparently opposed to it.

MR. MASON: There were some engineers.

CHAIRMAN ROGERS: Which ones?

MR. MASON: Mr. Boisjoly and Mr. Thompson were the outspoken ones.

CHAIRMAN ROGERS: I'm asking which ones changed their minds. Before you testified that all the engineers were opposed to the launch, and now you went through this process you've just described to the Commission.

My question is, did any of the engineers change their minds, and if so which ones?

MR. MASON: Well, I would say then that I would have to look at the list here. Based upon the conversations that we had there, I felt that Mr. Macbeth and Kapp were supportive. And Mr. Brinton, who was at Marshall, it is harder to judge, because he didn't have a lot to say.

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What we were faced with was that there were the outspoken individuals, and the rest participated to a lesser degree. But I believe that we are familiar enough with the people and their manner to be able to judge their feelings, and we heard what they had to say.

MR. WALKER: Mr. Mason, earlier in testimony in private session, you indicated that Mr. Boisjoly and Mr. Thompson were the most knowledgeable on the seals, is that correct?

MR. MASON: Yes.

MR. WALKER: Then the two most knowledgeable engineers were still opposed, and my understanding is they were still strongly opposed. Why did you proceed in the light of your two experts being very strongly opposed?

MR. MASON: It's kind of a horse race as to who is the most expert on the subject, because there are different areas of expertise, some on the gas flow and some on the metal parts. We identified that Mr. Boisjoly and Thompson were both generally knowledgeable people on those joints, and they were people whose opinion we regarded.

However, Mr. Kapp has a long history on the joint and he is as knowledgeable in my opinion as they are, and Mr. Sayre to a lesser degree. So I would say

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that we felt that, although they were outspoken, we listened to their reasons more than the intensity, and the reasons boiled down to the things that I said. The reason was the uncertainty about the rapidity with which that primary would move.

MR. ACHESON: Did Mr. Russell participate in the conversation, either on the telecon with NASA or in the caucus off-line that followed?

MR. MASON: Yes, he participated in both discussions, it is my recollection.

MR. ACHESON: What were his views as far as you can reconstruct them?

MR. MASON: I felt that he had similar thoughts that we did. He had concern about the uncertainty, but beyond that I would say that he was, if anything, neutral; that he was providing all of the information and was having some difficulty, as were the rest of us, trying to get a decision in which we had confidence.

MR. ACHESON: Now, during the telecon with NASA and during the private caucus off-line did anybody, either at NASA or at Thiokol, mention the change in the critical items list to Criticality 1?

MR. MASON: No, sir, that was not discussed. That was not an issue at that point.

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MR. WALKER: I have a question about the temperatures. You said that you decided that 29 degrees was safe to launch. Did the discussion result in some lower temperature limit below which you would not have recommended a launch?

MR. MASON: I'm sorry? Are you saying would we——

MR. WALKER: Your ultimate recommendation to NASA was that it was safe to launch, and I believe the predicted temperature of the O-rings was 29 degrees.

MR. MASON: That is correct.

MR. WALKER: Did your discussion result in the establishment of a temperature below which it would not have been safe to launch?

MR. MASON: No, we didn't pursue it below that. I think the initial concern when we heard 18 degrees and we were thinking that the O-ring might be that cold, that is how all of the discussion got started, and there was a lot of apprehension at 18 degrees.

And then it was later found that it was going to be somewhat warmer than that, but we didn't pursue the idea of getting any colder than 29 degrees.

MR. WALKER: Well, how critical was this? If the temperature turned out to be 28 degrees, would this

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have made a big difference? The recommendation was based upon a prediction. Of course that was only a prediction at the time and you didn't know exactly what the weather was going to do and exactly what the temperature was going to be.

And I would have thought that the recommendation would have been more specific, so that, depending on the exact conditions which prevailed when the launch occurred, the recommendation would have been one which could have been used to make a decision.

MR. MASON: Well, I can't argue with that reasoning. We were addressing the specific condition, not looking for a threshold.

MR. WALKER: But usually engineering decisions and calculations are pretty exact calculations, and you usually have a very specific number, and that is why there are launch criteria. And those numbers are specific numbers, they are not ranges.

And here evidently you had a range, but you didn't even specify what that range was. Wasn't that rather unusual?

MR. MASON: Yes, I would say it was, the fact that we were focused on that one point, and that is what we were addressing. Normally, if you were earlier, preparing for another launch or something, if the

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question came up, you would in fact tend to bracket it.

We were faced with, we felt, a specific condition here, and that is the reason we addressed it that way.

MR. WALKER: So you didn't feel uncomfortable with this position?

MR. MASON: I didn't. I felt that, after we had hashed it out and understood it, that the inability to precisely quantify the movement of that primary O-ring was the concern that we had. And that is the additional knowledge that I would like to have had.

But I felt that, with what the things that I've already pointed out, that we had a reasonable basis for feeling that it was—

GENERAL KUTYNA: Mr. Mason, you have used the word "uncertainty" now four or five times in the last five minutes, and now you've just had the inability to quantify this thing. That is the best thing you had going for you. I mean, every launch has a risk, but you take that risk because something must be achieved.

What was driving here? What was to be achieved that caused you to go?

MR. MASON: Well—

GENERAL KUTYNA: Why couldn't you wait a day?

MR. MASON: Well, as far as waiting a day is

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concerned, we wouldn't have gained any more information.

GENERAL KUTYNA: You would have gained temperature.

MR. MASON: That's true. And we had to decide whether we felt that it was safe to fly at 29 in order to respond to the question, should we fly. That was the question we were trying to answer.

MR. HOTZ: Mr. Mason, in responding to the General's question about the use of the word "inconclusive" and "uncertainty," in your whole presentation here today you were basing your decisions on inconclusive and uncertain information. And yet, my understanding of NASA's flight philosophy is that decisions to go or no-go are based on certainties and conclusions.

When did this change in philosophy affect your operations?

MR. MASON: Well, I would have to respond, I think it is a case of degree of uncertainty.

MR. HOTZ: Well, do you allow any degree of uncertainty before you launch?

MR. MASON: I believe that in any case in the design and testing and so forth of the whole system, there have to be some uncertainties. It is a question of

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degree.

MR. HOTZ: But it seems to me there was a high degree of uncertainty in your calculations here.

MR. MASON: Well, there was.

MR. HOTZ: According to your own testimony.

MR. MASON: Well, that of course is the point that we were struggling with that night, was what degree of uncertainty is there. And the area of uncertainty was the moving of the primary O-ring.

MR. HOTZ: Well, how would you characterize it? A high degree, a low degree, or a medium degree of uncertainty?

MR. MASON: As far as the probability of seating is concerned, I thought there was a low degree of uncertainty.

CHAIRMAN ROGERS: Following up on that, Mr. Mason, Mr. Kilminster when he wrote the telefax said "temperature data not conclusive on predicting primary O-ring blow-by."

Isn't that a very serious statement, saying that it was not conclusive? Isn't that another way of saying that there might be a conclusion that there was going to be primary O-ring blow-by?

MR. MASON: Well, what we did was say, since it wasn't conclusive, we were going to assume that it

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would have a negative effect. That is the way we approached it.

CHAIRMAN ROGERS: In other words, this is a statement saying we expect primary O-ring blow-by.

MR. MASON: We said we can't tell how much effect temperature is going to have, and so we had better look at what can happen if in fact it does.

(Viewgraph.) [Ref. 2/14-10]

CHAIRMAN ROGERS: Now, if that is the case, Dr. Ride's question is most relevant, because the critical items list said that you cannot rely on this redundancy. If you're predicting there's a possibility of primary O-ring blow-by, you are violating the critical items list.

MR. MASON: Well, we weren't predicting it, but we were saying because we weren't able to be certain—that the situation we were faced with is we have had blow-by on earlier flights. We had not had any reason to believe that we couldn't experience it again at any temperature.

That is where we were right then. And so we had to say that we can experience blow-by, because we had experienced it, and we had not yet been able to figure out what to do to prevent it. And so we knew that it could happen, and so we had to say, what's the

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probability.

But even given the low probability, we still had to think, what's it going to mean if it does. And that is what we endeavored to do, was to think, what does it mean if it does, even if it is a low probability.

CHAIRMAN ROGERS: Well, I don't want to continue arguing, but it seems to me, though, that the critical items list says if that is the case it's going to result in a catastrophe, because there is no redundancy. That was the finding as I understand it. Am I correct, Dr. Ride?

DR. RIDE: Well, I guess that, just reading the critical items list, what it says is that it is the primary O-ring which is Criticality 1, not a Criticality 1-R.

MR. MASON: If you just read it in that sense, yes. But if you know how it was generated, which was that it was not redundant full-time, that it was only redundant during the ignition transient and then it was non-redundant once the joint opened up—we knew that. We understood that, and that did enter our thinking.

CHAIRMAN ROGERS: Dr. Feynman.

DR. FEYNMAN: In discussing this idea that the secondary seal will seat, the principle is that is

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located in the correct place because of the pressure put on during making a check, which is two days or so before launch. After the check has been made in the VAB building, this machine is put on a carrier and carried, and jiggled and so forth all the way out to the site, and it stands there for two days and is rained upon, and we have found in other cases that water gets into the seals.

Did you consider, when you were discussing these, the probability or the possibility that in all of this jiggling and the water inside there freezing, making ice, that it could have displaced the secondary seal, and that your idea that the secondary seal is in exactly the right place and will seat immediately during the short transient might have even more uncertainty? Were you considering that possibility?

MR. MASON: We were not considering the possibility of ice in the joint. We did not discuss the potential for how much the O-ring might move as a point of rolling it out, although I think the feeling is that that is a pretty stiff joint and doesn't see any real movement.

But we did not consider the possibility of ice in the joint.

CHAIRMAN ROGERS: Mr. Armstrong.

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VICE CHAIRMAN ARMSTRONG: Typical approaches to performance expansions, envelope expansions in the case of airplanes or component performance envelope expansions, normally take the position, well, I've flown in these places and, based upon that and my analysis of the results of those flights, I would be willing to go this much farther for the next case.

But yet, your engineering group said they wouldn't be willing to go outside their existing experience base, or recommended that they not originally, and then you said, well, we will go this much more.

Was there before the fact any attempt, let's say, to provide a level of expansion which the company could stand behind?

MR. MASON: Well, there was work under way to get a better understanding of overall performance of that joint and the effect of rotation and the effect of temperature, the program that was kicked off after the August meeting at headquarters, where all of the joint designs and performance was reviewed.

Then there was a program plan put together to look at ways to improve the performance and to get a more full understanding. That was under way. We didn't have the data.

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VICE CHAIRMAN ARMSTRONG: The reasons that didn't exist after you had flown, as you say, 144 joints or whatever, could you attribute that to some reason? Was it that you weren't interested or didn't think it was important, or that you didn't have the resources?

How would you characterize the fact that you were only as far along as you were?

MR. MASON: Well, this spans along a fairly long period of time, but there had been a fairly long period where we had made some improvements in the layup of the putty. We had put some controls on the time it could be exposed and that sort of thing.

We were trying to make some changes that were not at risk of going in the wrong direction, but we had high confidence we were going in the right direction, and we were having a long period not having any erosion or blow-by.

In spite of that and in the middle of that period, in the August time period we all concluded that we needed to proceed with an aggressive effort, and so there was, say, a little pull and tug there, in that at that time the performance was running pretty well, but at the same time the analysis said we needed to get on with some improvements.

And we were searching for improvements that we

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could prove out without running any risks on the flight.

VICE CHAIRMAN ARMSTRONG: Mr. McDonald stated that he did feel some pressure in this meeting, and would you characterize your own feelings about pressure that may have influenced the decision process in any way?

MR. MASON: I've thought a lot about that, and there was some pressure, but I believe it is in the range of what we normally encounter. Whenever we're taking a position, NASA tests us on that position to explain it, justify it, that sort of thing. And that is the way I perceive we were being tested, is how supportive or how well can we justify our position.

And so we responded in that fashion. Now, I can say, I guess, that I get pressures in a lot of cases, for a lot of decisions, not just from NASA but from many people I deal with, my boss and so forth. And I think that I am able to treat that properly and make a sound decision independent of that.

VICE CHAIRMAN ARMSTRONG: Well, let me be more specific to follow up. Do you think schedule pressure or cost pressure had significant influences in this instance?

MR. MASON: Cost pressure definitely had no bearing on it. From a schedule standpoint, we take a

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lot of pride in the fact that we have supported all of the launches to date, and if there was any pressure, we wanted to continue to do the job we had been doing.

And that kind of situation exists every time. We have to say, are we ready to fly or are we not, and we want to be ready to fly, but we want to make sure it's safe.

CHAIRMAN ROGERS: Did anybody get in touch with you, any calls of any kind, urging you to go ahead with this launch?

MR. MASON: No, sir.

CHAIRMAN ROGERS: As far as you know, were any calls made to anybody in Thiokol?

MR. MASON: To my knowledge there were none. The only conversations we had were the ones on the net that have already been reported.

CHAIRMAN ROGERS: Just one other question. Did you realize, and particularly in view of Mr. Hardy's point that they wouldn't launch unless you agreed, did you fully realize that in effect you were making the decision to launch, you and your colleagues?

MR. MASON: Yes, sir.

MR. WALKER: Mr. Chairman, I had two other brief questions.

I would just like to return to your analysis

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of the data during your half an hour caucus in regard to the imprecision or uncertainty of the data. Is it not the case that the largest amount of blow-by was associated with the coldest launch, and that point was made by some of the engineers in the discussion?

Why did you not consider that conclusive?

MR. MASON: Well, we did consider it, but we didn't consider it conclusive, simply because we had had blow-by at the warmer temperatures and we had had erosion at varying temperatures.

MR. WALKER: But the largest amount of blow-by was at the lowest temperature.

MR. MASON: Actually, the erosion is more of a concern than the blow-by.

MR. WALKER: In fact, let's turn to the erosion. Mr. McDonald just stated that there was a case earlier in which the primary O-ring on the motor seal was, on the lowest seal, was eroded and only the secondary O-ring prevented a burn-through. And since you could not really rely on the secondary O-ring for the other seals, was that not also a very significant parameter?

I notice that is not mentioned at all in your analysis of your half an hour caucus.

MR. MASON: Of course, we were considering the

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joint seal and, as Mr. McDonald pointed out, we had concluded that the most likely cause of the nozzle problem, the nozzle and joint problem, was the fact that we had tested at a low pressure and probably had not had a seal of the primary, we had not confirmed that seal.

And we had subsequently gone to a higher pressure, the 200 psi, to be sure that the seals were proper, rather than be masked by the putty. What we found was that at the lower pressures, down at 100 psi, the putty could actually hold the pressure and so you weren't certain that the primary was seating. So we had to go to the higher pressure, which we knew the putty would not hold, and with that we could be sure the O-ring was seating.

And so our feeling on the nozzle joint was that that problem had been a product of not having proper sealing of the primary O-ring, and it hadn't been tested adequately because it was masked by the putty.

MR. WALKER: But was that incident discussed during this half-hour caucus?

MR. MASON: It was not discussed, no, sir. We were talking about the joint. It was of course known by everyone that that had occurred.

MR. ACHESON: Mr. Mason, can you recall any previous occasion when your at least preliminary advice

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to NASA had been not to launch and they wished to go ahead with the launch?

MR. MASON: No, sir, I do not.

MR. ACHESON: Was there any comment made during these conversations that you have described with NASA that this was a very unusual situation? Did anyone, for example, say that, it is odd that the shoe is on this foot?

MR. MASON: No, not in that fashion, no. The situation I felt sure, or feel sure, was clear, because of the conversations that had gone on and the discussions by all of the people about how they felt and the data they had was all on the net. And so it was understood where we were coming from.

MR. SUTTER: Could I ask one question? In the analysis to get ready for launch, there was this one statement that temperature concerns were maybe fuzzy because there was a blow-by or erosion at a 53 degree launch, but there was also one at 75 degrees. And I am just wondering

whether getting blow-by, erosion, at a very good, warm temperature where everything should be working quite well, wouldn't that raise concerns that maybe this system was running quite marginally, and then maybe temperature would really put it in a sub-marginal condition? That is one question.

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And the other one I have is, in this package you gave us it looks, as you proceed through the checks of the motors, more erosion and blow-by was occurring more recently than in earlier times, and the design changes, like the reduction in the structural strength of the motors, of the cases, the increase of the thrust—was there a concern at Thiokol that perhaps as time was going on and the system was asking it to perform better, that perhaps a seal was getting in more trouble?

And I'm just trying to understand. Was there that concern, or were you becoming more relaxed?

MR. MASON: I would say when we put it all together in August we indicated that we had a concern and that we needed to work on an improvement. We also in that summary said that we felt that it was safe to continue to fly as long as we had the proper conditions, that is, the 200 psi leak test, proper assembly, and that sort of thing.

And so we endeavored to summarize where we were in that August meeting, because we looked at all three joints, we looked at the field joints, we looked at the nozzle joint, and we looked at the igniter joint.

We looked at all of our history, and including

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the history that you have here, and made our assessment and identified that we ought to proceed in an orderly fashion, but aggressively, to make an improvement in the joint.

MR. SUTTER: Well, then in the August meeting, then, were there some ground rules laid down, like good inspections and other conditions to keep flying?

MR. MASON: Yes, and those were identified and those were in place.

MR. SUTTER: Do we have a record of what those conditions were?

MR. MASON: It is in the August 19 briefing that we have provided copies to the Commission on.

MR. SUTTER: Did you cover anything on temperature?

MR. MASON: No. In fact, in the copies that we provided we also put in a February 10th set of pages reflecting the things that had happened since, and in there we said that that is the condition that we should have.

MR. ACHESON: Those are the blue pages, are they not?

MR. MASON: Correct.

CHAIRMAN ROGERS: Dr. Keel?

DR. KEEL: Mr. Mason, in testimony that the

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Commission has received in executive session and also in personal notes made available, you were characterized as telling Mr. Lund in this caucus to take off his engineering hat and put on his management hat as part of that final management caucus.

And in fact, in your notes here from your presentation from your charts, you indicate that you have asked them to exercise best judgment and make management recommendation. Since Mr. Lund was your vice president of engineering and since he presented the charts and the recommendations not to launch outside of your experience base—that is, below a temperature of 53

degrees for the O-rings—in the previous 8:45 Eastern Standard Time teleconference, what did you have in mind when you asked him to take off his engineering hat and put on his management hat?

MR. MASON: I had in mind the fact that we had identified that we could not quantify the movement of that, the time for movement of the primary. We didn't have the data to do that, and therefore it was going to take a judgment, rather than a precise engineering calculation, in order to conclude what we needed to conclude.

MR. KEEL: What triggered the caucus that Thiokol asked for, according to the testimony, was the

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fact that NASA had expressed surprise and concern about the impact of such a recommendation of not launching below a 53 degree Fahrenheit temperature for the O-ring, and particularly the impact on schedule.

During the 30 minute caucus, was there any part of your discussion that addressed that impact on schedule and what that meant to Thiokol, any management concern on that?

MR. MASON: Well, when you say what—what triggered the caucus was not NASA's expression of concern. In fact, there is a bit of irony, but from my viewpoint the primary trigger was Mr. McDonald's comment about the fact that the secondary was in a preferred position, and that is a point that we had not actively considered in our previous discussion.

DR. KEEL: Well, what's the answer to the question, though? I said we have previous testimony that that is what triggered it. Regardless of what triggered it, considering those concerns were expressed, was there discussion in the Thiokol caucus with respect to schedule and the impact of sticking to a 53 degree Fahrenheit temperature as a launch condition and what that meant?

MR. MASON: No, there was not.

DR. KEEL: No discussion of schedule impact.

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MR. MASON: No. We simply addressed the issue of flight safety.

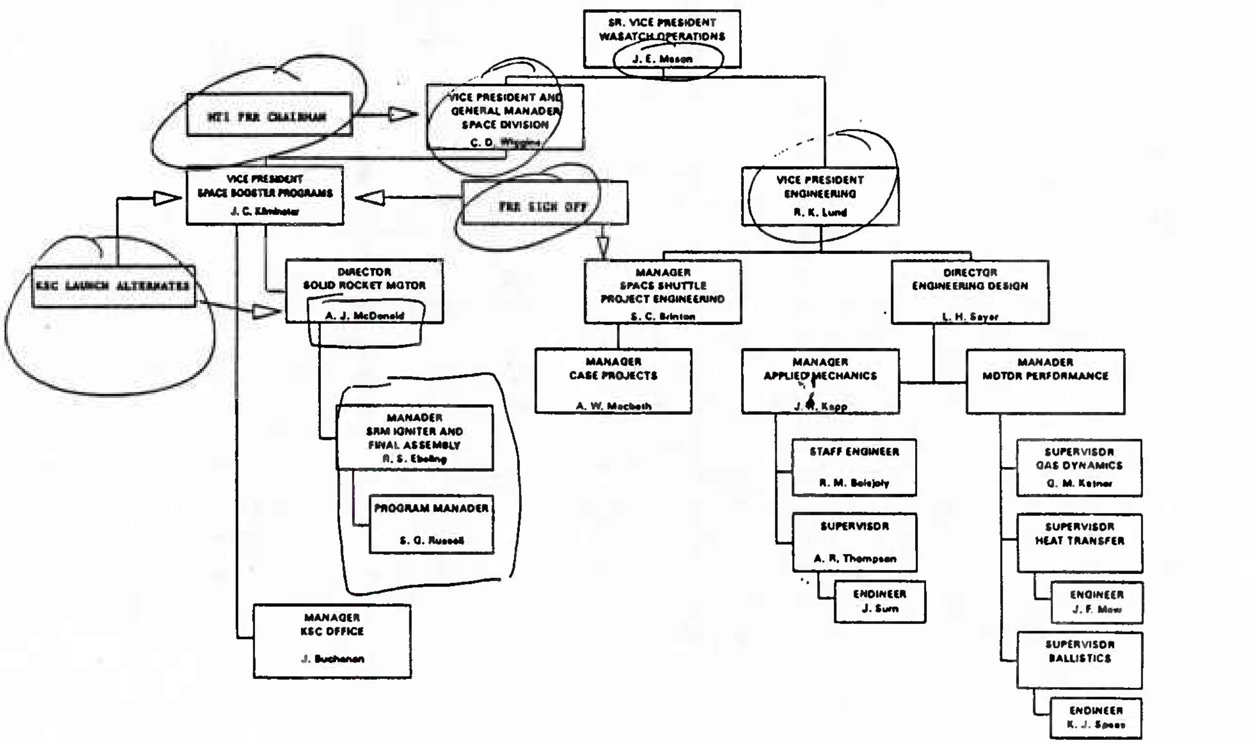
CHAIRMAN ROGERS: Mr. Mason, when you spoke to Mr. Lund and told him in effect to take off his engineering hat and put on his management hat, wasn't that pressure on your part to a subordinate that he should change his mind?

MR. MASON: Well, I hope not, but it could be interpreted that way.

CHAIRMAN ROGERS: Let's adjourn and reconvene at 2:00 p.m. And we plan to have Mr. Boisjoly and Mr. Thompson and Mr. Lund after lunch.

(Whereupon, at 1:00 p.m., the hearing in the above-entitled matter was recessed, to reconvene at 2:00 p.m. the same day.)

Morton Thiokol 27 Jan 1986
Meeting Participants



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55485-35 Organ Chart

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[Ref. 2/25-3]

27 January 1986 Chronology at Morton Thiokol (Utah)

Approximate Times

EST	MST	
1:00 pm	11:00 am	• W. Macbeth (MTI), A. Thompson (MTI), and C. Saderholm (MTI) receive calls from B. Brinton (MTI at MSFC) asking if 18°F overnight temperature at KSC causes concern for SRMs
1:10 pm	11:10 am	• R. Ebeling (MTI) and D. Ketner (MTI) notified of request
		• C. Saderholm initiates mean bulk temperature analysis of propellant
		• B. Russell (MTI) arranges meeting in R. Ebeling's office
2:30 pm	12:30 pm	• Meeting convened in R. Ebeling's office to discuss effect on seals
		• Attendees: R. Ebeling, W. Macbeth, R. Boisjoly, D. Ketner, A. Thompson, J. Burn, G. Gorman, B. Russell

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[Ref. 2/25-4 1 of 2]

27 January 1986 Chronology at Morton Thiokol (Utah) (Cont)

EST MST

- Discussion topics:
  - Static O-ring resiliency (recovery) test data at 50°, 75°, 100°F
  - Soot blowby and erosion of O-ring on 51C (SRM-15)
  - Previous launch and static test temperatures
  - Conclusion was that concern was valid
- Meeting adjourns about 2:00 pm (MST). Separate activities initiated
  - A. Thompson instructs J. Maw (MTI) to calculate O-ring temperature if ambient is 18°F
  - R. Ebeling notifies J. Kilminster (MTI) of concern. Kilminster directs Ebeling to involve engineering management including R. Lund (MTI) and to notify A. McDonald (MTI at KSC) and B. Brinton (MTI at MSFC) of concern

EST	MST	
5:00 pm	3:00 pm	• J. Buchanan (MTI at KSC) provides temperature predictions at launch time (34°F at 9:38 am EST)

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[Ref. 2/25-4 2 of 2]

27 January 1986 Chronology at Morton Thiokol (Utah) (Cont)

EST	MST	
5:30 pm	3:30 pm	• A. Thompson briefs J. Kapp (MTI) on concerns
5:45 pm	3:45 pm	• A. McDonald calls from KSC hoping to address concerns in telecon between MTI and NASA
		• People concerned called to conference room to support telecon
		• Meeting in progress in R. Lund's office interrupted to support telecon
6:05 pm	4:05 pm	• Telecon discussion between MTI and NASA begins

- Attendees:

R. Lund	J. Maw	B. Russell
L. Sayer	W. Macbeth	R. Tydeck
J. Kapp	R. Boisjoly	C. Saderholm
A. Thompson	D. Ketner	
J. Burn	R. Ebeling	

B. Brinton (MTI at MSFC)	J. Lovingood (NASA at MSFC)
K. Speas (MTI at MSFC)	L. Wear (NASA at MSFC)
A. McDonald (MTI at KSC)	J. Miller (NASA at MSFC)
	K. Coates (NASA at MSF…

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[Ref. 2/25-5]

27 January 1986 Chronology at Morton Thiokol (Utah) (Cont)

- J. Kilminster notified of beginning of discussions (receives call from LSS with updated overnight temperature predictions)
- Concerns discussed
  - 51C (SRM-15) launch was previous coldest and experienced greatest soot blowby past primary O-ring
  - O-ring resiliency (recovery) data—lower response at low temperature
- NASA requests better organized, written statement of concerns
- Follow-on telecon scheduled for 6:15 pm (MST)
- J. Kilminster calls J. Mason (MTI) and C. Wiggins (MTI) at Brigham City office about 4:30 pm (MST) to advise of concerns and pending telecon continuation
- R. Lund directs presentation chart preparation of data. Discussions continue between R. Lund, A. Thompson, L. Sayer, J. Kapp, D. Ketner, W. Macbeth, J. Kilminster, and R. Ebeling regarding conclusions and recommendations

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[Ref. 2/25-6]

27 January 1986 Chronology at Morton Thiokol (Utah) (Cont)

EST	MST	
		• J. Mason and C. Wiggins arrive in conference room (approximately 5:30 pm MST)
8:15 pm	6:15 pm	• NASA (MSFC) initiates resumption of telecon

- First charts faxed 6:30 pm (MST)
- Conclusion/recommendation charts faxed 7:20 pm (MST)

9:00 pm	7:00 pm	• Presentation begins by MTI

- Engineering's primary concern was effect of cold temperature on time for primary O-ring to move into sealing position AND SEAL
- Data review included:
  - Erosion history
  - Primary O-ring blowby history
    - 51C (SRM-15) +53°F
    - 61A (SRM-22) +75°F

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[Ref. 2/25-7]

27 January 1986 Chronology at Morton Thiokol (Utah) (Cont)

53
-29°
24°

  - Temperature effects on O-ring hardness (10 percent increase in hardness with 20 degree drop in temperature)
  - Temperature history of previous tests/flights
  - Subscale blowby tests at 30°F showing no blowby
- Effect of temperature on O-ring performance was unclear. Engineering position was to base recommendation on previous flight history—51C (SRM-15), 53°F O-ring temperature

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[Ref. 2/25-8]

27 January 1986 Chronology at Morton Thiokol (Utah) (Cont)

EST	MST	
9:45 pm	7:45 pm	• Presentation ends and following comments made:

- L. Mulloy (NASA/MFSC at KSC) disagrees with recommendation. Explains that data did not reflect correlation between blowby and erosion with temperature.
- G. Hardy (NASA at MSFC) expresses surprise at recommendation. Believes qualification temperature to be lower than 53°F. Would not recommend flying against MTI recommendation
- A. McDonald (MTI at KSC) acknowledges that cold temperatures are concern. Points out that secondary O-ring in position to seal if primary O-ring is slow in sealing, and that this should be considered

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[Ref. 2/25-9]

27 January 1986 Chronology at Morton Thiokol (Utah) (Cont)

EST	MST	
10:15 pm	8:15 pm	• J. Kilminster asks for off-line caucus to consider comments

- Caucus focuses on effect of cold temperature on joint seal function
  - Recognized that primary O-ring may be slower in moving to sealing position
  - Acknowledged that blowby may not be a direct function of temperature since blowby has occurred previously at O-ring temperatures of both 53° and 75°F
  - Recognized that if primary O-ring did not seal immediately, some blowby and erosion to primary O-ring could occur. Reestablished that three times amount of erosion previously experienced on 51C could occur without violating primary O-ring

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[Ref. 2/25-10]

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AFTERNOON SESSION

(2:05 p.m.)

CHAIRMAN ROGERS: The Commission will come to order, please.

(Witnesses sworn.)

CHAIRMAN ROGERS: Mr. Boisjoly, you are now presently employed by Morton Thiokol?

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TESTIMONY OF ROGER BOISJOLY, SEAL TASK FORCE, THIOKOL, AND ARNIE THOMPSON, SUPERVISOR, STRUCTURES, THIOKOL

MR. BOISJOLY: Yes, I am.

CHAIRMAN ROGERS: And how long have you been employed by them?

MR. BOISJOLY: Approximately five and a half years.

CHAIRMAN ROGERS: How many?

MR. BOISJOLY: Five and a half years.

CHAIRMAN ROGERS: And what is your present position?

MR. BOISJOLY: I am a Senior Scientist is my title, and I am basically a staff member to the Manager of Applied Mechanics.

CHAIRMAN ROGERS: And what kind of work have you been doing for the last five years?

MR. BOISJOLY: I have been involved in the joints, I have gone down to the Cape and taken a significant amount of inspections of the solid rocket boosters as they have come back from flights. I have reported on that information, and that information has been used in the flight readiness reviews for the next flights. And I have also participated in some of the

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structural analysis, and also was one of the primary drafters of the refurbishment specification on the cases themselves, particularly in the area of the joints.

I have given, made a videotape and have given a presentation at our plant, also at Vandenberg and KSC concerning the inspection characteristics of the joint surfaces themselves. I have been appointed as a member of the Special Seal Task Force that was initiated last August, and I am in that position currently.

CHAIRMAN ROGERS: You have been described in previous testimony as either the leading or a leading expert on the subject of seals and joints in the booster rocket, and I gather Mr. Thompson works with you in this connection?

MR. BOISJOLY: Arnie used to be my supervisor. I used to work for him until approximately two years ago, and I went out of that capacity, like I said, about two years ago.

CHAIRMAN ROGERS: Mr. Thompson, do you want to give a little background of your own experience?

How long have you been with Thiokol?

MR. THOMPSON: I have been with Thiokol 21 years.

CHAIRMAN ROGERS: How many, 31?

MR. THOMPSON: Twenty-one years.

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CHAIRMAN ROGERS: And during that time—well, what is your present job?

MR. THOMPSON: I am Supervisor of Structures Design for rocket motor cases, metallic rocket motor cases.

CHAIRMAN ROGERS: How many men work for you now?

MR. THOMPSON: About 25 people.

CHAIRMAN ROGERS: Are they mostly engineers?

MR. THOMPSON: Yes, sir.

CHAIRMAN ROGERS: And have you had extensive experience in connection with the solid fuel booster rocket and the O-rings and the seals that we have been talking about?

MR. THOMPSON: Yes, I would say so. I have been involved for about seven or eight years in the case program. I first came into it right after the first hydroproof of the cylindrical sections.

CHAIRMAN ROGERS: Mr. Boisjoly, do you have engineers working for you or with you?

MR. BOISJOLY: No, I am strictly in a staff position at this present time.

CHAIRMAN ROGERS: Do you want to go back a little bit? You have been doing some extra work in connection with the seals and the O-rings.

Do you want to describe that to the

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Commission, please?

MR. BOISJOLY: Yes. As a result of my involvement in hardware inspections after the vehicles had flown at Kennedy, I got involved quite heavily in some of the flight readiness reviews that presented problem areas with the seals, namely, the erosion that was going on.

CHAIRMAN ROGERS: Do you want to move the microphone just a little bit closer, please?

MR. BOISJOLY: I was involved in going down to the Cape and inspecting the hardware after it flew, and any of the hardware that experienced problems with the seals, namely erosion, I was also involved in the preparation and sometimes the presentations and flight readiness reviews along with Al McDonald.

CHAIRMAN ROGERS: So you have made some of the actual inspections we have been talking about this morning in connection with erosion and blow-by and so forth?

MR. BOISJOLY: I was the person who was there when SRM-15 was disassembled. I was the person that inspected those joints and reported on those joints, took the samples of the blackened grease, characterized the degrees of arc that the blackened grease existed, directed the photographs taken of the joint, took the

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samples for a chemical analysis back to Morton Thiokol for analysis of what the blackened condition actually was, and then prepared the presentations to give to Marshall on the condition of those joints.

CHAIRMAN ROGERS: So you had as much firsthand experience as probably anybody else in the field?

MR. BOISJOLY: That is correct.

VICE CHAIRMAN ARMSTRONG: Perhaps you could characterize for us the concern and activities that went on with respect to the joints, starting back in the time before the August of 1985 time period, or wherever before that where you should start.

MR. BOISJOLY: Yes.

Just along about SRM-8 or thereabouts—and I could be off a vehicle or two either way—we had previously to that experienced putty going around the end of the clevis and up to the primary O-ring and actually sometimes into the groove, and there was some concern expressed about that situation because the putty has an asbestos filler in it which is a fibrous filler, and the concern was that the fibers might interfere with the sealing capability of the O-rings.

Therefore, a change was made to lay up the putty in a little bit different character on the end of the clevis leg such that when you mate it, you would not

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see that characteristic amount of putty down there next to the O-ring. And by observation, that intent was successful because from that time on I never saw putty past the chamfer on the end of the clevis leg, and never around the corner on the radial surface and down toward the seal.

Then we would report on the erosion characteristics on the seals as we observed them. Like I said, we photographed them. We documented the degree of arc of any of the damage, both heat affected and erosion, and the erosion generally is very localized removal of the material in the O-rings, and it goes some distance longitudinally, and as the charts explain that you have, and we would report on that, and that became a focal point for discussion as a problem area for the next flight readiness review.

So basically that was my task at that time.

VICE CHAIRMAN ARMSTRONG: Was there an increase in concern in mid-1985 as a result of the observations that you and others had made?

MR. BOISJOLY: Yes. SRM-15 actually increased that concern because that was the first time we had actually penetrated a primary O-ring on a field joint with hot gas, and we had a witness of that event because the grease between the O-rings was blackened just like

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coal for those arc lengths that were described in the charts, and that was so much more significant than had ever been seen before on any blow-by on any joint.

MR. FEYNMAN: When was that flight?

MR. BOISJOLY: That was a year ago last January, and the reason it was so vivid in my mind is because I was at the Cape prior to the launch, giving the presentation to approximately 130 people at the launch facility on the inspection techniques on the joint to be used to ensure that any defects which had been removed from the joint were removed in a manner such that the transition on the defect was very smooth, so that the elastomeric condition of the O-ring could actually get into those imperfections because it had been smoothed out. And being elastomer, it would do just that.

And I remember watching the launch, and the launch—I went out of the VAB, and it was an afternoon launch, my recollection is sometime between 1:00 and 2:00 o'clock in the afternoon, I'm not sure, but it was reasonably cool, but yet I went out without a suit coat. During the presentation of that vehicle, after it was inspected, it was calculated by the folks back at the plant, the heat transfer folks, that the seal temperature would have been approximately 53

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degrees for that launch, and that was part of the characterization of the presentation in the flight readiness review for the next vehicle.

Now, that presentation was a very pointed discussion, a very detailed discussion, and quite frankly, I commented to Al McDonald once or twice that had I not been personally there and been personally witnessing what I had seen and presenting what I had physically seen, I don't think we could have been able to convince NASA to continue to fly.

I felt very strong about that fact because I spoke from conviction, and I was challenged by just about everybody in the room about what I had reported, and I was very grateful that I was there and was able to stick by my convictions because I had seen the condition of the joint.

GENERAL KUTYNA: Mr. Boisjoly, on 51-C, you saw a very blackened joint.

MR. BOISJOLY: Two of them.

GENERAL KUTYNA: Well, however, on 61-A, the temperature was 75 degrees, and that has been used to minimize the effects on 51-C, saying that it occurs at warm temperature also.

Would you explain the difference that you saw or heard of between the two joints?

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MR. BOISJOLY: Yes. the difference, the SRM-22 that was used as a comparison basis was not one that I saw personally but a junior engineer, or a younger—I shouldn't say junior, but a younger engineer who works for Arnie had actually been at the Cape and seen those joints. I questioned him, and asked him to tell me in his words what he saw. What he told me was that he saw light grayish to dark grayish splotches that occurred over an arc length of approximately 40 degrees.

Then we discussed the photos. He had a photo of that joint, and I had a photo package of the SRM-15, and we put the photos side by side, and you could see that the SRM-15 joint had coal black color where we had photographed it, and the joint was as he described; it was not coal black, it was significantly lighter and a little bit splotchy. The splotchiness wasn't so definitive, but that is how he defined it to me.

Now, on that basis, I concluded, and so presented the night before the launch, that that was extremely significant. It was very significant because it was telling us that temperature was indeed a discriminator, and that although I couldn't quantify what that discriminator was from an actual threshold standpoint, in other words, I couldn't say you can fly

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above this temperature, you can't fly below this temperature, I couldn't do that. I don't know of anybody that could do that with the data we had at the time, but I was extremely concerned, and my concern ran deeper than SRM-15. It ran as a result of the SRM-16 nozzle which the primary nozzle was compromised, and we presented that as having never sealed, and as having happened in the ignition transient, in launching that vehicle.

Now, that was characterized as a seal that never sealed because it either was not properly leak checked, because of what was explained before—I shouldn't say not properly leak checked, but it was leak checked at 100 psi, which we knew, we had data that said that putty could mask a leak check at that level. We also postulated that maybe there was nothing wrong with the leak check and maybe in the transfer of the O-ring in the groove, it had simply ridden up on a piece of contamination and never did seal.

But the fact was that now you introduced another phenomenon. You have impingement erosion and bypass erosion, and the O-ring material gets removed from the cross section of the O-

ring much, much faster when you have bypass erosion or blow-by, as people have been terming it.

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We usually use the characteristic blow-by to define gas past it, and we use the other term to indicate that we are eroding at the same time. And so you can have blow-by without erosion, you can have blow-by with erosion.

Now, I don't remember the temperature of that particular vehicle, but that turned the gain up very, very high on the seal problem, extremely high in my mind. We were just playing with a dangerous situation.

So you have seen the packages that I have turned in, and you have probably read the memo that I wrote and expressed the concern that I had to make absolutely sure in everybody's mind that management was aware of my concerns, seeing that I was one of the fellows that was most involved with that particular situation.

VICE CHAIRMAN ARMSTRONG: Let me ask, since you are on the task force—

MR. BOISJOLY: Yes.

VICE CHAIRMAN ARMSTRONG: Tell us the history of the establishment of the task force and what they were charged to do.

MR. BOISJOLY: We were charged to look at alternate designs in the joint upstream of the O-rings themselves, and to also look at the seal design as it

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relates to the secondary seal. And that was to try to come up with something that would replace the putty, would be of a vented design and assure that we got gas pressurization to the seal, the primary seal, also look at an alternative secondary seal design which would indeed follow the gap opening of the metal parts such that should a primary O-ring be compromised in any manner, that the secondary seal would be in a position because it was in contact with both sides of the metal parts, to be pressurized, pressure actuated, and cause the seal to be effected. And that was the primary task of this team.

VICE CHAIRMAN ARMSTRONG: To what extent were temperature considerations involved in the work of the task force?

MR. BOISJOLY: To the extent that we had run tests, or Arnie actually had them run at the lab, but I was aware of them, we had run tests on O-ring resiliency, and from the team standpoint we ceased to talk about an O-ring and started to talk about seal because some of the seals under investigation are not O-rings; they are cross sections that have metallic leaf springs in them such that you will get a mechanical assist to force the elastomer to remain in contact such that if you did violate a primary seal, you would be

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capable of pressurization.

So with that information, we were looking at the whole regime. We were trying to get resiliency data run at the plant since last October, which I mentioned in my statement, and also that night mentioned over the phone, and we had trouble because we kept having machine breakdowns, we kept having priority problems, and here we were, three months later, and we still didn't have the data necessary for us to pursue it even further.

So that night we presented the information on the basis of what we knew, and what we knew—and I feel very strongly about it—is that we had a problem with temperature, and it was away from the direction of goodness, and that is my phrase because I can't quantify it, but I always use that. It is away from the direction of goodness. And we had run tests to show that

the seal never lifted off in a resiliency test at 100 degrees Fahrenheit. We had a data point that night that showed that the seal lifted off in the ignition cycle for 2 1/2 seconds, or 2.4 seconds at room temperature, 75 degrees. We showed that it lifted off and remained lifted off for in excess of 10 minutes, because we stopped the test at 10 minutes at 50 degrees.

Now, with that testing that was run initially, I through my concern and the joints that I had

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witnessed, had asked them to please run one more, and that was to back off the instrom machine platens that had compressed the seal 10/1000 inch, and see what happens. And they did that, and it remained seated. That was my comfort zone. I am the one that made the chart that Al McDonald presented at Washington in August which said that if you go from zero to 170 milliseconds, you have a high probability of a good secondary seal. If you go from 170 to 330 milliseconds, you have a reduced probability of a secondary seal. And if you go beyond that point to full pressurization, you most assuredly might not have a secondary seal.

Now, that was based on the information that I had at the time, and I just went in and estimated those from a pressure trace, pressure time trace, and made that judgment. And that was the basis of my concern, and that was also the basis of continuing to fly because it happens—either it happens or it doesn't happen right at the very first threshold of the ignition transient.

VICE CHAIRMAN ARMSTRONG: Mr. Thompson, in your group, were you or any of your people working this problem as well, or aware of this?

MR. THOMPSON: Yes, sir, we were. Right after the January witness of SRM-15, and the disastrous

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effects, in my judgment, on the O-ring situation, and seeing lots of soot in both the primary O-rings of the field joints and between the primary and secondary field joints and the nozzle, it appeared to me that some additional tests needed to be run. And as Roger mentioned, the resilience test was one of the first because then, if we were getting soot blow-by by the primary O-ring, we had better be checking and making sure that our secondary is in good shape.

So we devised a test to squeeze an O-ring down a nominal amount and then come off it about two inches per minute in an instrom machine, and I would like to quantify that just a little bit because if one looks at the rate at which the surfaces separate, it is a nonlinear function. It has kind of a toe on it at first, and then it has a fairly high rate over an inflection point, and then kind of comes over the top. And if one takes a secant from time zero to the top point, which is about, oh, 42/1000 at .6 second, that secant was on the order of 3.2 inches per minute.

And the tests that we had run up to this point, including the night that we were discussing this, was 2 inches per minute. And additionally, the concern is if one looks at the steeper slope after you come off the toe through the inflection point, that that slope is

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on the order of 10 inches per minute.

So what it means to me, and I hope to others, is that it is a very, very high rate shortly after it comes off, on the order of 100 to 200 milliseconds. So the message I believe, and I tried to convey, was for 100 milliseconds to 200 milliseconds, Roger has indicated 170, you really probably do have a pretty good chance for a seal. Thereafter you do not.

And so, to further answer the question—and I hope I'm not too long—is that blow-by, of course, was an issue. And I set up a series of tests to try and determine the factors that affect

blow-by, such as squeeze, such as temperature, and in fact, attempt to measure the blow-by using argon at first, and then subsequently we changed to freon because it is a better witness.

MR. SUTTER: Can I ask a question about these tests to measure the rate of gap opening, if that is the proper term?

Was this on an actual set of hardware, or was it a test vehicle, and how would tolerances influence that? Could you get quite a varied answer depending upon whether you had a tight joint or whether you had a system that was close to the other end of the tolerance?

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MR. THOMPSON: Yes, sir. what we did, recognizing this is, first of all, it was a bench test, it was a three inch piece of O-ring, in fact, two inch piece of O-ring that was squeezed to about a nominal squeeze, which is like 40/1000. Many of our joints have about that squeeze. The particular one in question in the incident was 35/1000, 37/1000. And so we just merely would give it the squeeze, and then back off at the simulated rate that we could do on the instrom machine.

MR. SUTTER: Well, do you think you might get a variation in results on actual flight hardware then, depending upon the various production runs on the cases?

MR. THOMPSON: Yes, sir, we do. In fact, if you have additional squeeze, you not only have the two surfaces, the one surface pushing off, but if it is a lot of squeeze and a small O-ring groove, the side surface of the O-ring probably can also help, and that gives you additional energy to help it respond.

MR. SUTTER: Well, what about the other directions where the tolerances are on the other side?

MR. THOMPSON: We investigated tolerances and gaps, and we have set up a matrix to do that very thing. This was kind of a preliminary test we had run, and as Roger mentioned, we have refined the tests and

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have some results now, but after the fact, and the tests included squeeze and included temperature, and it also, the variable rate that we think we really have in the motor, we have simulated that by input, using a function generator as an input to the instrom machine so that we could generate the actual motion as we saw it.

MR. SUTTER: But with the fact that the testing is somewhat late and that there's a lot of variables and there's been a lot of commentary on temperature, could you come up with a situation where temperature and tolerances could develop a submarginal joint and that of the 144 cases that have been fired, you could run the gamut from having very good ones to very bad ones?

MR. THOMPSON: Yes, sir, we could. We ran, as I say, a preliminary run that showed that it was a problem.

MR. SUTTER: But would all of the parties be aware that you could have wild variations, then, that—and couldn't this develop quite a concern on everybody's part that the system was pretty tender?

MR. THOMPSON: In my feeling, the system was tender, and this is the reason I objected that night.

MR. BOISJOLY: That was the point we tried to make that night, is because we didn't have sufficient

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data.

MR. SUTTER: Well, my question gets around to why wouldn't everybody have that feeling that the situation was very tender, and why wouldn't everybody develop a feeling of great concern, and then by that means develop a very, very conservative attitude toward approaching a launch?

MR. BOISJOLY: That is what we thought happened when the original conclusions and recommendations were made because they were quite consistent with what we had presented prior to the meeting and prior to making the charts.

CHAIRMAN ROGERS: Can I interrupt just a second? I would like to come to the night of the launch and the launch itself, or the decisions, in a little while, but I would like to go back just a minute to some of the background before we get to that point, to show your concern as expressed to management.

I have here a letter which appears to be one signed by you dated July 31, 1985. And I will ask Dr. Keel to give it to you.

And I gather from the files that we have received from you, you wrote a series of letters or memos, I guess memos is a better way to describe it.

MR. BOISJOLY: Those were activity reports.

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CHAIRMAN ROGERS: Expressing your concern about this problem of the seals and the O-rings and so forth.

And I would ask you, if you don't mind, to read that memorandum dated July 31, which you wrote to R. K. Lund, who is Vice President of Engineering. [Ref. 2/25-11]

Would you mind?

MR. BOISJOLY: Yes.

"This letter is written to ensure that management is fully aware of the seriousness of the current O-ring erosion problem in the SRM joints from an engineering standpoint. The mistakenly accepted position on the joint problem was to fly without fear of failure and to run a series of design evaluations which would ultimately lead to a solution or at least a significant reduction of the erosion problem.

"This position is now drastically changed as a result of the SRM-16A nozzle joint erosion which eroded a secondary O-ring with the primary O-ring never sealing. If the same scenario should occur in a field joint (and it could) then it is a jump ball as to the success or failure of the joint because the secondary O-ring cannot respond to the clevis opening rate and may not be capable of pressurization. The result would be a catastrophe of the highest order—loss of human life.

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"An unofficial team (a memo defining the team and its purpose was never published) with leader was formed on 19 July 1985 and was tasked with solving the problem for both the short and the long term. This unofficial team is essentially nonexistent at this time. In my opinion, the team must be officially given the responsibility and the authority to execute the work that needs to be done on a non-interference basis (full time assignment until completed).

"It is my honest and very real fear that if we do not take immediate action to dedicate a team to solve the problem with the field joint having the number one priority, then we stand in jeopardy of losing a flight along with all of the launch pad facilities."

Then I signed it, and a manager that I worked for countersigned it as concurred.

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CHAIRMAN ROGERS: Thank you.

I assume, Mr. Thompson, you agree with the contents of that memorandum?

MR. THOMPSON: Absolutely, yes, sir.

CHAIRMAN ROGERS: Unless the Commission has any further questions, I would like to now go to the January 27 and January 28, to ask you, Mr. Boisjoly, to describe in your own words your first involvement in those discussions and what was said, who took part, and your position, and later on, your position.

MR. BOISJOLY: Okay. My first—

CHAIRMAN ROGERS: And can I say to you also, just before you start—I'm sorry to interrupt, but you did make notes on the 27th about the events at that time?

MR. BOISJOLY: Yes, I did. I made them after the meeting. [Ref. 2/14-15]

CHAIRMAN ROGERS: But on the same day.

MR. BOISJOLY: Yes, and then I made a few more notes on the day of the launch, prior to the launch, and then I made a short entry after the launch.

CHAIRMAN ROGERS: But the notes on the 27th and the 28th were made on those days?

MR. BOISJOLY: Yes, they were.

CHAIRMAN ROGERS: And the last one was made

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after the accident?

MR. BOISJOLY: That is correct.

CHAIRMAN ROGERS: In your discussion you may refer to those notes, and if you have no objection, we may make them public.

MR. BOISJOLY: I kind of just paraphrased them and made them on three by five cards, but I think they essentially have the content of what is in those notes—my first knowledge about any temperature concern on the launch was approximately 1:00 p.m. on the 27th of January. There was a preliminary meeting held to discuss the concern at the low temperature for resiliency, soot blow-by, previous launch and static test temperatures, and there were several, well, there were quite a few people in that meeting, most of whom were engineers, some of whom were program management. I don't remember the names, honestly.

Anyway, that meeting concluded with the tempo that there was a valid concern for temperature, low temperature. I also heard sometime afterwards that it had been cold for several days, and what really impressed me about this piece of information was that it was opposite what SRM-15 had experienced, and that concerned me because when I got off the plane a year ago last January for the SRM-15 launch, it was very cold,

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like 17 or 18 degrees when I got off the airplane at Orlando. The next few days, although it remained cold, it got warmer, and by launch time, like I said, it was probably in the 60s, but it did get warmer.

Now, what was concerning me about this piece of information was it was the reverse, it had been cold for several days and was getting colder. And that is why I noted that because that just turned the gain up on my concern because that was away from goodness again. SRM-15 that I am referring to was the coldest launch up to that point in time.

Okay, I felt we were very successful in convincing engineering and management of the problem. By now we heard that the overnight low was predicted to be 18 degrees Fahrenheit, and again my concern deepened because of what I just spoke, it was going the wrong direction from the past experience base.

A telecon was set up with Marshall Space Flight Center and Kennedy Space Center to present data over our concern about the low temperature. Now, there had been some meetings

in between, and quite frankly, I am almost a total blank because I was down in my office, I ran a calculation that showed that the O-ring shrinkage due to low temperatures and dropping from 75 degrees to 25 degrees, it was just a rough number, was

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3/1000 inch, and that was due to both the shrinkage on the diameter and the effect of temperature stretching it around its circumference and thus causing the diameter to shrink down again. There was roughly a fifty-fifty split in those numbers, but the bottom line was it was about 3/1000 inch.

Okay, that shows me that squeeze wasn't really an effect. Squeeze was not an issue. It never has been an issue with me because if you look at all the eroded O-rings, they don't correlate with squeeze. The O-rings that you would expect to be eroded are those that have the lowest squeeze, and that is not the case. So squeeze was never an issue, but I ran the numbers anyway to assure myself that it was okay, which it was.

Okay, the telecon data—and I must emphasize this. We had very little time to prepare data for that telecon. When it was decided in a group in our conference room that we would take a systematic approach and assigned certain individuals certain tasks to prepare, we all scurried to our individual locations and prepared that information in a timely manner. That is why the charts for the most part are hand printed or hand written, because we didn't have time to get them even typed.

Each person during the telecon presented their

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own charts. Just for the record, I presented Charts 2-1, 2-2, 2-3, and I also presented 3-1, which I did not prepare but which was sequenced right after one that I did, and I was asked just to present it, which I did. I presented 4-1 and 5-1. I expressed deep concern about launching at low temperature. I presented Chart 2-1 with emphasis—now, 2-1, if you want to see it, I have it, but basically that was the chart that summarized the primary concerns, and that was the chart that I pulled right out of the Washington presentation without changing one word of it because it was still applicable, and it addresses the highest concern of the field joint in both the ignition transient condition and the steady state condition, and it really sets down the rationale for why we were continuing to fly. Basically, if erosion penetrates the primary O-ring seal, there is a higher probability of no secondary seal capability in the steady state condition. And I had two sub-bullets under that which stated bench testing showed O-ring not capable of maintaining contact with metal parts, gap, opening rate to maximum operating pressure. I had another bullet which stated bench testing showed capability to maintain O-ring contact during initial phase (0 to 170 milliseconds of transient). That was my comfort basis of continuing to fly under normal

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circumstances, normal being within the data base we had.

I emphasized when I presented that chart about the changing of the timing function of the O-ring as it attempted to seal. I was concerned that we may go from that first beginning region into that intermediate region, from 0 to 170 being the first region, and 170 to 330 being the intermediate region where we didn't have a high probability of sealing or seating.

I then presented Chart 2-2 with added concerns related to the timing function. And basically on that chart I started off talking about a lower temperature than current data base results in changing the primary O-ring sealing timing function, and I discussed the SRM-15 observations, namely, the 15A motor had 80 degrees arc of black grease between the O-rings, and make no mistake about it, when I say black, I mean black just like coal. It was jet black. And SRM-15B

had a 110 degree arc of black grease between the O-rings. We would have low O-ring squeeze due to low temperature which I had calculated earlier in the day. We would have higher O-ring shore hardness, and that is the characteristic similar to the hardness test that is done on metal except that it has a different anvil size, and when you press against the elastomeric material, that is

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the measure of how far a standard size anvil will go into the particular elastomer, and it gives you a measure of relative hardness of the substance.

Now, that would be harder. And what that really means basically is that the harder the material is, it would be likened to trying to shove a brick into a crack versus a sponge. That is a good analogy for purposes of this discussion. I also mentioned that thicker grease, as a result of lower temperatures, would have a higher viscosity. It wouldn't be as slick and slippery as it would be at room temperature. And so it would be a little bit more difficult to move across it.

We would have higher O-ring pressure actuation time, in my opinion, and that is what I presented. If action time—and these are the two. These are the sum and substance of what I just presented. If action time increases, then the threshold of secondary seal pressurization capability is approached. That was my fear. If the threshold is reached, then secondary seal may not be capable of being pressurized, and that was the bottom line of everything that had been presented up to that point.

CHAIRMAN ROGERS: Did anybody take issue with you?

MR. BOISJOLY: Well, I am coming to that. I

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also showed a chart of the joint with an exaggerated cross section to show the seal lifted off, which has been shown to everybody. I was asked, yes, at that point in time I was asked to quantify my concerns, and I said I couldn't. I couldn't quantify it. I had no data to quantify it, but I did say I knew that it was away from goodness in the current data base. Someone on the net commented that we had soot blow-by on SRM-22, which was launched at 75 degrees. I don't remember who made the comment, but that is where the first comment came in about the disparity between my conclusion and the observed data because SRM-22 had blow-by at essentially a room temperature launch.

I then said that SRM-15 had much more blow-by indication and that it was indeed telling us that lower temperature was a factor. This was supported by inspection of flown hardware by myself. I was asked again for data to support my claim, and I said I have none other than what is being presented, and I had been trying to get resilience data, Arnie and I both, since last October, and that statement was mentioned on the net.

Others in the room presented their charts, and the main telecon session concluded with Bob Lund, who is our Vice President of Engineering, presenting his

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conclusions and recommendations charts which were based on our data input up to that point. Listeners on the telecon were not pleased with the conclusions and the recommendations.

CHAIRMAN ROGERS: What was the conclusion?

MR. BOISJOLY: The conclusions were that we should not fly outside of our data base, which was 53 degrees. Those were the conclusions. And we were quite pleased because we knew in advance, having participated in the preparation, what the conclusions were, and we felt very comfortable with that.

MR. ACHESON: Who presented that conclusion?

MR. BOISJOLY: Mr. Bob Lund. He had prepared those charts. He had input from other people. He had actually physically prepared the charts. It was about that time that Mr. Hardy from Marshall was asked what he thought about the MTI recommendation, and he said he was appalled at the MTI decision. Mr. Hardy was also asked about launching, and he said no, not if the contractor recommended not launching, he would not go against the contractor and launch.

There was a short discussion that ensued about temperature not being a discriminator between SRM-15 and 22, and shortly after, I believe it was Mr. Kilminster asked if—excuse me. I'm getting confused here. Mr.

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Kilminster was asked by NASA if he would launch, and he said no because the engineering recommendation was not to launch.

Then MTI management then asked for a five minute caucus. I'm not sure exactly who asked for that, but it was asked in such a manner that I remember it was asked for, a five minute caucus, which we put on—the line on mute and went offline with the rest of the net.

CHAIRMAN ROGERS: Mr. Boisjoly, at the time that you made the—that Thiokol made the recommendation not to launch, was that a unanimous recommendation as far as you knew?

MR. BOISJOLY: Yes. I have to make something clear. I have been distressed by the things that have been appearing in the paper and things that have been said in general, and there was never one positive, pro-launch statement ever made by anybody. There have been some feelings since then that folks have expressed that they would support the decision, but there was not one positive statement for launch ever made in that room.

Picking up where the caucus started—

CHAIRMAN ROGERS: Excuse me. Is that your recollection, too, Mr. Thompson?

MR. THOMPSON: Yes, particularly in the

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caucus. I think Roger and I were the only people that expressed our views.

CHAIRMAN ROGERS: But nobody expressed a view that you should proceed with the launch at that time?

MR. THOMPSON: Not amongst the people that I was able to hear.

GENERAL KUTYNA: Mr. Boisjoly, you said there was some confusion in your mind as to Mr. McDonald's position at the time he came out with the comment about the secondary ring seating, that you should consider that?

MR. BOISJOLY: That is correct. When Al first came on the line and explained the situation about the primary and secondary O-ring and how the secondary O-ring was in position, I had a flash of confusion in my mind as to what he was saying, but I, having worked on the SRM-15 presentation and the SRM-16 presentation, realized exactly what he was trying to say. But I did have that tinge at the beginning, and so there was definite misunderstanding about Al's statement, there's no question in my mind, because I had it, and I was probably closer to that joint than most anybody else, and I had that tinge, and I so stated that to you.

Okay, the caucus was started by Mr. Mason stating that a management decision was necessary. Those of us who

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were opposed the launch continued to speak out, and I am specifically speaking of Mr. Thompson and myself because in my recollection he and I were the only ones that vigorously continued to oppose the launch. And we were attempting to go back and rereview and try to make clear what we were trying to get across, and we couldn't understand why it was going to be reversed.

So we spoke out and tried to explain once again the effects of low temperature. Arnie actually got up from his position which was down the table, and walked up the table and put a quad pad down in front of the table, in front of the management folks, and tried to sketch out once again what his concern was with the joint, and when he realized he wasn't getting through, he just stopped.

I tried one more time with the photos. I grabbed the photos, and I went up and discussed the photos once again and tried to make the point that it was my opinion from actual observations that temperature was indeed a discriminator and we should not ignore the physical evidence that we had observed.

And again, I brought up the point that SRM-15 had a 110 degree arc of black grease while SRM-22 had a relatively different amount, which was less and wasn't quite as black. I also stopped when it was apparent

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that I couldn't get anybody to listen.

DR. WALKER: At this point did anyone else speak up in favor of the launch?

MR. BOISJOLY: No, sir. No one said anything, in my recollection, nobody said a word. It was then being discussed amongst the management folks. After Arnie and I had our last say, Mr. Mason said we have to make a management decision. He turned to Bob Lund and asked him to take off his engineering hat and put on his management hat. From this point on, management formulated the points to base their decision on. There was never one comment in favor, as I have said, of launching by any engineer or other nonmanagement person in the room before or after the caucus. I was not even asked to participate in giving any input to the final decision charts.

I went back on the net with the final charts or final chart, which was the rationale for launching, and that was presented by Mr. Kilminster. It was hand written on a notepad, and he read from that notepad. I did not agree with some of the statements that were being made to support the decision. I was never asked nor polled, and it was clearly a management decision from that point.

I must emphasize, I had my say, and I never

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take any management right to take the input of an engineer and then make a decision based upon that input, and I truly believe that. I have worked at a lot of companies, and that has been done from time to time, and I truly believe that, and so there was no point in me doing anything any further than I had already attempted to do.

I did not see the final version of the chart until the next day. I just heard it read. I left the room feeling badly defeated, but I felt I really did all I could to stop the launch.

I felt personally that management was under a lot of pressure to launch and that they made a very tough decision, but I didn't agree with it.

One of my colleagues that was in the meeting summed it up best. This was a meeting where the determination was to launch, and it was up to us to prove beyond a shadow of a doubt that it was not safe to do so. This is in total reverse to what the position usually is in a preflight conversation or a flight readiness review. It is usually exactly opposite that.

DR. WALKER: Do you know the source of the pressure on management that you alluded to?

MR. BOISJOLY: Well, the comments made over the net is what I felt, I can't speak for them, but I

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felt it—I felt the tone of the meeting exactly as I summed up, that we were being put in a position to prove that we should not launch rather than being put in the position and prove that we had enough data to launch. And I felt that very real.

DR. WALKER: These were the comments from the NASA people at Marshall and at KSC?

MR. BOISJOLY: Yes.

MR. FEYNMAN: I take it you were trying, you were asked to prove that the seal would fail?

MR. BOISJOLY: Yes.

MR. FEYNMAN: And of course, you couldn't, because as a matter of fact it didn't. That is, five of them didn't, and only one of them did, and if you had proved that they would have all failed, you would have found yourself incorrect and under criticism because five of them didn't fail.

MR. BOISJOLY: That is right. I was very concerned that the cold temperatures would change that timing and put us in another regime, and that was the whole basis of my fighting that night.

MR. FEYNMAN: It is just that the probability had been increased to a point where it was intolerable?

MR. BOISJOLY: That's right.

MR. ACHESON: Mr. Boisjoly, your correspondence in the summer of 1985 indicates that the

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heightened sensitivity to the O-ring problem at Thiokol was really initiated by heightened sensitivity about it at NASA, and for example, reference is made to the problem that has escalated so badly in the eyes of everyone, especially our customer, NASA.

Now, was any mention made during the telecon net with NASA the evening of January 27 of how the NASA view had moved from this heightened sensitivity to the O-ring problem to the position that they were now putting forward at the January 27 telecon?

MR. BOISJOLY: No, that wasn't mentioned, but in fact, they were just as concerned as we, our counterparts that we worked with, that they were right on top of the seal task force team with us, and we had status reviews going on all the time. In fact, when any time we would hit a situation where they felt we weren't going quick enough, we ended up having a visit, and they would just be there and watch over our shoulders and make darned sure that we were proceeding in a timely manner. And so that to me told me that they were just as concerned about it as we were.

DR. WALKER: Mr. Boisjoly, could you tell me something more about the task team that was set up? You alluded to it in your letter of 31 July, and you asked that that team be reconstituted and made more urgent.

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Did that happen?

MR. BOISJOLY: In a way.

DR. WALKER: Who was on that team?

MR. BOISJOLY: Don Ketner headed the team up. I was a member. Brian Russell was a member.

DR. WALKER: Were there any NASA members?

MR. BOISJOLY: They had a counterpart team, yes. They had members of their team. Our main interface was with a gentleman named Jerry Peoples and Ron McIntosh, but they had their folks involved also.

DR. WALKER: You had meetings with representatives from both NASA and Thiokol?

MR. BOISJOLY: Yes, we did, when we outlined the plan on how we were going to proceed to fix this problem, they were part and parcel to that plan.

DR. WALKER: Now, this is the team that evolved a number of solutions to this problem, some of which have already been publicized.

MR. BOISJOLY: That is correct.

DR. WALKER: And some of those solutions were short term solutions, and others were long term solutions?

MR. BOISJOLY: We categorized them, at least I did in all my notes, as short term solutions, medium term solutions and long term solutions. And to give you

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a flavor for what put what in what category was I considered a short term solution, for instance, a 292 diameter O-ring which would have afforded us 12/1000 more squeeze and still not have an overfill condition in the groove with the elastomeric material. Also couple that with selective shimming that would make the tang and clevis more concentric to one another, and take as much of the free volume gap on the outside away to make sure that we were putting more and more squeeze on the O-ring and less and less chance for the gap to open up to the rate that it is opening.

DR. WALKER: Do you have some estimate of how long the short term solutions would have taken?

MR. BOISJOLY: Yes. We had assembly tested a 292 O-ring in some static motor joints—excuse me, some inert motor joints, and put them together and found that we had good success with that. They are currently installed in the aft two field joints of the QM-5 test motor that was ready to fly or fire about two weeks ago at our plant. And so we had made that progress, and we were well on our way to showing that those seals would work in a joint.

And I must emphasize that the QM-5 motor is a filament wound case motor, and it is a joint with a capture feature on it that doesn't have the gap

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opening. But the major issue on the 292 O-ring was the assembly characteristics of it, and we were getting like 12/1000 inch squeeze for free. Basically that was the short term issue that we could implement as quick as possible.

DR. WALKER: Did the team report on a regular basis to both NASA and Thiokol management?

MR. BOISJOLY: We had status reports with the Marshall folks at regular intervals, and we had a Friday teleconference at mid-day and statused the proceedings of that previous week with them.

DR. WALKER: So everyone in the telecon on January 27 was aware of the work of this team, the solutions that it had proposed?

MR. BOISJOLY: I don't know that everyone was, but there were a number of people that were, yes. I don't know. I cannot make the statement that everybody that was on the telecon knew about that. For instance, I had never heard Cecil Houston's name before. I didn't know that he was a Marshall type until I heard on the telecon afterwards that he was down at KSC. I had not met that gentleman, so I don't know if he knew it or not. And I don't know if some of

the other folks knew, but there were enough folks on that telecon that knew what the status was.

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DR. WALKER: What about Mr. Malloy and Mr. Hardy? Do you know if they knew?

MR. BOISJOLY: I don't have sure knowledge of that, no, I don't know.

MR. FEYNMAN: I want to get away for a moment from the temperature question which comes up later, and go back to the earlier time when you were worrying about these things, and I would like to know whether already you felt that there was a serious problem in flying even at 53 degrees, where the following logic is possible, perhaps. And you can correct me.

If their primary ring would start to erode and would take enough time to erode that it takes, the pressure builds up to 600 psi or so, then if there is joint rotation from the result of that, the secondary seal may fail, if the erosion would get all the way through the primary seal. The erosion got through a third of the way of the primary seal in fact in 15C.

Do you feel that you understand the process of erosion well enough to know that it was unlikely to go three times as far?

MR. BOISJOLY: I think we need to clarify one thing. One third of the seal was not eroded. One third of the allowable erosion number occurred, and you really have to clarify that because we are talking about a seal

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diameter that is 280 in diameter, okay, and we really eroded about 32/1000 inch on SRM-15, and the factor of three is being used against subscale data which showed that we were able to seal an eroded seal on a subscale rig that had 10 inch diameter seals, that up to an eighth of an inch. Then we had a window where we failed to seal, and then we had another one up to I believe it was 145/1000 or 147/1000 of an inch that also sealed.

And now, the analysis from the gas dynamics standpoint, which is out of my area, had been conducted, and they found an excellent math model of that erosion process, and it correlated extremely well with the subscale test data which had been run about a year before, and there were 27 tests that characterized that erosion. They ran most of them at 3 seconds, and then my recollection is there was one or two 5 second tests and one or two 20 some odd second tests to show that it was indeed at ignition transient, and what that test was all about was they had a metal orifice with a rectangular orifice that was 40/1000 wide and had different characteristic heights, and they ran a parametric study using those tests, that test rig, and determined what the erosion rate, or not the rate, but the erosion depth was as a function of the orifice

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size.

Then the math model was made and characterized that, and my recollection is they were within 12 percent of all of the test data, and they were very pleased at the characterization analytically of what was going on.

So from an erosion standpoint, I think it is fair to say that there was indeed sufficient margin, but that was not the issue. Erosion was not the issue. What was the issue is can you stand a longer period of time in an attempt to seal before the erosion eats you alive and you don't have a seal, period? Because once you get flow you have two types of erosions. You have blow-by erosion and impingement erosion. The 27 tests characterized impingement erosion only. There were no blow-bys in that.

And I might say, too, we ran tests on that same rig without an orifice and pressurized it exactly the same way on an unprotected O-ring, and there was not even the sheen missing on the O-ring. So the analysis characterized that very well and they understood it very well.

What it is, it is a volume filling problem as a function of time. The higher the volume, the more the time the hot gas flows, if you have a blow-by through the putty, that will enable you to get erosion, and I felt

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very confident they had a good analytical handle on that.

MR. FEYNMAN: Thank you. I have another question now on the other side, which is that blow-by early on, that is, in the first whatever you call it, 170 milliseconds, during which it was supposed that the secondary ring would be close enough to the right place to be sealed, but now I am going to talk about the lower temperature.

Under those circumstances where the thing is standing around and it has no resilience, and in fact, when the engine goes off it swings or twangs back and forth and there are various stresses that have been there since the time the ring has been pushed into place by the test, which I now discover was 28 days earlier, not just three days earlier; isn't it possible that the secondary ring has gotten squoze or squeezed to a smaller size than it should be, and then when it bends back it leaves a hole, that there is no 170 milliseconds at all, that it is already too small because it has so low a resilience time and it takes so long to respond after it has been squeezed.

MR. BOISJOLY: That is possible. I had done a series of tests relating clevis opening problems back about three years ago where we had experienced a

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phenomenon where the clevis was opening up, oh, maybe 3/1000 to 5/1000 inch over its original manufactured dimensions, and we tried to run that down and pin that down as to where that came from, and in the attempt to do that I ran a series of tests which took an empty cylinder segment, and I actually characterized when it was round what happened to the clevis, when I ovalized the case what happened to the clevis, when I took the case and put shims underneath the longitudinal direction, what happened to the clevis, and characteristically with very large numbers, like when I ovalized it to a value of approximately an inch, I could only affect the clevis gap opening say several thousandths of an inch.

And so, even though you get motion, it was not characterized as a major motion relative to the squeeze that we had. But in reference to temperature, which is what you are talking about, yes, you could have very well had that situation.

MR. HOTZ: Mr. Boisjoly, we have had some data, you probably know, on experience with water accumulating in the field joint while it is standing out there on the stack. As Dr. Feynman pointed out, 51-L stood out there for 28 days during which there were some heavy rainstorms, wind driven rain.

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What would be the effect—and then we also had cold temperatures the night before and a couple of nights earlier. What would be the effect if water accumulated in the field joint and then froze during these hard freezes?

MR. BOISJOLY: Well, first of all, I had no prior, prelaunch knowledge of that occurring. I have since found out that there was a vehicle that they changed out one of the segments, and when they pulled the pin, water poured out of the joint. I did not know that prior to the launch.

We have been looking at that since the incident and actually have run a test, and the effect on the actual opening of the clevis, the actual spreading of the clevis itself was almost nonmea-

surable, and we attribute that I guess to the fact that you cannot fill the joint, by configuration, all the way up to the secondary O-ring.

Now, if you postulate that there was enough grease in there and you actually moved the grease with the ice, then you could move the O-ring. But that is speculation at this point. It looks from the design configuration of the joint that if you filled the joint with water, you would still have an air column that would probably be on the order of 5/8 to 7/10 inch high,

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and you would simply compress the air column. And it appeared like you wouldn't be able to generate enough pressure to transfer the ring.

But in all fairness, we are running tests to find that out. We haven't completed them yet, or I should say Marshall is doing that.

MR. ACHESON: Mr. Boisjoly, were you at Thiokol when the design of the joint for these SRMs was developed?

MR. BOISJOLY: No, I was not.

MR. ACHESON: But you were there when SRM-8 was fired, were you not?

MR. BOISJOLY: Yes, I was there when SRM-1 was fired.

MR. ACHESON: Can you account for what appears to be a concentration of erosion and blow-by cases in these O-rings following SRM-8 as opposed to prior to SRM-8?

MR. BOISJOLY: That has been a topic of much discussion over the years, and there was a putty change. The original putty was a Randolph putty, and then they simply stopped making it—no, excuse me, not Randolph, Fuller-O'Brien, excuse me—and they simply stopped making it, and then we switched to a Randolph putty, and that is a simultaneous general area of time

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sequence with the changes in the layup of the putty to bring it back around from the edge of the clevis so that we were sure there wasn't any putty down by the O-ring. But that hasn't fully tied together either because the first erosion that was ever indicated, to my knowledge, was on SRM-2, or STS-2, the second flight. In fact, the second flight had the most erosion of any of the O-rings in the field joint.

So we have been struggling with that because the data appears just totally random. If you look at the degree locations, if you look at the temperatures, if you look at—it is random in regards to erosion per se.

MR. ACHESON: Were you at Thiokol when the Critical Items List was amended in December 1982 to make it a Crit 1 instead of a Crit 1R?

MR. BOISJOLY: I was at Thiokol at that time, but you know, I don't even know what a CIL is. I am hearing all of this for the first time. I just don't know.

VICE CHAIRMAN ARMSTRONG: I would like to ask Mr. Thompson, from your point of view in the engineering group, did you feel that perhaps particular constraints needed to be established on the operation of the motor and the case and its joints that hadn't been, and for

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example, temperature related or other function related? And was that passed up the line at all?

MR. THOMPSON: Yes, I did feel impressed to, back in the times of STA-1, which was structural test article, STA-1 structural test article, we discovered that the joint was opening up rather than closing as our original analysis had indicated, and in fact, it was quite a bit. I think it was up to 52/1000 at that time, for the primary O-ring. And at that point, at that same meeting, we went to Marshall and discussed the problem, and we also indicated, in fact, I think it was me that indicated that the secondary O-ring in certain worst on worst conditions pressurized

even without resilience considered, could be opened, could be noncontact with the adjoining surface.

And so we have been working with these problems for the last several years. And of course, you can immediately see our response, both Roger and myself, we know that we need an elastomeric seal very badly, more so than in many hydraulic applications, because if you look at the usual, probably a dynamic seal, but nonetheless, the requirements were for a 70 durometer O-ring at 1,000 psi. People want to talk about 6/1000 or 7/1000, and now we are talking about that type of clearance to start with, but coupled with another 50/1000 of motion. And so it

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is an elastomeric problem of the first order, in my judgment.

And what is required is a visco-elastic type of phenomenon. As we all know, any visco-elastic material is very temperature sensitive.

VICE CHAIRMAN ARMSTRONG: Well, did you in fact, or do you know of specific recommendations being made with respect to establishing limitations of any type on launch?

MR. THOMPSON: Yes, sir. Several changes were made. We looked at the O-ring tolerances. We looked at the O-ring groove tolerances. We looked and we decreased on the side of goodness, as Roger would say, all of those things, and in addition to that, the shim—we called it a centering strip at that time—was 20/1000. We replaced that with an M type clip that goes over the pins in that area, and increased that to 32/1000.

And these are some of the things we have made in an attempt to make the joint better from where it was originally.

VICE CHAIRMAN ARMSTRONG: And all of the recommendations that you made, to your knowledge, were accepted and implemented?

MR. THOMPSON: No, sir, not all of them were

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made. Recently, and as late as August of 1982, I wrote a memo to my management project engineering, indicated that we should stop flying until such point as we could double the squeeze on the O-rings. [Ref. 2/25-12]

CHAIRMAN ROGERS: What was the date of that?

MR. THOMPSON: About the 22nd of August 1985.

CHAIRMAN ROGERS: You said 1982, I think.

MR. THOMPSON: That was 1985. And my recommendation there was to double—I went through an example in the memo—I think it was SRM-20—where if we went to the shim that Mr. Boisjoly has indicated, a full shim, which is about a 55/1000 shim versus a 32/1000 shim, coupled with a 292 O-ring, which is about the largest O-ring we can get in there with confidence that we are not going to nick it when we assemble, that this would about double the squeeze on the O-ring, which we felt was an important thing to do.

And I realized at that point that the O-ring committee that had been dispatched to discover these things had in fact discussed that. But it was my view that we needed to get it into the flight right away.

VICE CHAIRMAN ARMSTRONG: Were any recommendations made with respect to temperature of the seal area at launch, to your knowledge?

MR. THOMPSON: Not at that time. But that is

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one of the reasons, is that if you get additional squeeze on it, particularly the secondary and the primary, the primary will have a better chance of seating with the additional squeeze, and the

secondary will get more help from the side walls of the O-ring groove and help its resiliency problem. And we know that additional squeeze also helps in extending the time that the O-ring is in contact with the seal.

MR. RUMMEL: Mr. Boisjoly mentioned the capture ring a few moments ago as having been included in one of the tests. At what point, if ever, was the capture ring seriously considered for adoption? And as I understand it—and I appreciate your comment on this—the capture ring is a flange which is intended to arrest the opening up of the two-seal—the area of the two seals.

When was that first seriously considered, or has it been seriously considered for production?

MR. THOMPSON: When it was first brought to my attention was in the design of the filament wound case. I believe someone from Marshall suggested it in the adaptors for the filament wound case, and it was I think roughly two and a half or three years ago. And it seemed like a good idea because what it does, it holds that inside flange in against the tang, and our

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finite element analysis indicates that instead of opening on the order of 50/1000, it cuts it down to 5/1000 or 6/1000, which is in the direction that we need to be going.

MR. RUMMEL: Has that been considered for adoption, or has it been adopted?

MR. THOMPSON: Yes, sir. It particularly has come to recent attention because of the heads up program where we have discussed higher pressure going up to 1110 psi.

MR. HOTZ: Mr. Thompson, could you tell us what happened to those recommendations you made in August of 1982, or 1985—I beg your pardon? Were they ever implemented?

MR. THOMPSON: No, sir, they were not.

MR. HOTZ: Do you have any idea of why not? Or did you ever receive a reply back from management explaining that?

MR. THOMPSON: No, sir, I did not. The person that I wrote it to, directly involved in management, has since transferred down into my group again, and so that may have been part of the problem. But management was aware of it because of questions and feedback that I received from it.

MR. HOTZ: But you got no formal response from

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management on it.

MR. THOMPSON: That is correct.

MR. RUMMEL: On the capture flange, I have a couple of more questions.

Apparently it has been considered for some time, and I take it the installation of the flange is an integral part of the case, is that correct?

MR. THOMPSON: Yes, it is part of the tang, and as it comes in first, it engages first on a tapered basis, and then it comes into a cylindrical basis. At first it was attempted to have two tapers fit, but the tolerances became very unreasonable, and so we would engage first on two tapered surfaces and then come into a cylindrical engagement where we could maintain the same type of tolerances that we have on the sealing joint.

MR. RUMMEL: Would that involve a change in the forging process or the manufacturing process, or the forging billet?

MR. THOMPSON: Yes, sir. The inside bark on the forging was not quite sufficient to every time assure that we would be able to find the part.

MR. RUMMEL: Would a change of that magnitude have been practical for your company to undertake without the concurrence and authorization from NASA?

MR. THOMPSON: No. I think that would take a

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high cooperation between ourselves and NASA because it is quite a large undertaking, of course.

MR. RUMMEL: Well, could you clarify for me if that in fact has been undertaken and what the first implementation dates were or might be? Obviously it wasn't on 51-L.

MR. THOMPSON: I think that it is indicated to me that on a general effort, that it was on the order of 18 months, but on the basis of an extremely large effort, a wartime effort I think was the word that was used, it could be made available in 12 to 13 months.

MR. RUMMEL: Are any under process of being manufactured at the present time with the capture flange?

MR. THOMPSON: Yes, I think several forgings have been ordered, and I don't know if it is a full flight set or not. Some other people would have to answer that.

MR. BOISJOLY: I would like to address that on the seal team that was part of the long term fix—and there had been some forgings ordered, and the expected delivery was in February of 1987, and that was in the works, and that was part of the peripheral action, if you will, on the long term basis of the seal task team.

MR. RUMMEL: Have they been tested yet?

MR. BOISJOLY: No, sir, there hasn't been one

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delivered.

MR. RUMMEL: When is a test scheduled?

MR. BOISJOLY: The first delivery was scheduled for February of 1987, and so we wouldn't be able to test until after the hardware was received.

DR. WALKER: I would like to ask a question about the putty.

Do you or does anyone know what the impact of temperature is on the putty and the formation of blow holes?

MR. THOMPSON: We have the formation of blow-holes on assembly. Since then, during the investigation which we of course didn't know at the time we were making this decision, we have tested putty in a putty test fixture somewhat similar to what the motor has, only instead of pressurizing from the outside, as we usually do in running our seal pressure test, we pressurize from the inside, and in some cases, in the worst on worst small clearance basis, it held pressure for about 3200 seconds.

MR. BOISJOLY: I should amplify, the night of the launch, the only data we had in our possession on the putty was that we had run temperature tests down to 40 degrees, and that was the characterization that

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determined the leak check pressure, to ensure that the leak check pressure was not masked by the putty, and we have held up to 100 psi with the putty, pressurized through the leak check port. And so we had temperature data at 40 degrees on the putty at that particular time, and we determined that 200 psi would be a conservative number that would bracket all of the testing that had been done, and we would have a leak check at 200 psi for 15 minutes with an open nitrogen gas source to ensure that the putty was not masking a leak in the O-ring, and we would drop to zero, repressurize to 50 psi, and run the actual low pressure leak check on the seal. Fifty psi is a very severe check on an O-ring seal at low pressure, holding that for ten minutes and allowing a one pound drop is a very, very small leak, on the order of an atomic energy type reactor leak.

DR. WALKER: There is one thing that disturbs me somewhat about the qualification tests in regard to the putty. As I understand it, during qualification tests, the putty was actually smoothed out or there were attempts made to eliminate any imperfections in the putty, but in fact, during an actual launch, that is not done. So in that sense, qualification tests did not truly simulate launch conditions.

Did that disturb anyone?

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MR. THOMPSON: Yes, sir, that is true. To assemble these motors under a horizontal condition, which we would do for the development motors and the qualification motors, the motors go out of round probably more so than they do during the normal stacking, vertical operation, and so the putty is probably moved more than it would be during normal stacking. And so probably more blow holes were generated, and you can see, we sometimes called them volcanoes in the putty.

DR. WALKER: You say probably? You don't really have any data?

Does anyone or has anyone actually photographed the condition of the putty or examined the putty when the vertical stacking is done, even early in the program?

I had heard that someone was lowered in a bosun's chair during the early phases of the program to examine the putty on a field joint.

Do you know whether that has ever occurred?

MR. THOMPSON: I don't have any detailed knowledge on that. We do know that as the tang is inserted into the clevis and the first and once the O-ring covers up on the tang, the sealing surface, that the residual air is compressed, and we know for a fact that

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sometimes that air that is compressed will cause a blow-by just on tang insertion.

GENERAL KUTYNA: Mr. Boisjoly, I wonder if we could go back to a broader question. You talked about data that you couldn't get, resources that you didn't have. Mr. Thompson has mentioned a letter that he put in in August that he got no response to. You wrote an activity report on the 4th of October that characterized some of this.

Could you summarize some of the things you said in that activity report, the key points, please?

MR. BOISJOLY: I have just got to check to make sure I know which one you are speaking of.

GENERAL KUTYNA: It is titled SRM Seal Problem Task Team Status, and it starts out with "The team generally has been experiencing trouble."

MR. BOISJOLY: Okay, I have that. That was written October 4.

Do you want me to read it?

GENERAL KUTYNA: Well, the pertinent points; I would like you to make those points, if you would.

MR. BOISJOLY: I felt, as I stated in my original memo before the team was formed, that the only way that we could pull off a timely resolution to this problem was to have people dedicated to this problem

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full time with full cooperation, and I felt that wasn't coming to pass. I felt very strongly that we were working towards a solution in a timely manner, but we were getting—

CHAIRMAN ROGERS: Could I interrupt just a second?

You might as well read it. It is a short memorandum. Dr. Keel will give it to you.

MR. BOISJOLY: This was an activity report that I turned in that said "SRM Seal Problem Task Team Status. The team generally has been experiencing trouble from the business-as-usual attitude from supporting organizations. Part of this is due to lack of understanding of how important this task team activity is, and the rest is due to pure operating procedure inertia which prevents timely results to a specific request.

"The team met with Joe Kilminster on 10/3/85 to discuss this problem. He wanted specific examples, which he was given, and he simply concluded that it was every team member's responsibility to flag problems that occurred to organizational supervision, and to work to remove the roadblock by getting the required support to solve the problem. The problem was further explained to require almost full time nursing of each task to ensure

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that it is taken to completion by a support group. Joe simply agreed and said we should then nurse every task we have.

"He plain doesn't understand that there are not enough people to do that kind of nursing of each task, but he doesn't seem to mind directing that the task nevertheless gets done. For example, the team just found out that when we submit a request to purchase an item, that it goes through approximately six to eight people before a purchase order is written and the item actually ordered. The vendors we are working with on seals and spacer rings have responded to our requests in a timely manner, yet we, MTI, cannot get a purchase order to them in a timely manner.

"Our lab has been waiting for a function generator since 9/25/85. The paperwork authorizing the purchase was finished by Engineering on 9/24/85 and placed into the system. We have yet to receive the requested item."

The reason I made an issue of that is because we had heard it was on the shelf down in San Diego, it was simply a matter of picking it up and using it.

"This type of example is typical and results in lost resources that have been planned to do the test work for us in a timely manner. I, for one, resent working at full capacity all week long and then being

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required to support activity on the weekend that could have been accomplished during the week. I might add that even NASA perceives that the team is being blocked in its engineering efforts to accomplish its tasks. NASA is sending an engineering representative to stay with us starting October 14. We feel that this is the direct result of their feeling that we, MTI, are not responding quickly enough on the seal problem.

"I should add that several of the team members requested that we be given a specific manufacturing engineer, a quality engineer, a safety engineer, and four to six technicians to allow us to do our task on a noninterference basis with the rest of the system. This request was deemed not necessary when Joe decided that the nursing of the task approach was directed.

"Finally, the basic problem boils down to the fact that all MTI problems have number one priority, and that upper management apparently feels that the SRM program is ours for sure, and the customer be damned."

And I signed it October 4, 1985. [Ref. 2/25-13]

CHAIRMAN ROGERS: May I turn the subject a little bit around now to another aspect of it? One of our responsibilities is to attempt to determine the cause or causes of the accident, and I guess both of you gentlemen have suggested in these memos that if certain

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things were not done, a catastrophe might result.

Looking at the pictures that we now have available to us, which show a puff of black smoke during the first, less than the first second of the launch, which was about 25 feet high, what is your opinion? Is that consistent with the concern that you had that a catastrophe might occur in that joint?

MR. BOISJOLY: We should back up and give you a little bit of background information first. It was everybody's considered opinion, including mine—and I am not a solid rocket motor man. I don't know anything about the propellants or anything like that, but it was everybody's considered opinion that if we developed a problem, that we would blow it up on the pad, and that we would never even lift off the pad.

There has been a lot of speculation about the puff of smoke. It hasn't really been determined what it is, where it came from. It for sure—I mean, there is a lot of speculation, but if that was a joint, then that supports the seal—excuse me, the leak seal leak theory, which is to say that you had a puff of smoke or a puff of products of combustion pass both seals at a very early stage in the ignition transient; then you sealed with whatever remaining material you had left in the seals, and you can postulate this, be it either the

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primary or the secondary, it would work both ways, and then at some time later, due to either going through a Max Q environment or vectoring of the nozzle, that you could—and vibration or aerodynamic forces, whatever you want to characterize, that there wasn't that much left, and it spit it out at that point in time and started to leak again.

That has not been really determined at this point that that——

CHAIRMAN ROGERS: I understand. I was just really asking whether the first puff of black smoke which has been described as about 25 feet high, is not inconsistent with the theory that you have been concerned about.

MR. BOISJOLY: That's right.

CHAIRMAN ROGERS: Mr. Thompson, do you agree with the comments that Mr. Boisjoly made earlier about the discussions in the telecon and other discussions that you were engaged in?

MR. THOMPSON: Yes, sir. I think I am prepared to talk about these things, but I think it has been mentioned, and most of the points that I have have been brought out.

CHAIRMAN ROGERS: Well, why don't you, if you would like to, just look over your notes and see if there's anything you want to add because we would be

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perfectly glad to have you go through the whole thing.

Maybe you can glance at them to see if you want to add anything.

MR. THOMPSON: Maybe I could just quickly summarize.

About 10:00 o'clock the morning of January 27 I received a phone call from Mr. Boyd Brinton. He is manager of project engineering, and he called me from Marshall and told me about the 18 degree temperature that was indicated that night, and this particular phone call was prompted by Mr. Larry Wear of MSFC, program manager, and indicating that they were both concerned about this. And they asked me how I felt and what my—if I felt concerned. And of course I told them that I did. And it went through a series of meetings up through engineering throughout the day, ending up finally in Mr. Lund's office, and we had concluded there that we would not launch if the temperature were anything lower than 53 degrees.

So at this point I felt fairly content, and we met back over in the engineering conference room, we call the MIC room, and prepared to make charts, and you have heard much of this conversation. And I guess the only thing I would like to add to it is I also heard that Marshall's reaction was that they were appalled at

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our recommendation, and I was very much surprised by this, having had the experiences over the years of how careful and conservative these people are, and I really admire them. And so I was extremely surprised and really not ready for this type of a recommendation.

And so I guess we had put together some things, probably we may have put together some more if we had any anticipation that our recommendations would not be accepted. But I think we pretty well got everything out on the table that we had in mind, and they indicated that the seal was not a function of temperature, and being a visco-elastic seal, it had to be a function of temperature, in my thinking, although I was unable to explain the SRM-22, where we got some soot there also, in a supposedly warm motor.

And during the caucus I, as Mr. Boisjoly has mentioned, and something that I am not usually accustomed to doing, sitting down between two managers and trying one more time to explain and make sure that I got my thoughts across to them. And after not completing that conversation but concluding that I probably wasn't being—I wasn't communicating, and that may be my fault, I am happy, not happy, but I readily admit that it may have been my type of explanation, I wasn't getting through.

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I think that—just one last point. I think that we had seen erosion on SRM-15, erosion, but more important, we had seen blow-through, and it was fairly severe. We had blow-through on two field joints, and in fact, two nozzle joints also, we saw soot between the O-rings in four places that I recall, and that was 53 degrees, and it was my judgment that we had no reason to take the risk of shooting at an O-ring temperature that was lower than 53 degrees, and my recommendation in some of the former meetings was to just at least wait until the afternoon, and then, depending upon temperatures, perhaps wait another day. Ands I think that concludes my remarks.

CHAIRMAN ROGERS: Can you remember any time in your experience at Thiokol when the Engineering Group has been overruled in effect by management?

MR. THOMPSON: Nothing that comes immediately to my mind, sir.

CHAIRMAN ROGERS: Thank you.

MR. ACHESON: Mr. Boisjoly, the activity report of October 4, 1985, to whom was that directed?

MR. BOISJOLY: I turned that in to my immediate supervision for incorporation to go up through the ladder as it reports on a weekly basis.

MR. ACHESON: Who did you give it to, what

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individual?

MR. BOISJOLY: My supervisor is Jack Kapp. I turned it in to his secretary.

MR. ACHESON: Were you given an opportunity to discuss it at any subsequent time with anybody in management?

MR. BOISJOLY: No, sir.

MR. ACHESON: Thank you.

DR. RIDE: Just to be perfectly clear about this, is it fair to say that as engineers you don't believe that you had data or analysis to characterize the performance of the joint at a 30 degree temperature?

MR. BOISJOLY: That is correct.

CHAIRMAN ROGERS: Is that true of you, Mr. Thompson?

MR. THOMPSON: Yes, sir.

CHAIRMAN ROGERS: Okay.

Thank you very much.

Now we would ask Mr. Lund and Mr. Kilminster and Brian Russell to come up.

(Witnesses sworn.)

CHAIRMAN ROGERS: Mr. Lund, do you want to proceed and make any remarks or give any testimony you would care to, particularly in light of the testimony we

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have had this morning and this afternoon?

MORTON THIOKOL INC.

Wasatch Division

Interoffice Memo

31 July 1985
2870:FY86:073

TO: R. K. Lund
Vice President, Engineering

CC: B. C. Brinton, A. J. McDonald, L. H. Sayer, J. R. Kapp

FROM: R. M. Boisjoly
Applied Mechanics - Ext. 3525

SUBJECT: SRM O-Ring Erosion/Potential Failure Criticality

This letter is written to insure that management is fully aware of the seriousness of the current O-Ring erosion problem in the SRM joints from an engineering standpoint.

The mistakenly accepted position on the joint problem was to fly without fear of failure and to run a series of design evaluations which would ultimately lead to a solution or at least a significant reduction of the erosion problem. This position is now drastically changed as a result of the SRM 16A nozzle joint erosion which eroded a secondary O-Ring with the primary O-Ring never sealing.

If the same scenario should occur in a field joint (and it could), then it is a jump ball as to the success or failure of the joint because the secondary O-Ring cannot respond to the clevis opening rate and may not be capable of pressurization. The result would be a catastrophe of the highest order - loss of human life.

An unofficial team (a memo defining the team and its purpose was never published) with leader was formed on 19 July 1985 and was tasked with solving the problem for both the short and long term. This unofficial team is essentially nonexistent at this time. In my opinion, the team must be officially given the responsibility and the authority to execute the work that needs to be done on a non-interference basis (full time assignment until completed).

[Ref. 2/25-11 1 of 2]

R. K. Lund - 2 - 31 July 1985

It is my honest and very real fear that if we do not take immediate action to dedicate a team to solve the problem, with the field joint having the number one priority, then we stand in jeopardy of losing a flight along with all the launch pad facilities.

Roger M Boisjoly
R. M. Boisjoly

Concurred by:

Jack R Kapp
J. R. Kapp, Manager
Applied Mechanics

[Ref. 2/25-11 2 of 2]

MORTON THIOKOL INC.

Wasatch Division

Interoffice Memo

2871:FY86:141
22 August 1985

TO: S.R. Stein,
Project Engineer

CC: J.R. Kapp, K.M. Sperry, B.G. Russell, R.V. Ebeling, H.H. McIntosh, R.M. Boisjoly, M. Salita D.M. Ketner

FROM: A.R. Thompson, Supervisor
Structures Design

SUBJECT: SRM Flight Seal Recommendation

The O-ring seal problem has lately become acute. Solutions, both long and short term are being sought, in the mean time flights are continuing. It is my recommendation that a near term solution be incorporated for flights following STS-27 which is currently scheduled for 24 August 1985. The near term solution uses the maximum possible shim thickness and a .292 +.005/-.003 inch dia O-ring. The results of these two changes are shown in Table 1. A great deal of effort will be required to incorporate these changes. However, as shown in the Table the O-ring squeeze is nearly doubled for the example (STS-27A). A best effort should be made to include a max shim kit and the .292 dia O-ring as soon as is practical. Much of the initial blow-by during O-ring sealing is controlled by O-ring squeeze. Also more sacrificial O-ring material is available to protect the sealed portion of the O-ring. The added cross-sectional area of the .292 dia O-ring will help the resilience response by added pressure from the groove side wall.

Several long term solutions look good; but, several years are required to incorporate some of them. The simple short term measures should be taken to reduce flight risks.

A.R. Thompson

ART/jh

[Ref. 2/25-12]

ACTIVITY REPORT

SRM Seal Problem Task Team Status

The team generally has been experiencing trouble from the business as usual attitude from supporting organizations. Part of this is due to lack of understanding of how important this task team activity is and the rest is due to pure operating procedure inertia which prevents timely results to a specific request.

The team met with Joe Kilminster on 10/3/85 to discuss this problem. He wanted specific examples which he was given and he simply concluded that it was every team members responsibility to flag problems that occurred to organizational supervision and work to remove the road block by getting the required support to solve the problem. The problem was further explained to require almost full time nursing of each task to insure it is taken to completion by a support group. Joe simply agreed and said we should then nurse every task we have.

He plain doesn't understand that there are not enough people to do that kind of nursing of each task, but he doesn't seem to mind directing that the task never-the-less gets done. For example, the team just found out that when we submit a request to purchase an item, that it goes through approximately 6 to 8 people before a purchase order is written and the item actually ordered.

The vendors we are working with on seals and spacer rings have responded to our requests in a timely manner yet we (MTI) cannot get a purchase order to them in a timely manner. Our lab has been waiting for a function generator since 9-25-85. The paperwork authorizing the purchase was finished by engineering on 9-24-85 and placed into the system. We have yet to receive the requested item. This type of

[Ref. 2/25-13 1 of 2]

example is typical and results in lost resources that had been planned to do test work for us in a timely manner.

I for one resent working at full capacity all week long and then being required to support activity on the weekend that could have been accomplished during the week. I might add that even NASA perceives that the team is being blocked in its engineering efforts to accomplish its tasks. NASA is sending an engineering representative to stay with us starting Oct 14th. We feel that this is the direct result of their feeling that we (MTI) are not responding quickly enough on the seal problem.

I should add that several of the team members requested that we be given a specific manufacturing engineer, quality engineer, safety engineer and 4 to 6 technicians to allow us to do our tests on a non-interference basis with the rest of the system. This request was deemed not necessary when Joe decided that the nursing of the task approach was directed.

Finally, the basic problem boils down to the fact that ALL MTI problems have #1 priority and that upper management apparently feels that the SRM program is ours for sure and the customer be damned.

Roger M Boisjoly

Roger Boisjoly 10/4/85

[Ref. 2/25-13 2 of 2]

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TESTIMONY OF ROBERT LUND, VICE PRESIDENT, ENGINEERING, THIOKOL; JOE KILMINSTER, VICE PRESIDENT, SHUTTLE PROJECT, HUNTSVILLE; AND BRIAN RUSSELL

MR. LUND: I'm not sure there's anything. We've been over so many things; if there are additional questions, I would certainly be happy to talk about that.

CHAIRMAN ROGERS: How do you explain the fact that you seemed to change your mind when you changed your hat?

MR. LUND: I guess we have got to go back a little further in the conversations than that. We have dealt with Marshall for a long time and have always been in the position of defending our position to make sure that we were ready to fly, and I guess I didn't realize until after that meeting and after several days that we had absolutely changed our position from what we had been before. But that evening I guess I had never had those kinds of things come from the people at Marshall that we had to prove to them that we weren't ready.

CHAIRMAN ROGERS: Do you want to move your mike a little bit closer, please?

MR. LUND: And so we got ourselves in the

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thought process that we were trying to find some way to prove to them it wouldn't work, and we were unable to do that. We couldn't prove absolutely that that motor wouldn't work.

CHAIRMAN ROGERS: In other words, you honestly believed that you had a duty to prove that it would not work?

MR. LUND: Well, that is kind of the mode we got ourselves into that evening. It seems like we have always been in the opposite mode. I should have detected that, but I did not, but the roles kind of switched, and so after making, or listening to the verbal presentation in the afternoon, they asked what Thiokol's position was, and I looked around the room, and I was the senior person, and I said I don't want to fly. It looks to me like the story says 53 degrees is about it.

And of course, we were requested then to go back and do something more and prepare detailed charts to show that in more detail. And so we got busy then, and I gave assignments to a dozen or so people to go out and generate data that would in a workmanlike manner show the rationale and show the data that we had so that everyone would understand all the data and where it came from.

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And we spent the next couple of hours beating the bushes trying to put together that data. And so we did that and began transmitting charts even late then, and then went through that rationale. And all of this time we were preparing the data, the data was coming in, and I was

trying to put together what I was concluding out of all this because there was some additional data that was being generated, and trying to understand and to absorb all of the data that was there, to again see what my thought processes were.

And so we stood at the white board there in the Management Information Center and as the data would come in we would try and say, now, what does this tell us, what does this tell us, and put together a rationale for what we wanted to do.

So it was in a real time mode, and we were trying to absorb the data and put together the story.

Well, as a result of that telecon, I gave the charts that made the recommendation that we wait until the motor got to 53 degrees. I didn't see anything different that I hadn't seen before. And of course, you have heard the story of what happened after that.

VICE CHAIRMAN ARMSTRONG: Let me ask a question about that 53 degrees.

You stated that you—it has been often

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stated that the recommendation was at this point in time to stay within your experience base, but I find that to be a peculiar recommendation in the operation of any kind of system because normally you say from our experience base, our data points, and our analysis, and our extrapolation, we would be willing at any time to go beyond our experience point out this far as a next step, and the only reason you would say "I would stay within my experience base" is that you had a problem at that point that said you dare not go any farther.

So could you clarify why you said that?

MR. LUND: It wasn't a question of that. It was a very definite question of conservatism. You know this program has people on it, and so I am very concerned about that, and I want to make sure that if there is any hint of a problem, that we are not extending that. And we didn't have any data at that point that would indicate that we should go beyond that.

Is that an answer to the question or do you want me to try again?

DR. RIDE: Maybe a slightly different way of asking that is normally, when you are trying to extrapolate beyond, maybe beyond your flight experience, you rely on your qualification testing program, and a

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system or a subsystem is qualified to fly within a certain regime or a certain envelope. That would include environmental effects like temperature.

Did you have any sort of qualification range on the temperature of the SRM?

MR. LUND: Yes, we had development qual motors that were down in the 40s, as has been pointed out. The data, because of the horizontal assembly problems, what we were trying to do is to put them back to a condition that would resemble that that we would have in a vertical installation. And so we had gone in and repaired the putty, because when you put those together horizontally you can't do it the same way. You can't do it the same way.

And so although the intent was to put them back to the vertical, there is some doubt that you can really do that adequately. And so there is always the question, well, was it perfect? And the answer was, it was not.

And so my belief is that those 40 degree motors are probably valid and adequate, but there is the doubt.

DR. RIDE: So going down below, say, 47 degrees or 40 degrees, you could consider it, perhaps taking it below what the motor had been qualified to?

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MR. LUND: Correct. I think one thing that we need to make very clear is, as we have talked about SRM-15 and SRM-22 being the ones that were the blow-by motors, and that we couldn't tell because of those, but we have got to keep in mind there were ten motors between those, 30 joints between those two temperatures that had no blow-by at all.

And so I think that as Marshall pointed out, I think Mr. Mulloy pointed out, he said, you know, the data is just not conclusive at all, and it wasn't because we had a low temperature motor and a high temperature motor, and we had ten motors in between that showed nothing.

MR. SUTTER: Could I ask a question on that? In looking at the earlier data it appears there is erosion and blow-by occurring more frequently later than sooner, and Mr. Boisjoly pointed out that during the year 1985, because of what he learned in looking at, inspecting that one motor that he looked at, he developed greater concern, and in line with Neil's question, did you want to stay with 53 rather than, say, exploring something slightly beyond that, because there was a greater concern developing, and was this concern—did you share Mr. Boisjoly's concern? Just why did you stay with 53 and not say explore a little

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further?

MR. LUND: Well, again, the conservative aspect of looking at what our experience has been, and that was the rationale that we presented to Marshall, is stay within the experience band, don't extend it.

MR. SUTTER: Well, how did you view—you mentioned ten motors without blow-by. In looking at the data that was handed out this morning, there are several cases of problems with various parts of the motors, and it seems to be more of it later rather than earlier.

MR. LUND: We haven't been able to identify those parameters that are causing that more pronounced effect.

MR. SUTTER: But if you look at that, since you had a problem that was developing to a greater extent, and it was an unknown reason as to why this problem would happen, I can't understand why there wasn't a greater area of concern developing.

MR. LUND: Well, there was, and that is why we had initiated all of this. What are we going to do with this joint, how are we going to investigate it, set up a special task team. I took one of my best supervisors and gave him the task of doing that. We assigned Mr. Boisjoly to the task team, and we put one of our best project engineers on it, and a program manager was assigned. We

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implemented a full time activity to evaluate and do that.

MR. SUTTER: Well, one more question.

Since your people were responsible for the design and were responsible for the testing and were responsible for the qualification, when it came up to the point where there was a question of whether a launch should be made, shouldn't you alone or your organization alone be the one that says yes, or not? And why would there be a question coming from NASA because of not having data as well presented as they wanted? Why would they raise questions on your data, and why would you respond to that question? Why didn't you just tell them it's our decision, and this is it, and not respond to the pressure?

MR. LUND: As a quarterback on Monday morning, that is probably what I should have done, but you know, you work with people and you develop some confidence, and I have some

great confidence in those people at NASA we worked with. We have worked with them for a long, long time.

MR. SUTTER: But in what I think I have heard is that your experts were developing a greater and greater concern, including writing rather powerfully stated memos, and it is hard to understand why they

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didn't get more attention.

VICE CHAIRMAN ARMSTRONG: I would like to continue that line of thought in response to your earlier answer about the ten motors between the 53 degrees and the 75 degrees. One conclusion might be from that data base is that you shouldn't operate at 75 degrees or 53 degrees, but you ought to stay within the temperature range of those motors which exhibited no erosion or blow-by.

So again, I am going to ask you the same question I asked before: what was conservative about saying 53 instead of a number other than that which might be higher or might be lower, depending upon how you approach the problem?

I am trying to understand your thought process.

MR. LUND: I guess all the engineering rationale said that cold was probably worse and warm was probably better, and all the physics of the situation, as we best understood it. And so we felt that, you know, the conservative thing to do would be to stay warmer.

VICE CHAIRMAN ARMSTRONG: But did you like 53 degrees? Did you like the results on the 53 degree flight?

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MR. LUND: I would rather have a motor that has no erosion and no blow-by and looks perfect at the end of the motor firing.

VICE CHAIRMAN ARMSTRONG: Then might not you have taken the position that 57 would have been a number, or 62 or some other number?

MR. LUND: We have had motors down in the low 60s, and I guess there are some, just looking at my chart, in the low 60s at least that were perfect, and we have had some in the lower 50s that were not anywhere near as bad as SRM-15. So there is no clear dividing line, at least.

VICE CHAIRMAN ARMSTRONG: There's a lot of other factors other than temperature that might be involved, is that correct?

MR. LUND: That is correct. So we don't know what the effect of temperature was.

MR. SUTTER: Can I ask just one more question?

I heard these comments that some tests looked pretty good at 75 and others looked okay at 53, but was the qualification testing thorough enough for any proper data base to be in hand to really know what the critical variables are like the thrust of the motor, the tolerances of the metal joints, the tolerances of the

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O-rings, or the application of the putty or the application of the seal?

Isn't one of your problems the fact that the qualification testing was inadequate?

MR. LUND: It seems like the size of the qualification program is inversely proportional to the size of the motor, which is called dollars, and there's no doubt about that. With small tactical motors it is not unusual to fire many, many, many, and the larger the motor, the smaller the qualification program, and to get a full statistical range of every parameter of that motor, I don't think there's enough money in the national treasury to do that.

So there is a practical limit to what you can do.

MR. SUTTER: Well, can't you go from the partially qualified motors and make changes and go into another area of further unknown exploration like reducing the strength of the motor or increasing the thrust or changing the putty?

MR. LUND: There were qualification motors, of course, to do the strength or the thickness of the case and the thrust change.

MR. SUTTER: Well, were they adequate?

MR. LUND: In engineering opinion, yes.

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DR. WALKER: I have a question on temperature.

Mr. McDonald testified that he had a discussion, I believe it was with Mr. Mulloy, about the meaning of the temperature range of 40 to 90 degrees, whether it would apply just to the bulk temperature of the motor or whether it would apply to every part of the Shuttle.

What is your understanding?

MR. LUND: If you would have asked me a month ago, I would have told you the motor is 40 to 90.

DR. WALKER: And so there is no qualification in your mind on the O-ring temperature? For example, the O-ring temperature could have been quite cold because the O-ring is certainly going to move in temperature with the metal case. The metal case could be quite a bit colder than the bulk temperature of the propellant if the weather had been cold.

MR. LUND: There were no full scale motors fired below 40.

DR. WALKER: But what I am asking is did you as the supplier of the system to the government have a specification on the O-ring temperature, or could the O-ring temperature have been anything?

MR. LUND: I don't believe there is a

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specification on the O-ring temperature other than the material itself.

DR. WALKER: And what is that specification?

MR. LUND: Well, it is a specification that says the material can withstand these kinds of environments.

DR. WALKER: But it doesn't specify the operational aspect, does it?

MR. LUND: To my knowledge, no.

DR. WALKER: In fact, the Milspec says specifically that the O-rings should be qualified for whatever uses they are put to.

So did you take any steps to do that, that is, qualify the range of temperatures over which these O-rings were to be used?

MR. LUND: Yes. The development motors were fired from 40 to 84, and the qualification motors from 45 to 83.

DR. WALKER: So then 40 degrees was the temperature limit for the O-ring?

MR. LUND: In the full scale qual program, that is correct.

DR. WALKER: So when it was predicted that the temperature of the O-ring at launch of 51-L was going to be 29 degrees, the O-ring was outside of the

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qualification temperatures by some 10 degrees.

MR. LUND: That is correct.

DR. WALKER: Then how could you make a recommendation to launch if you were 10 degrees outside of your qualification?

MR. LUND: Our original recommendation, of course, was not to launch.

DR. WALKER: Well, I understand that, but your final recommendation was to launch.

MR. LUND: Okay. What we need to do, then, is go through that rationale.

DR. WALKER: So you are going to answer my question then at the end of this discussion, hopefully?

MR. LUND: If you want me to go through it now, I would be glad to do that.

CHAIRMAN ROGERS: Well, I think we have heard what explanation you have given. I think the problem we are having, it is not convincing. I mean, let me, if you don't mind, I assume you have great confidence in your engineers Boisjoly and Thompson and the others, and they are probably as well qualified as anybody in the country in dealing with these problems of O-rings and seals and so forth, is that right?

MR. LUND: Yes.

CHAIRMAN ROGERS: And you had a long

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discussion in the telecon, and you decided, all of you, I understand, all of you decided that for safety reasons you would oppose the launch. And thereafter, NASA, in one way or another, made it clear that they were displeased with that recommendation, and I assume that you knew when you made the recommendation that your recommendation in fact was going to determine whether that Shuttle would be launched or not because NASA had indicated to you that they would not fly unless they had a written report from Thiokol saying you approved the launch.

MR. LUND: I didn't know NASA would accept that.

CHAIRMAN ROGERS: You didn't know that?

MR. LUND: No.

CHAIRMAN ROGERS: Well, you must have known your recommendation was very important. You knew that if you voted against the launch it would not have been launched, didn't you?

MR. LUND: Well, we had voted prior to it, and they didn't accept it, so I couldn't forecast what NASA would do.

CHAIRMAN ROGERS: But you knew that that was the reason they asked you to reconsider. That is why you had the five minute recess, didn't you?

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MR. LUND: That's a fair statement, yes.

CHAIRMAN ROGERS: Now, knowing that, and knowing that the safety of the crew was involved, and knowing your own people, the engineers that you respected, were still against the launch, what was it that occurred in your mind that satisfied you to say okay, let's take a chance?

MR. LUND: Well, I didn't say take a chance because I felt that there was some rationale that allowed us to go ahead.

CHAIRMAN ROGERS: Well, maybe that isn't fair. Then what was it that occurred in your mind that caused you to be willing to change your mind?

MR. LUND: I guess one of the big things was that we really didn't know whether temperature was the driver or not. We couldn't tell. We had hot motors that blew by and cold motors that blew by, and some very near either end that did not. The data was inconclusive, and so I had trouble justifying in my own mind and saying, by golly, temperature is a factor.

Second——

DR. WALKER: May I interrupt for just a moment?

Mr. Boisjoly has said that the thing which was compelling to him was that the blow-by on the coldest

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motor was much more severe. He has emphasized how black was the blow-by and how large an angle over which it occurred.

MR. LUND: But three or four degrees above that was zero.

DR. WALKER: So his discussion was not convincing to you?

MR. LUND: Well, it wasn't totally convincing because in two or three degrees it went from very, very bad to no problem, no blow-by.

MR. FEYNMAN: There were many seals that didn't have any problem, and so it is obviously a random effect. It depends upon whether or not you get a blow hole or you don't get a blow hole. So if within a particular flight it happens that all six seals don't get a blow hole, that's no information. The fact that—as far as I can understand this, it doesn't mean you are suddenly good because it worked and the next time when one goes off it's suddenly bad. It seems to me that it has to be understood as a probabilistic and confusing, complicated situation.

So you could never decide they are all going to break or they are all not going to break it's not the wonderful one-horse shay, and when you look at it that way, it is a question of increasing and decreasing probabilities that we have to consider rather than did it work or didn't it work.

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And I would like you to explain it to me from that point of view.

MR. LUND: Well, the question still is, is temperature the thing that caused the SRM-15 effect?

MR. FEYNMAN: You have heard your engineers argue that there was an effect of temperature which looked like it made things worse. Is it not inconceivable that there is something else that sometimes produces blow-by, that there is more than one effect, and that temperature could still be an important effect and increase the probability in spite of the fact that at a high temperature it gets worse?

Is there any evidence that temperature is not an important matter just because at some high temperature you have an accident?

MR. LUND: Well, there is, as you point out, there are many variables in the thing, and it wasn't clear that temperature was the effect.

MR. FEYNMAN: But logically, from the point of view of the engineers, they were explaining why the temperature would have an effect, and when you don't have any data, you have to use reason, and they were giving reasons.

MR. LUND: That's right, and that is what we did as we included in our rationale the fact that sure

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enough, the temperature could be an effect.

Okay, it wasn't clear, but we said we will consider that to be so.

MR. RUMMEL: I have great difficulty with this. In the usual practice, when there is any real doubt about flight safety, whether it is aircraft or whatever, you simply don't fly, and it seems to me this is the reverse, and I just have great difficulty understanding the question that has been asked before, that is, understanding any answer. I just haven't heard it as to why, if there is doubt in your mind, why you went ahead, why you changed your mind. I just don't understand it, and I would appreciate very much if you could explain that.

CHAIRMAN ROGERS: Maybe we ought to go to Mr. Kilminster, if you don't mind. He signed the Telefax, and we have asked Mr. Lund a lot of questions, and I think maybe in fairness to Mr. Lund, Mr. Kilminster, could you attempt to explain to the Commission why—I guess you changed your mind, too, didn't you?

MR. KILMINSTER: Yes, sir, based upon the discussion that we had and the rationale that was developed.

CHAIRMAN ROGERS: How can you say you changed

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your mind when you say temperature and data not conclusive on predicting primary O-ring blow-by?

Did you have a feeling that you had to prove that it was—that the burden of proof was on you to show that it wasn't safe?

MR. KILMINSTER: No. I think we were asked to relook at the data, which we did.

CHAIRMAN ROGERS: Could you tell me what data it was that you looked at that was different from the data you had looked at first?

MR. KILMINSTER: There was one piece of data that we looked at which has been discussed before, and that was the erosion parameter, and the factor of three, but my evaluation was—

CHAIRMAN ROGERS: Is that the only new piece of data?

MR. KILMINSTER: That is really the only new piece of data that we had not previously discussed on the telephone, but I think that the data that we did show, the fact that we had subscale tests at 30 degrees showing no blow-by was an indicator. We, as Mr. Lund pointed out, had other flight motors at temperatures between the two in question, that is, 75 degrees and 53 degrees O-ring temperatures that had shown no blow-by, and we had static test motors at temperatures lower than

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53 degrees that had no blow-by.

Now, I would like to discuss that just a moment.

CHAIRMAN ROGERS: Could you particularly, though, point out the new data because the purpose, as we understand it, for the recess was to find out if there was any new data, or were you just asked to change your mind based upon the data you had?

MR. KILMINSTER: I had one piece of data that was new in our discussion was that if we did have blow-by past the primary O-ring, as it was being positioned to seal, and recognizing the fact that the cold temperatures could cause that timing function to extend, then we had an opportunity perhaps of having some erosion occur. We looked at the erosion that had occurred on 51-C, and compared that to the data that we had developed, both from cold hydraulic oil testing and from hot subscale testing, that indicated that that very flight had a safety factor of three over what would have to have happened in order to get to an area of questionable sealing capability.

With that information in mind, then, and also the fact that we had had some analytical work done earlier to develop the limiting erosion parameters on O-rings, led me to believe that we were in the condition

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of having a safe position for recommending a flight.

CHAIRMAN ROGERS: Excuse me. How come that data wasn't available the first time? How did it happen to show up at the recess?

MR. KILMINSTER: Well, it was just a matter of a discussion that we had while we had the recess. When we said that well, if indeed the temperature is going to cause a longer time for the primary O-ring to function, and there is the possibility of additional erosion, then where do we stand relative to 51-C, and how much more margin did that exhibit?

CHAIRMAN ROGERS: So you really had the data there before?

MR. KILMINSTER: Yes.

CHAIRMAN ROGERS: You hadn't analyzed it. In other words, it was not new data. It was data that you hadn't properly analyzed, is that right?

MR. KILMINSTER: It was data that we hadn't prepared or discussed on the earlier telecon or the earlier portion of the telecon.

CHAIRMAN ROGERS: And that was through an oversight?

MR. KILMINSTER: Well, I don't know if it was oversight or just another piece of data that we were searching for in order to establish our position.

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DR. RIDE: You are saying that you had a safety factor of three over the 51-C erosion problem. How were you attempting to analyze how the timing function would change and how much erosion that could possibly give you on 51-L?

MR. KILMINSTER: Only on a subjective basis that if, as has been stated, the timing function under cold condition was to extend, and if what was observed under 51-C, not only the blow-by but the erosion, were combined, then we developed the rationale about that safety factor of three.

CHAIRMAN ROGERS: Mr. Kilminster, did you have any feeling of pressure being put on you by NASA, or were you just calmly reassessing?

MR. KILMINSTER: I felt that the pressure that was put on us was to go back and look at the data, look at the detailed information that had been presented to see if there was something that we were seeing that we were not representing on the phone.

CHAIRMAN ROGERS: You didn't feel they were trying to get you to change your mind?

MR. KILMINSTER: I did not feel a significant amount of pressure in that regard.

MR. ACHESON: Mr. Kilminster, have you had an opportunity to look at the correspondence of Mr.

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Boisjoly written in the summer and autumn of 1985?

MR. KILMINSTER: No, I have not.

DR. WALKER: Mr. Boisjoly's memo to you of 31 July, you received that and read it, I presume.

MR. LUND: Yes, and we took immediate action on that and established the O-ring task team within a week or so of that memo.

DR. WALKER: Did you pass along Mr. Boisjoly's concerns to anyone at NASA?

MR. LUND: The normal process of that sort of information is through the project office and Mr. McDonald and so on were on that. That would be their task.

DR. WALKER: Well, then, you are saying that it would have been Mr. McDonald's responsibility to alert NASA that a key engineer thought a catastrophe was possible?

MR. LUND: I think NASA understood that clearly, and we have been working with them since that time on this whole problem.

DR. WALKER: I am not asking that. I am asking whether as a result of Mr. Boisjoly's memo to you, you or Mr. McDonald or anyone else at Thiokol alerted NASA to the concerns of this key engineer.

MR. LUND: I did not call anyone at NASA

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specifically but I saw that they were alerted because of their immediate response in working with us.

DR. WALKER: But are you aware of their having heard that a potential catastrophe was possible?

MR. LUND: I am not aware.

DR. COVERT: Mr. Kilminster, I would like to ask a couple of questions about the temperature, if I may, a sequence of questions.

You say that on this 40 to 90 degree launch or certification criteria, is this a cold soak, or is this some other kind of a temperature?

MR. KILMINSTER: The 40 to 90 degrees is a mean bulk temperature of the propellant, which means to me that we could have a motor where the propellant grain was soaked out to 40 degrees, or conversely, propellant grain was soaked out to 90 degrees.

DR. COVERT: What is the time constant on this case plus insulator and so forth to get to an equilibrium bulk temperature?

MR. KILMINSTER: We ran some calculations based upon equilibrium at the Cape, and I believe it is about 30 days.

DR. COVERT: So that it is possible to be exposed to a cold temperature and have sharp temperature gradients within the propellant or the insulator and so

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part of it would be at a temperature far outside of what you think would be appropriate and the rest of it would still be warmer, shall we say?

MR. KILMINSTER: Yes, but I think that is limited by the other requirement which was spelled out in the Level II document that said 31 degrees to 99 degrees.

DR. COVERT: Is it possible that some of it could be at 26 degrees and other at 55 degrees, considering again a 21 to 30 day transient to get to the bulk temperature?

MR. KILMINSTER: That is not the way that I would read the combined two specifications or two requirements.

DR. COVERT: Is it possible that there is a difference between the behavior of the subscale motors at some temperature and the behavior of the large motors at the same temperature if one was in thermal equilibrium and the other was in some sort of a nonuniform temperature distribution?

MR. KILMINSTER: Yes, I would expect there could be some differences.

DR. COVERT: So there may be some doubt in your mind as to how exactly to apply the subscale motor data within the framework of the large scale motor if

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you allow for the temperature transient?

MR. KILMINSTER: Yes, sir.

DR. COVERT: Thank you.

MR. ACHESON: I have a question for Mr. Lund or Mr. Russell.

When I was talking with you a few days ago at your plant, I think I am correct in recalling that one of you told me that if the temperature at the Cape on the day of launch had been in the 9 to 10 degrees above zero Fahrenheit range, there would be no question but that you would have stuck to a no-launch position.

Am I right in that recollection?

MR. LUND: That is right. There's many, many things in the motor then that we would be concerned about.

MR. ACHESON: So you did recognize that temperature was a discriminator in an absolute sense. The only question was what the margin was.

MR. LUND: That's right.

MR. ACHESON: Below 53 degrees, is that correct?

MR. LUND: Yes.

MR. ACHESON: Were you acquainted at all with this correspondence which I assume you have had a chance to read, of Mr. Boisjoly, written in the summer and autumn of '85? It seems to say that management does not

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have an adequately heightened sense of the urgency of the work to be done on the O-ring problem, the seal problem.

My question is were you aware of that feeling on his part at the time? Did it cause you concern? And what if anything did you do about it?

MR. LUND: I didn't meet with Mr. Boisjoly, I met with Don Ketner, who is the task team leader. He made recommendations to us, and I think we followed those recommendations, and did everything we could to help him with them.

Now, undoubtedly there are things that were bugging them that they didn't bring to my attention, that I didn't do anything about because I didn't know about them, but when they came, and Don Ketner, particularly, we worked very hard with him to try and establish and take corrective action as needed.

MR. ACHESON: Thank you.

VICE CHAIRMAN ARMSTRONG: I am sure that all of you and everybody associated with the Shuttle knew that it was NASA's intention to fly on a very rapid time schedule throughout the year, or build up to the point of that, and that they would have to launch on winter mornings and at other times.

In the process of evaluating the information

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brought to you, did you at any time suggest to NASA that the launch commit criteria, the LCCs or other rules, may be insufficiently constraining, and that you ought to establish rules with respect to the SRM that kept you, one, either within your experience base, your qual test, or your extrapolations?

MR. LUND: We did not. Mr. McDonald evidently had some discussions with them along that line.

VICE CHAIRMAN ARMSTRONG: Thank you.

MR. ACHESON: Mr. Russell, I have a question for Mr. Russell.

Perhaps you would give us your recollection of the thought process followed in your mind in the change of position between the view presented in the telecon that Thiokol was opposed to a launch, and the subsequent conclusion of the caucus within the company.

MR. RUSSELL: I was part of the overall day's efforts that started off pretty early, as has been mentioned, and as the day developed, pretty much agreed with the recommendations, and as our presentation was made on the charts, the subsequent answers that came back, you have asked others if they felt pressure and undue pressure, and I felt some pressure with some of the comments. I thought our rationale was reasonable. I have seen many instances in the past where we have

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used previous limits for data on putty that I am familiar with and other raw materials, for example, that we do not exceed our experience base in the usage of these materials. And so I felt that this was the proper thing to do, albeit our 53 degree experience, as has been mentioned, wasn't desirable, but we had deemed it acceptable at that time.

And the statements that seemed to me to put pressure or at least to make me feel the pressure were the responses of being appalled at our recommendations, the responses of when we should launch, how long do we have to wait, April? And also there was argument about what sort of rationale we could have for 53 degrees, and Mr. Armstrong made the point that what temperature could we make a strong statement on? And the data couldn't tell us which temperature other than 53 degrees, which had been our previously most successful low temperature. And so I felt some pressure based upon those comments.

I would like to add that my feeling as I believe it was Mr. Mulloy—and this is, again, by voice recognition, but after we had made our recommendation initially, he talked for about five minutes and explained in his mind why the data really didn't add up to the conclusions that we had made. And

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I will have to admit at that time that it was a very logical presentation that he had made, and then there was talk back and forth as to what the data really meant, and as has been mentioned here, in the caucus there weren't any new data brought up that I recognized. We pretty much talked over the same types of things, and my feeling of it was that what some of us were understanding and what others were understanding and feeling important didn't really agree.

I didn't make—I think I can only recall making one comment in the caucus. I can't even remember what that was, and it was very brief, and I don't think it made a big impact at all. And the reason that I didn't make so many comments, despite what I felt, was that Roger and Arnie were making the points to the best they could, and I couldn't see a way of making them any better.

So we got to a point in the caucus where Mr. Mason said, and rightfully so, we are covering the same information, we are not talking about anything new here, and it's time for a decision.

And I maybe tend in my position to feel more pressure. I know these other gentlemen have many pressures from all sides, and I can't really speak for them, but I felt pressure that we were—and I don't

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know if Roger was referring to me in his testimony, but he could have been because I had the feeling that we were—that it was a distinct feeling that we were in the position of having to prove that it was unsafe instead of the other way around, which was a totally new experience. And I believe I made that statement afterward. I know I did when I went home and talked about it with my wife.

But here again, I think it's impossible for me to say what might have swung the decision. But I felt in my mind that once we had done our very best to explain why we were concerned, and we meaning those in the camp who really felt strongly about the recommendation of 53 degrees, the decision was to be made, and a poll was then taken. And I remember distinctly at the time whether I would have the courage, if asked, and I thought I might be, what I would do and whether I would be alone. I didn't think I would be alone, but I was wondering if I would have the courage, I remember that distinctly, to stand up and say no.

However, we were not asked as the engineering people. It was a management decision at the vice president's level, and they had heard all that they could hear, and I felt there was nothing more to say, that we could change anything, and also felt in my mind

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that I didn't see—and I believe I mentioned this to you last week—that I didn't see a dangerous concern. I knew we were entering into increased risk, and I didn't feel comfortable doing that.

I was nervous. You asked how I slept that night, and I said I thought I slept okay, and my wife differed with that as I talked with her after that. But there was a nervousness there that we were increasing the risk, and I believe all of us knew that if it were increased to the level of an O-ring burnthrough, what the consequences would be. And I don't think there's any question in anyone's mind about that.

I don't know if I've answered the question. I guess your original question was what I thought was in the thought process, but that was what was in my thoughts.

MR. ACHESON: Thank you very much.

CHAIRMAN ROGERS: Thank you very much.

Just one other question and I think we would like to adjourn for the day.

Did either of you gentlemen, Mr. Kilminster or Mr. Lund, have any pressures, outside pressures? Did anybody call you or anybody suggest that you should vote to launch? Is there anything of that kind that the Commission should know about?

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MR. KILMINSTER: No, sir.

CHAIRMAN ROGERS: Thank you very much.

We will adjourn for the day.

(Whereupon, at 4:25 p.m., the Commission recessed, to reconvene Wednesday, February 26, 1986.)